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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1861.

PART FIRST.

Analytical and Critical Reviews.

Review I.

Historia Bibliográfica de la Medicina Española. Obra póstuma de Don Antonio Hernandez Morejon, Médico de la Real Cámara, Primer Catedrático de Clinica en los Estudios de Madrid, &c. &c. Tomos VII.—*Madrid,* 1842–1852. 8vo.

Bibliographical History of Spanish Medicine. A Posthumous Work by Don Antonio Hernandez Morejon, &c. &c. Seven Volumes. —*Madrid,* 1842–52. 8vo.

(*Continued from our last.*)

Such were a few of the names in the palmy days of medical science and cultivation in Spain. Necessarily rapid and imperfect as has been our sketch, we may safely say, that in no other country at this period was a more adorned or a more powerful intelligence brought to bear upon the advancement of our art: we might even venture to say, that in none was this equalled. But the era was approaching when these glories were to become matter of history, and when men were to look mournfully back upon a Villalobos, a Laguna, a Daza, a Valles, and a Mercado, as upon a race of giants who had retired from the field to leave the world to its little men. In the seventeenth century, a few distinguished names still occasionally occur, while, for the common herd, the halo of their great predecessors seemed to be all the lustre that remained to them. Even the professorial chairs began to be occupied, for the most part, by men who could not arise beyond the most frivolous or unprofitable questions, discussed in a miserably petty spirit of partisanship, and in language far apart from the pure and manly Castile which they ought to have inherited, but which they attenuated and degraded to a level with their treatment of their favourite topics, investing these in a vicious and affected rhetoric. It was to the better period of the Spanish schools that Primerose referred, when, writing of the "doctores parum docti" sent forth by many universities, and of the causes of the negligent teaching prevalent amongst them, he alleges* "paucæque supersunt academiæ quas abusus ille non occupavit; Hispaniâ exceptâ, in quâ licentiatus et doctoratus sunt magni honoris, ut audio." It was in the time of their deterioration, that even regarding Valencia the adage arose: "Médicos de Valencia, haldas largas y poca ciencia;" which we may transfer into the analogous English doggerel of "Doctors of Valencia College, wide robes and little knowledge." Then also applied the satire of Suarez de Figueroa,† where he says that no one in Spain need be in want of a university, where he could follow the prescribed courses and take his degree, the Faculty pronouncing their verdict unanimously, and as a matter of course: "Accipiamus pecuniam, et mittamus asinum in patriam suam." To some of the more deserving, if not always the more distinguished names, still arising at intervals, it remains a satisfaction to us to devote a few observations; and we willingly commence with Juan de Villarreal, the subject as well as the merits of whose treatise,‡ relating to what we now recognise as diphtheria, may claim to be viewed with more than ordinary interest.

Villarreal was a native of Ubeda, and studied and graduated at the University of Alcala, where he is said to have held a professorial chair; and with this terminates all that the best Spanish authorities supply to us regarding his personal history. His treatise appears to have been composed in the year 1608, but was not committed to the press till 1611, when it was published at Alcala. We have been fortunate in having had an opportunity of perusing this interesting work, which constitutes a quarto volume of two hundred and forty pages; though we could almost have resigned ourselves to being satisfied with the lucid analysis of its contents presented as a manifest labour of love by Morejon, which, extending to upwards of twenty pages, and accompanied by copious extracts, is sufficient to enable the reader to form a close estimate of the style and quality of the performance. Villarreal describes the exudation in the disease, which he succeeded in recognising, as a dense, impacted matter, in the form of a parchment-like, partially elastic membrane, lining the throat and fauces, sometimes white, and sometimes verging towards a livid colour; and he claims the priority for the comprehensiveness and accuracy of his description of the nature, properties, and varying conditions§ of this morbid product. Thus, among the rest, he establishes clearly the important distinction, that the whitish or ash-coloured layer denoted no ulcer, but depended on a tenacious matter, thrown out, and adherent on the surface.‖ He maintains that the affection thus characterized, which he considers to be a form of angina, well designated, from its tendency to produce closure of the windpipe, by its

* Jacobus Primerosius: De vulgi erroribus in Medicina, lib. i. cap. 2.

† El Pasagero, p. 144.

‡ De signis, causis, essentia, prognostico, et curatione morbi suffocantis, libri ii. 4to. Alcala, 1611.

§ Op. cit., pp. 4, 34, 90.

‖ Op. cit., p. 103.

popular name of garrotillo, was then new to medical experience, and that the diseases described by Aretæus, Celsus, and others of the ancients, though nearly analogous, were not identical. He regarded it as a contagious disorder, but propagated only by immediate contagion. His account of the symptoms and of the pathognomonic signs is exceedingly graphic, showing throughout great delicacy of observation; and he evinces no mean discrimination of the value of the indications for prognosis, and of the import of the complications. Thus far, all seems the work of an accomplished master, well-ordered and well-reasoned, with its parts in just subordination, its essentials duly prominent, and its accessories in their proper degree of subservience. So regarded, we might imagine ourselves to be occupied with a dissertation of the present day, by one of those enlightened physicians who are sagacious enough not to confine their range too exclusively to the appreciation of physical signs and of their pathological basis, rather than with the production of a writer dating back more than two and a half centuries.

Under the consideration of the causation of the disease, however, Villarreal is naturally less in advance of the ruling speculations of his times; yet even these, though he might find a difficulty in setting them aside wholly, he introduces and employs with comparative moderation. But if we could have willingly seen here less in him of the false dialectics of those who perverted while they professed to follow Galen, we must admit that, even where most engaged with hypothetical subtleties, it is less the force of his reasoning than the defectiveness of his premises that we have the right to challenge. His account of the essence of the disease has somewhat less of the same taint, though it is still not altogether free from it; but nowhere, in the midst of all such conventionalities, do we miss wholly that true spirit of research, the results of which are ever gleaming through, like threads of gold in a meaner tissue. The deaths, he states, which were very numerous among the younger patients, generally took place from the fourth to the fourteenth day, while he had occasionally seen a lingering convalescence prolonged to beyond the hundredth day.* Epistaxis, his experience taught him to regard as a fatal symptom. He had remarked protracted debility of the legs and arms as a consequence of the disease in many; and he names one patient† who recovered, after having been paralytic in the extremities for about two months. In the treatment, he recommends a nourishing diet at the commencement, though he rarely saw advantage from wine; and he had a cautious recourse occasionally to venesection, with what he considered benefit. The use of emetics he seems to have regarded as prejudicial. Topically, he reprobated the application of caustics,‡ and employed either bland or aromatic gargles, injecting the latter, in the younger patients, with a syringe;§ and he was favourable to the use of blisters when applied between the shoulders. He anticipated profit from a more extended trial of mercurial inunction than he had known to be attempted. Upon the whole, the more closely the treatise of

* Op. cit., p. 150. † Ibid., p. 196. ‡ Ibid., p. 210. § Ibid., p. 230.

Villarreal is studied, the more readily, we are convinced, will its merits be admitted; and our recent observers in diphtheria may look to their laurels, if we are to weigh what they have accomplished, with their modern advantages, against the fruits of the clear and philosophical spirit of inquiry and observation displayed with reference to the disease at so remote a period, and by this almost unknown or forgotten physician. Of the writings of his Spanish contemporaries on the subject, we have only seen the dissertations by De Fontecha,* by Andres de Tamayo,† Herrera,‡ and Gil y de Pina,§ besides the short monograph by Mercado already referred to; but meritorious, in many respects, as these are, as well as generally superior to what we encounter contemporaneously in Italy, none of them can yet be held to rival the treatise of Villarreal in the full body of doctrine he has established regarding the disorder. That of so evidently able a writer and investigator we should only possess this single work, though he appears to have contemplated others, must be a matter of regret to us: that we possess no record of his career, even to the extent of the date of his birth and of his death, is to us a defect, but to his countrymen a discredit. Sure we are that in every country many a glittering reputation has been built, and many a rich reward gathered, on far less enlightened labours than those which, with regard to him, have been suffered to glide towards the verge of oblivion: if, indeed, they at any time stood far apart from it, for we have no evidence that his treatise ever passed beyond a first edition, notwithstanding the interest which then necessarily attached to its subject. We cannot venture to extend pity to such a man, so high in his endowments, if apparently so humble in his fortunes; but we may well pity the age that seems to have debarred him from the use of those large opportunities, and that conspicuous and ample sphere of duty, the chief reward of which would have been to its advantage and might have widened our inheritance.

But if we leave Villarreal, little satisfied with what has been evidently to him a negation of his just honours and competent sphere of action, it is only to turn to another proof of the wantonness of fortune, and to find in Herrera the object of a more direct ingratitude; for to him the sphere of action was opened, the results sought by him were to a large extent accomplished, but their reward, beyond the rich one dependent on a sense of duty, was withheld. Of Christóbal Perez de Herrera, Antonio speaks‖ as of a man full of piety, prudence, and learning. His claims to distinction were manifold: for he was eminent as a physician, a socio-political reformer, a soldier, and a poet. Born

* Disputationes medicæ super ea quæ Hippocrates, Galenus, Avicenas, necnon et alii Græci, Arabes, et Latini, de Anginarum naturis, speciebus, causis, et curationibus scripsere diversis in locis; et circa affectionem hisce temporibus vocatam Garrotillo. 4to. Alcala, 1611.

† Tratados breves de Algebra y Garrotillo. 12mo. Valencia, 1621.

‡ Tractatus de essentia, causis, notis, præsagio, curatione et precautione faucium et gutturis anginosorum ulcerum Morbi suffocantis, Garrotillo Hispané appellati. 4to. Matriti, 1615.

§ Tratado breve de la curacion del Garrotillo. 12mo. Zaragoça, 1636.

‖ Bib. Hisp. Nov., tom. i. p. 249.

at Salamanca, according to Morejon, in 1558, but probably somewhat earlier, his works nevertheless appeared for the most part in the prior portion of the seventeenth century, where therefore they fitly offer themselves to our consideration. He was a student and graduate of Alcalá, having been a pupil of Valles. One of his first public appointments was that of physician in chief of the Galleys of Spain; and towards the close of the century he was honoured with a place as one of the physicians of the King. We are reminded of the exploit of our late eminent surgeon, Mr. Guthrie, when he served with Lord Cochrane, while we read of some of the soldierly achievements of Herrera; in one of which, at Fayal, he received a musket-wound through the body that nearly proved fatal. In various campaigns and expeditions, the duty of the humane physician and of the fearless combatant seemed to be assumed by him alternately with equal ardour, and he was to be found usually, when not required for the sick, on foot or on horseback, either by the side of the General or cheering on the soldiery. He took, besides, seven flags from the enemy in maritime engagements;* two from the Turks, by himself leaping sword in hand into their galliots, two from the Dutch, two from English vessels, and one from a cruiser of Rochelle. When settled afterwards in Madrid, he acquired reputation as an able and successful practitioner; but his active intelligence turned itself besides towards many questions of social and political reform, and, among the foremost, towards the formation of a regulated system for the relief of the deserving poor, and for the restraint of mendicancy. He was, by his own strenuous efforts, the founder of a house of refuge for the indigent in the capital; gathering for its institution and maintenance a sum of 50,000 ducats, of which 16,000 seem to have been derived from his own private resources, while the remainder was procured by him through personal solicitations from door to door. After a long period of service, he asked Philip III. to permit him to retire upon a pension, or with some other compensation for his great charges in his various labours of philanthropy. An annuity of two hundred ducats was decreed to him, but it seems doubtful if it was ever paid: at all events, we have his own evidence that his other claims were left unsatisfied.

The first publication by Herrera, which appeared in 1595, related to the condition of pauperism in Spain, his benevolent aims in relation to which it was designed to promote. It contains much curious information with regard to the habits and practices of the contemporary Spanish mendicants, who are shown as a class to have been at least as savage, vicious, and unprincipled as those of that period elsewhere, of whom likewise we have preserved to us not a few remarkable pictures by other writers. Among Herrera's later works were a disquisition on the 'Diseases of Children' (1604), a 'Compendium of the Principles of Medicine' (1614), a 'Treatise on the Garrotillo,' already alluded to (1615), and his 'Proverbios Morales' (1618), which last work, in its third edition of 1733,† we have had a direct opportunity of examining.

* Morejon, tom. iv. p. 122.

† Proverbios Morales, y Consejos Christianos. 4to. Madrid, 1733.

At the end of this agreeable volume, we find a recapitulation of the different benevolent projects of Herrera, embodied in fourteen propositions. Part of the first is a recommendation that there should be wards for convalescents established in connexion with all the hospitals in the kingdom, in order that the patients should not be subjected to removal while still languid and debilitated, and therefore exposed to suffering from want, as well as prone to encounter relapse or even fatal injury. In 1649, an establishment of this kind, we should add, was actually founded in Madrid by Don Antonio Contreras; giving an example of a wise and kindly provision, for which philanthropists elsewhere are still too frequently striving unsuccessfully. It is less pertinent to our subject to mention here, that in his sixth proposition he introduces a plan for augmenting and manning the navy, for the advantage of the nation; because, he remarks, it is certain that the country that is master at sea will be master on land. In an epilogue, appended to the whole, he gives a summary of his services and his losses, showing that, in his zeal for the common welfare, he had expended time which he might have devoted gainfully to his own interests, and that he had published and widely distributed forty larger or smaller treatises, eight of which were medical; the whole at a direct cost to himself of 4000 ducats, besides his other and greater sacrifices. Yet on this occasion, his object seems more to plead for a furtherance of his schemes by the State, than for a requital to himself.* A maxim in his Compendium of the Theory of Physic shows well the tendencies of the Spanish school of medicine, in its higher views and aspects, in at least the earlier portion of his time, and his own participation in them. The physician is not, he says, to involve himself in the discussion of sophistical or metaphysical entities, or in perplexed syllogisms or subtleties, but is to occupy himself wholly with solid philosophy, and with medical theory and practice. It would have been well if advice so sagacious had not been soon afterwards forgotten almost wholly by his countrymen.

The introduction of the Cinchona, or Peruvian bark into practice, is a debt which, about this period, we owe to the Spanish physicians. It appears that it was first brought to Spain by Juan de Vega, physician to the Counts of Cinchon, from whom it derived its name; and that the first portions of it were sold in 1640, at what was then the great price of from a hundred to a hundred and twenty reals a pound. Originally its designation was simply the "cascara" (bark), or the "cascarilla" (little bark), a title which has since been transferred to the product of one or more varieties of Croton. At this time also, the use of tobacco was extending itself so largely as to attract attention towards the habit, in its sanitary aspects, on the part of the physicians of Spain; by some of whom it was assailed with as vigorous a counterblast as that which was directed against it by our British Solomon, while in others it found its willing defenders. Among those who attacked the practice with the most conspicuous zeal was Francisco de

* Op. cit., pp. 332, 341, 356.

Leiva y Aguilar, a learned and able physician of Cordova, whose somewhat remarkable work,* published in 1634, is now before us. The evils which he lays to the charge of the herb, whether used as snuff or in smoking, are weighty enough, and include shortening of life, depravation of intelligence, production of melancholy and madness, injury of the teeth and of the senses of sight and smell, induction of apoplexy, spitting of blood, diseases of the throat, cutaneous affections, especially of the face, and baldness, with lastly, promotion of immorality. Our old Burton, or a modern Pinel or Esquirol, might envy him his picture of the victim of melancholia,† which is as just in its traits as in antithesis of language it is keen and happy. The description elsewhere of the hypochondriac smoker‡ is not less graphic. His incidental notices of a variety of matters, such as the qualities, duties, and fortunes appertaining to physicians, with his remarks on a number of popular ideas and customs in relation to medicine and its practitioners, regular or irregular, are sagacious and interesting, and now and then are conveyed with a degree of humour, certainly not approaching to that of Cervantes, yet reminding us sometimes that both are countrymen. Morejon states, that it is said that this work had so great an influence, that the Council of Castile prohibited its circulation, owing to its effect upon the revenue. What the Spanish medical historian seems to doubt on the part of the discretion of the Council, we are inclined to doubt on the part of that of the people; just as we are inclined to question now, whether all the wise moderation with which a Brodie has recently opposed the habit among ourselves, added to the efforts of more inconsiderate declaimers, will effect much in diminishing the enormous revenue of five and a half millions sterling annually, which, independent of original cost, the nation now expends on this, perhaps the least defensible, though not the most pernicious and costly of its follies. Yet the time may come, when men, looking far back, and knowing the good they have inherited from us, will consider whether they are to view the statement of this monstrous expenditure as incredible; or whether they are to reserve it as their sharpest instrument of satire, when in the mood to lash the extravagance and irrationality of their predecessors. Certainly, at all events, the curious inquirer who, a thousand years hence, may desire to compute the value of money in the nineteenth century, will not find the record of such a fact of singular aid to him in arriving at an unperplexed conclusion from his researches.

Besides some attention to the extension of the system of the Universities, especially in the Spanish possessions in America, and the foundation of many hospitals, during the sixteenth and seventeenth centuries, an attempt of a different description was made for the advancement of medicine at the close of the latter century, in the establishment of a Royal Society of Medicine at Seville. This institution was at first strenuously opposed by the Universities, but pos-

* Desengaño contra el mal uso del Tabaco. 4to. Cordova, 1634.
† Op. cit., p. 186. ‡ Ibid., p. 266.

sibly with little foresight for themselves, and happily, in so far, without success: for a profession which possesses no central point of union is indeed at the mercy of every assailant; and, weak from the isolation of its members, must yield now to the domination of the State, and now to the usurpation of inferior authorities, or must suffer helplessly its dignity and interests to sink under pressure at the caprice of any section of the community that may choose to bear upon it rudely. From this society, which was afterwards favoured with considerable endowments by the Crown, have issued, up to comparatively recent times, eleven volumes of 'Transactions,' not a few of which, unfortunately, denote too plainly that the institution had entered too late upon its career of labour, and was unable to check, because it had not been established sufficiently early to anticipate, the serious degeneracy to which, beginning with the teaching of the Universities, the science of medicine in Spain had become subjected. During the seventeenth century, Spain continued to suffer under frequently recurring epidemics of great severity; among which the true plague and the petechial typhus stood conspicuous for their ravages, and became, like the garrotillo, the subject of many writings and of much controversy. But leaving these topics, meanwhile, we revert to a notice of a few others from among the more remarkable physicians and medical authors of this period. Among these we encounter the name of Gerónimo Gomez de Huérta, whose reputation as a translator and commentator on Pliny, and even as a poet, entitles him to mention. Juan de Fontecha, in the dissertation on the varieties of angina, already referred to, points out the advantages of emetics in such disorders; and, in extreme cases, sanctions a resort to tracheotomy, thus following several of the ancient physicians, with whose writings he was evidently well acquainted. Pedro Garcia Carrero, an able Galenist and something more, distinguished himself as a medical teacher at Alcalá. But he was modest, as we learn from others; and he was poor, as he himself tells us, because he desired to depend solely upon his knowledge of his art for his subsistence, with no other consideration than to exercise it as he ought.* And truly, in reflecting on such destinies, it sometimes almost seems to us that, even where merit thrives it is owing to some accidental adjuvant beyond itself, and that it is not to their best qualities that even our best men owe their success; while it is manifestly so much the interest, in a multitude of positions, for mediocrity to keep down unaided talent and knowledge, that one rather wonders that they ever rise. Let us hope of such men as Garcia Carrero, that possibly they sometimes solaced themselves with the idea, though half suspecting its vanity, that some one, after they had passed from the scene, might draw their labours from obscurity, own the true basis on which they were founded, and give that praise which, though too late for them, might still impart pleasure to some survivor who loved them. Alonso de Freylas, in an important work on the Plague, published in 1606, to which we have

* Morejon, tom. iv. p. 207.

referred with satisfaction,* reasons well against the establishment of extensive hospitals for the disorder, and especially against the introduction into these of the patients by compulsion.

The History of the Animals used in Medicine, by Francisco Velez de Arciniega, published in 1613,† is a work of considerable but indiscriminate learning, in every page of which we meet proofs of the unstinted credulity of the writer, who requires, besides, but a remote affinity with his subject to introduce the wildest of travellers' fables or supposed miracles. Of these, the tradition regarding the head of an infidel judge that retained the faculty of speech for many years after death, and so was enabled at last to crave and obtain baptism from an Archbishop of Milan, occurs to us as scarcely the most extravagant example. Alfonso Nunez, a graduate of Salamanca, wrote ably on the Pulse (1606), and on Garrotillo (1615). We have a sound proof of his candour and sagacity in the latter treatise,‡ when he tells us, in contrast to a physician of his time, who pretended to have a secret for the cure of the disease in question, that he had no secrets, and did not fear to reveal any of his remedies, because these do not yield benefit by any proper or specific quality of their own, but by the judgment and skill with which they are applied; so that, if it be stated with Galen that medicines are nothing in themselves, it is with justice, for they are nothing unless they be rightly proportioned and directed by him who uses them. We have in Simon Ramos, a physician of Seville, an example of a different grade; and we could not better illustrate than from some of his writings the unprofitable nature of the singular and mystical, if not profane questions, which were now held fitting with many to occupy the attention of the so-called medical inquirer. In a dissertation, published in 1630, Ramos considers it worthy of a physiologist to seek to determine whether the human frame, while in a state of innocence in Paradise, should be regarded as having been exempt from all the fouler excretions: whether the most holy Virgin, in the conception of our Saviour, contributed actively or passively, and might be called the mother-father (matripater) of Christ, her son: and whether Christ and the Virgin Mary could, by the sole powers of nature, be freed from the grosser excrements. Another similar work, which Morejon himself cites as the most remarkable specimen of the medico-theological raving of the seventeenth century, is that by Vicente Moles y Garcia, entitled 'Philosophia Naturalis Sacrosancti Corporis Jesu Cristi,' published in 1631, the questions discussed in which are equally odious and irrational.

Having ourselves seen nothing of any of the writings of Melchor de Villena, we merely record the assurance of Morejon regarding him, that he was styled in his day the new Hippocrates, and that his great reputation led to his being consulted from all quarters, and even from widely distant countries, a renown of which we trust he was more worthy than of the all but oblivion that now covers him. In a work

* Conocimiento, curacion, y preservacion de la Peste. 4to. Jaen, 1606.

† Historia de los Animales mas recibidos en el uso de Medicina. 4to. Madrid, 1613.

‡ Morejon, tom. iv. p. 257.

by Pedro Lopez de Leon, a native of Seville, and a pupil of Hidalgo de Aguero, which was first published in 1628, but of which we have only seen the second edition of 1685,* we meet with details showing the intelligent and dexterous surgeon, though with proofs, also, of what was then frequently the helplessness of the art. Among his successes he mentions the case of a woman from whom he had removed a great part of the uterus, who bore a child ten years afterwards, and who at the end of other ten years was still alive.† He was accustomed, he states, in traumatic hæmorrhage to proceed by passing a needle below the vessel, and by this sole puncture, in performing which it is evident from the context that he intended the needle to remain ("*quedanse alli las agujas*"), he found that the flow of blood was usually arrested, even in vessels of great magnitude. "When other methods do not succeed," he continues,‡ "this puncture is to be made in the depth of the wound, and through its means you may avoid what is directed by authors in wounds of veins or in injured arteries, who order the parts to be laid open and the vessel to be tied above and below, dividing it afterwards between the ligatures." Lopez was an enthusiastic admirer of his master, Hidalgo, and in this he was followed by another surgeon, Pedro Gago Vadilla, whose work, probably first published shortly after 1630, we have examined in its third edition,§ which is that also which has been seen by Morejon. Vadilla eulogizes Hidalgo's method in the treatment of wounds as more like a revelation from heaven than the work of man;‖ and certainly his experience in Peru, of which he relates many remarkable illustrations, abounds as singularly in proofs of how freely his fellow-denizens in Lima¶ used the stiletto and the knife, as of the happy success with which he applied the rules of his preceptor to remedy the effects of their savagery. Another writer near this time, whose treatise on Injuries of the Head we have consulted, is Christoval de Montemayor, who was one of the surgeons to both Philip II. and his successor. This work appears to have been first published in 1613, and Morejon mentions another edition in 1664, without apparently being aware of an intervening one in 1651,** which is that now before us. The author, who treats his topic generally with judgment, appears, however, to have been more liberal with the trephine than would have been sanctioned by the surgical reformer of Seville. A work on Surgery, of this period, which would scarcely have merited notice were it not that it had passed through several impressions, and has chanced to fall into our hands, is the epitome of the art by Alonso Romano.†† Appended to it is a treatise on Strictures of the Urethra, by Miguel de Leriza, who adds to his title of Surgeon that of Official of the Holy Inquisition.

* Pratica y Teorica de las Apostemas en general, y Praticas de Cirugia. Folio. Calatavid, 1685.

† Op. cit., lib. ii. cap. 2, p. 154. ‡ Ibid., lib. ii. cap. 3, p. 160.

§ Luz de la verdadera Cirugia. 4to. Pamplona, 1692.

‖ Op. cit., p. 38. ¶ Ibid., p. 285.

** Medicina y Cirugia de vulneribus Capitis. 12mo. Saragoça, 1651.

†† Recopilacion de toda la theorica y pratica de Cirugia. 12mo. Valencia, 1665.

The phlebotomists of this period are instructed in their art by Alonso Muñoz,* blood-letter and first barber to the king, who, we find, in speaking of extraction of the teeth, is inclined to dispute the ordinary notion that the adult possesses thirty-two teeth, twenty-eight being all that he had been able to discover. Two works on Veterinary Medicine which we have examined, the one published in 1629 by Baltazar Francisco Ramirez,† and the other in 1685 by Pedro Garcia Conde,‡ afford some example of the way in which this department of the healing art was cultivated in Spain during the seventeenth century.

Juan Gutierrez de Godoy, a physician of the Court, is favourably known for a work on the duty of mothers to nurse their children;§ an obligation then lamentably neglected, and often in a way singularly repulsive to modern notions. This treatise, which we have been able to consult, was published in 1629. Our acquaintance with it entitles us to give high credit to the humanity of the writer, whose reach of thought, however, as shown in his numerous digressions, by no means generally passes beyond that of his contemporaries. We ought, perhaps, not to omit the name of Gallego de la Serna, who passed into France as physician of Anne of Austria, daughter of Philip III., were it only to record the remuneration he received for his shrewdness in resisting the mischievous interference of the other physicians, during an illness of the Queen which they had pronounced desperate, but which he declared to be progressing towards recovery, and to require only to be kept from all powerfully disturbing remedies. Proving right in his prognostication, his reward, he tells us, was two thousand florins from the King, with the same amount from the Queen, and a life annuity of eight hundred florins besides, which he continued to enjoy. Gaspar Brabo de Sobremonte Ramirez, who died in 1683, at the age of upwards of seventy, was a graduate of Valladolid, where he afterwards held a chair. He was an indefatigable, and, in his day, highly esteemed writer; his collected works having appeared in five volumes folio. Of his separate publications, several of which went through numerous editions, we are directly acquainted only with his 'Disputatio Apologetica,'‖ which is a defence of dogmatic or rational medicine, first published in 1669; and which has appended to it two other treatises, the one discussing the details of a series of medical cases and consultations, and the other presenting an epitome of what he terms practical medicine, but more truly of the principles of medicine, designed for the use of the student. Sobremonte was body physician to the two kings, Philip IV. and Charles II.; of the last illness of the former of whom, oc-

* Instruccion de los Barberos Flobotomianos. 12mo. Valencia, 1621.

† Discurso de Albeyteria. 4to. Madrid, 1629.

‡ Verdadera Albeyteria. Folio. Madrid, 1685.

§ Tres discursos para provar que estan obligadas a criar sus hijos a sus pechos todas las madres. 4to. Jaen, 1629.

‖ Disputatio Apologetica. Pro dogmaticæ medicinæ præstantia; et omnium scientiarum, et artium dignitate, ex omnigenæ literaturæ decretis. 4to. Coloniæ, 1671.

curring in 1665, he gives an interesting record.* His other works chiefly related to physiology, and to the consideration of numerous practical and theoretical medical questions; to the elucidation of which, if we are to judge from what we have ourselves perused, he brought great stores of that peculiar learning, as well as singular skill in that intricate apparatus of dialectics, which appeared so attractive then, but which seem to us so vague and unprofitable now. In the volume before us, many points in medical ethics and polity, however, are treated in a way to excite and reward curiosity.

A name of greater general distinction in medicine than that of the, in so far, deservedly fortunate Gallego de la Serna, or even than Sobremonte, was that of Gaspar Caldera de Heredia, a graduate of Salamanca, and resident up to the time of his death as a physician in Seville. He was the contemporary and personal intimate of the learned bibliographer Antonio, who assigns this as a reason, along with the desire not to do violence to his friend's modesty, for desisting from extending to him what would have been otherwise his due measure of praise.† His principal work, to the merit of which we can speak from a former direct acquaintance, now renewed for our present purpose, is his 'Tribunal Medicum, Magicum, et Politicum,' published in folio at Leyden in 1658. We may single out, as worthy of notice in the first part of this remarkable performance, the discussions on prognosis, the comments on tracheotomy, those on tubercle of the lung, the treatise on the plague, and that, "utilis et jucundus," on the variety of drinks, where we find, among the rest, an account of a tippling-house then open night and day in Madrid,‡ known, from the variety of its potations, as "the house of the hundred wines" (*la casa de los cien vinos*). In the second part, or the 'Tribunal Medico-Magicum,' we find a contrast to the many excellences of the portion that precedes, in the too frequent surrender of the author to a belief in the grossest follies and superstitions of his age. In the 'Tribunal Politicum' the author again rises in our estimation: but the volume unfortunately closes with a new tribute to fanaticism, in the shape of a discussion of the question whether the Kings of Castile, by hereditary descent, are endowed with the power to cure the possessed and to cast out devils; which knotty point, after a careful investigation of all the probabilities, and a demand for further proofs, he considerately reserves as a topic for his greater leisure, without hazarding an absolute denial, as is alleged in his favour by Morejon. Caldera was born towards the close of the sixteenth century, and survived to a ripe age. A Benedictine monk, Fr. Estevan de Villa, who officiated as apothecary to the Hospital of St. John, at Burgos, distinguished himself about this time as a pharmaceutist. A perusal of his treatise on Medicinal Plants,§ with the annexed formulary, leads us to assign to it a creditable place among the similar works of this era. A taxation of the prices of drugs and medicaments in Spain, issued by Royal authority in

* Op. cit., p. 240. † Bibl. Hisp. Nova., tom. i. p 520.
‡ Tribunal Medicum, p. 465.
§ Ramillete de Plantas. 4to. Burgos, 1646.

1680, happens to be bound up with the copy which we have examined, to which it constitutes an interesting, though not strictly a contemporaneous appendage, full of curious and suggestive particulars. We have also seen, by this writer, the lives of the so-called twelve princes of medicine, a series of concise biographies which appeared at Burgos in 1647.

We leave to Miguel Vilar, styled in his day "the second Esculapius in his profession, the first Apollo in learning, the honour of Valencia his country, and the admiration of strangers," and who was for long a professor of anatomy and dean of faculty in his university, all the renown which belongs to him from this announcement. A more substantially acquired reputation was that of Pedro Miguel de Heredia, a native and graduate of Alcalá, whose works, published posthumously in 1665, may still be consulted with interest. Another graduate of Alcalá, who enjoyed great reputation from his writings, was Juan de la Torre y Balcarçel, a native of Hillin, in Murcia. His principal treatise, the first edition of which is now lying before us, was his 'Compendium of the Theory and Practice of Medicine,'* of which at least two other editions appeared afterwards. His remarks in this work on medical ethics,† whether as relating to the physician or the patient, deserve attention, partly in themselves, but still more as illustrative of the conditions of the period. He mentions the case of a nun at Ubeda, who, suffering from prolapsus uteri, was expelled from the convent and forced to wear the dress of a man;‡ and of another nun in Cadiz, similarly afflicted, from whom Sebastian de Antequera, a surgeon, removed the uterus and its ligaments with success,§ the patient recovering in twenty days. We have examined a treatise by Juan Nieto de Valcarcel, a native of Cordova, descriptive of a febrile epidemic which occurred in 1684,‖ and have been much gratified by its prevalent good sense, and by the clearness and liveliness of its style. We have also consulted a treatise by Juan de Cabriada,¶ the object of which is to claim regard for the then more recent advances in medicine, in comparison with what had been received from the ancients, though not with the design of depreciating the latter, a task which he has executed with an independence of judgment not common in his time.

If, in the greater part of the seventeenth century, it is only rarely that we encounter any name of real merit to cheer us amid the general gloom of a science stooping feebly to its degradation beneath the puerilities of a baseless and aimless sophistry, it is still to find, towards the close of that century, and in the commencement of that which followed, that the deterioration had changed only a few of its features, while throwing off none of its essential imperfections. We gladly escape in part, though by no

* Espejo de la Philosophia, y Compendio de toda la Medicina Theorica y Pratica. Folio. Amberes, 1668.

† Op. cit., p. 184. ‡ Ibid., p. 72. § Ibid., p. 76.

‖ Disputa Epidemica. 4to. Valencia, 1685.

¶ Carta Filosofica, Medico-Chemica, en que se demuestra, que de los tiempos, y experiencias se han aprendido los majores remedios contra las Enfermedades. 4to. Madrid, 1686.

means totally, from the superstitious dedications with which many treatises open in the former division of the period, and from the abject, though not needless deprecation of the censure and vengeance of the Inquisition with which they frequently close; but it is not, nevertheless, to encounter immediately greater manliness and justness of thought in the times which we approach. There was the still near reflection of a real glory at the one era, which survived only as a recollection in the other; though men, pointing to their ancient fame, continued to vaunt as an adornment what they had no longer energy enough to follow as an example. Thus the Spaniard spoke then, in the midst of his political and mental enthralment, as he speaks too habitually even now, when we trust he has better prospects before him, with that tetchy arrogance natural to those who, the more they feel insecure in their claims, are ever the more jealous in exacting an acknowledgment of them. When the individuals of a nation complacently praise themselves and each other, and boast the old perfection of the institutions with which they are connected, or in which they have an interest, while they are weakly impatient of all challenge from without, certain it is that such a nation is conscious of the hollowness of its pretensions, and that such institutions are tottering to their decline; for in matters of science, dependent as that is on extension and progress, even to be content is to recede. In the beginning of the eighteenth century, there were still truly many Spanish medical writers, but their trivialities constituted no true medical literature. It was towards the middle of this century that a sounder spirit began to insinuate itself into the medical science of the Peninsula, characterized by the reappearance of a greater activity of observation, and more just and moderate habits of reflection. Not that the progress of the Spaniards was then in any degree equal to that of the more advanced of other European countries; for the burden, rather than the incitement, of their former renown, rendering them prone to rest on their old memories, made them proportionately slow to accept new doctrines and discoveries. Though this impassiveness may have saved them from a few fluctuations in opinion, and a few caprices in practice, still the want of movement by which it was accompanied was generally little favourable to the growth of knowledge; for it is by the collision of opinions that we ultimately elicit the truth, however prejudicial it may often be to receive at first, too confidently and rashly, doctrines which a riper examination may expose as chimeras, or however hurtful be that innovation which, merely as innovation, leads so many into error, to throw them back into scepticism. No new code of theoretical medicine, justly so designated, arose at this period in Spain; and at no portion of it did any of those bodies of doctrine, emanating from the schools of Leyden, Edinburgh, Montpelier, and Vienna, which had so deep an influence elsewhere, hold an extensive sway throughout the Peninsula, though each of them had successively its partisans. But with those among whom real science lies thus in lethargy, there is always the greater aptitude for the growth and the fostering of pseudo-science.

There arose thus in Spain sharp enough controversies on different devices or methods of practice, and above all, on what was accredited as the water-cure of these days, and on the vexed question of the remedial value of bloodletting. Both of these might have assuredly been legitimate questions in medicine, had they not been converted into absorbing questions; and there is nothing singular in the fact—for it is the usual fate of such topics—that both of them have now been revived amongst ourselves, just as both had, at still remoter periods, risen before into undue notice, and again had ceased to engross attention. No reasonable physician had ever been averse, at any era, to attribute to water, at different temperatures, and under different modes of application, whether external or internal, the most valuable therapeutic qualities; but in conceding this, it was while maintaining for it its due place with relation to other remedies. The world, however, loves men with the simplicity of single ideas, and is ever anxiously longing for the accession of some sole dogma which is to embrace all knowledge and to rule all practice. Thus when, about the years 1708 and 1710, there circulated throughout southern Spain a pamphlet recommending water as a universal remedy, only requiring to be drunk the more largely the more pressing the malady, many minds were eagerly ready to accord their confidence to the new panacea. Already, in 1715, when the first part of Gil Blas was published, Le Sage alludes to the prevailing heresy; uniting it somewhat inconsistently with the then also prevalent, and equally heterodox tendency to the abuse of bloodletting. When Doctor Sangrado admonishes Blas regarding the error of other physicians in building-up their knowledge upon a thousand difficult studies, it is to assure him, that with the single maxim of prescribing venesection and warm water, he may spare himself the trouble of mastering the host of natural sciences, including pharmacy, botany, and anatomy, and so shorten what would otherwise be a very long road. True to the design of him who tells us that his desire was to represent "la vie des hommes telle qu'elle est," the satire of Le Sage has all its force still; and we would still willingly leave such questions to be dealt with by such hands and with such weapons, could we only secure them within our reach. It was perhaps unfortunate, therefore, that in 1717, Navarrete, an eminent professor at Granada, more memorable for his services in having propounded a plan for a general medical topography of Spain, such as we would gladly see attempted for Great Britain, assailed the pretended system,* with the view of undeceiving the public: for, as it has rarely been a suc-

* We translate from Navarrete the following notice of the method, as we find it quoted by Morejon :—"As soon as the doctor is called, and resolves to adopt this treatment, he commences by giving the patient two or more pints of cold water before breakfast, the same at midday, and a like quantity in the evening, with freedom to add to this at pleasure during the day; or the rule is made more rigid for several days, during which the patient, under deprivation of food, is to drink every two or three hours, with the warning that what he takes before going to sleep is the most beneficial. In the subsequent days, the quantity of water is gradually augmented. If the patient lose appetite, he is told that it is a favourable sign; they go on diminishing the quantity of food, or allow only broth, giving abundance of cold water before and after it. Should diarrhœa occur, although a total relaxation be manifest, they still say that it is good; and even when the

cessful plan to address reason to those who have begun by showing themselves unreasonable, so controversy on such matters for the most part only extends notoriety and provokes adherence. It is thus wiser to leave the deluded in possession of their bauble till they themselves weary of it, while prudent men pursue their onward course, taught by the experience that the child in knowledge, as in years, clings the more desperately to a toy that is attempted to be snatched from it, yet shortly afterwards, of its own accord, will toss it away with contempt. Thus, as the new cure found opponents, it found defenders; till at last the flimsy system, after ruling so widely that in many places drugs are said to have become unsaleable, was luckily shaken by feuds among its own proselytes, who themselves first brought it into ridicule, and then consigned it to neglect. Still, if we were asked to point now to the best, or the most plausible exposition of the virtues of water, regarded as a universal remedy, we would say that this has been the most ingeniously accomplished, judging from the ample quotations in Morejon, not by any of our recent hydropathists, but by a Spanish expositor of more than a century back. We refer to Jose Ignacio Carvallo Nunez de Castro, the length of whose name, nevertheless, we suspect largely exceeds that of the duration of his renown generally; if he, and not Vicente Ferrer, were really the author of the work in question.

Passing over some intervening remarks on the subject of inoculation, for the promulgation of which Morejon, on no very satisfactory grounds, claims a priority on the part of Spain, but the notice of which leads him, with more justice, to offer a grateful admiration to our Jenner for his later and better discovery, we come to the controversy on the subject of bloodletting. It was in developing his views of an expectant system of treatment, in which the powers of nature were to be chiefly considered and respected, that Miguel Marcelino Boix y Moliner, writing in 1711, arrived at the conclusion that bloodletting was not only an improper interference in the generality of diseases, but, anticipating the notions of some of our modern controversialists, declared further, that even in pneumonia and pleuritis its effect was to disturb the natural process of cure, and that, therefore, its use was everywhere inappropriate and contrary to sound rules of practice. In this he advanced beyond the opinions propounded by Bernard Caxanes, in the sixteenth century, whose aim was to restrict

stomach shows signs of complete debility and enervation, it is good to drink on. If great weakness of the pulse, or nervous depression, sleeplessness, and atony of the natural, vital, and animal actions supervene, they say that it is good, and the patient must drink more. If the liver, or the spleen, or the abdomen, become tumid, or the limbs, with the whole body, begin to swell, they say that it is good, and all that is wanting to dispel the tumefaction is to drink on. If cold sweats and faintness present themselves, it is still good, and he must drink. And lastly, if he die (as has happened to many), they say it is because he did not drink more, or because, finding himself in the last extremity, he did not choose to drink during the agonies of death. And even when they see produced the opposite effects to those they desire, and instead of the head being refreshed, there is loss of sleep, instead of water being dispelled, there is retention of urine, it is a rare event that they do not insist, and insist, and, even when warned, continue to insist."—Tom. vi. p. 368. The picture might be paralleled at this day.

the use of detraction of blood in a class of cases where its employment might really be generally admitted to be of more than doubtful advantage. The contest, proceeding with variations to greater or less extremes, had never wholly ceased up to the time when the intervention of Boix imparted to it bolder features and increased animation; and he was soon followed by other disputants, some arguing for, and some against, the recourse to venesection in all acute diseases. Had the argument merely been that bloodletting was a remedy capable of being abused, and, like all other powerful remedies, the more capable of effecting mischief, when misdirected, that it was energetic for good when it was directed wisely, certainly in the times of Boix it was not unneeded. Nieto de Valcarcel, in the work to which we have already made reference, tells us, for example, that during a considerable part of the interval between his fifteenth and twentieth years, he had himself, as a precautionary measure, been bled regularly every two months, with the loss of colour and of strength that was to be expected from so extraordinary a discipline. We may wonder that he was able to add that he felt himself healthy after this, but weak,* and that he recovered his vigour rapidly when he broke through the custom. In most acute diseases, while the application of the rule was then proportionately prompt, it was equally merciless. Olmedilla mentions that it was a matter of boast with many, at the close of the seventeenth century, to have repeated bloodletting fourteen times, even in cases of petechial typhus.† But we need no longer to war against such flagrant excesses, and must take heed rather that we do not reduce the legitimate resources of our art, through the common tendency of rushing into an opposite extreme. To allure us into this, it will be seen that we have here, at least, had no enticement in the charm of novelty.

Spain possesses great resources of wealth and advantage in its mineral waters, which occur throughout the country in rich abundance, and with every variety of constituent. Even so early as 1498, attention was directed to certain of these by Juan Gutierrez, to whose writings we have already referred; and the example of this writer was afterwards followed by many others, the subject gradually rising in importance as the advancing knowledge of chemistry gave amplitude and precision to the necessary inquiries. A general treatise on the subject, under the title of a universal history of the mineral waters of Spain, was commenced to be published by Bedoya y Paredes in 1764, and was received with great approbation. Of this work, which was planned on so large a scale as to be designed to extend to six quarto volumes, embracing an account of fifteen hundred medicinal springs, two volumes only have been completed, the waters described in which exceed two hundred in number. This work, again, has been succeeded by various treatises, either relating to individual springs or to groups occurring in individual districts, or aspiring to include the whole medical hydrology of the kingdom. In 1817, thirty-one of the watering-places of principal resort were placed, in imitation of what

* "Sano me sentia; pero debil."—Op. cit., p. 112.

† Morejon, tom. vi. p. 49.

occurs in other continental countries, under the control of a government department or commission, which still continues its functions, having since extended them over a wider range. Physicians are appointed to superintend the different establishments; a charge, the emoluments of which, though small according to our notions, are sufficient to render the post a coveted one. Among the more recent writers on this important subject, the bearings of which in Spain deserve to be more generally known in this country, we may mention Gonzalez y Crespo, Sanchez de la Matas, Rodriguez Solano, Bantista Foix, Raimund de Monasterio, and Pedro Maria Rubio, as having presented more or less comprehensive, but always valuable details. Another Government measure was the establishment, in 1748, of a Medico-Chirurgical College at Cadiz, destined for the instruction of practitioners for the naval service, upon which was bestowed afterwards by the Crown the right of granting to candidates the degrees of Bachelor of Philosophy and Surgery, along with the Licentiateship, with the same prerogatives as if these had been conferred by the Universities. This system was next extended to the education of all the other surgeons of the kingdom, and was provided for, in the face of much opposition, by the erection finally of seven colleges of surgery, located in different cities. The provision thus designed for the promotion of the cultivation of surgical art, for which an imperious necessity had arisen through the neglect with which its study was treated by the Spanish universities, and through the state of depression into which it had lapsed throughout the country, had soon its manifestly beneficial results in a gradually improving condition of this important branch of practice.

Let us now proceed to a brief notice of some of the principal writers, whose labours illustrated what may be termed this period of comparative revival in Spanish medicine. Martin Martinez, born at Madrid in 1684, and a pupil of the school of Alcalá, was a voluminous writer on a variety of subjects in relation to medicine, and contributed much towards the infusion of a better spirit of inquiry into the medical science of his time. Contemporary with him, another physician, Francisco Solano de Luque, attracted much attention for a while, though rather in foreign countries than in his own, by the promulgation of his ideas on the means of prognosticating a crisis in disease through the medium of the pulse. The views of Solano, originally published in Spain in 1731, in a treatise the sounder portions of which struggled dimly through a haze of pseudo-philosophical disquisition, were introduced to the notice of the English reader by Dr. James Nihell, in a work* which is now before us. Thus, Solano taught that the dicrotic pulse was a sure indication of an approaching epistaxis; that the intermitting pulse foretokened a critical diarrhœa; and that an irregular pulse, constituted by the recurrence of a gradually strengthening succession of strokes, denoted the access of the critical

* New and Extraordinary Observations concerning the Prediction of various Crises by the Pulse. Second edition. 8vo. London, 1750.

sweat.* The estimation of the value of such indications naturally fell, along with the faith in the doctrine of crises with which they were associated. Still, we cannot but recognise in some of the particulars, and especially in the dicrotic pulse, a relation to certain of those conditions under which epistaxis is prone to occur, though not infallibly occurring; while beyond this it would be imprudent to proceed, on this score, in so trying a matter as a prognosis, notwithstanding the inordinate recommendations of Morejon, with whom the rules of Solano seem still to be held as deserving of the most implicit confidence. Two Portuguese surgeons published works at this period, the one of which, by Jozé Ferreira,† is mentioned, but had not been seen, by Morejon: the other, by Feliciano de Almeyda,‡ seems to have wholly escaped his attention, which, however, appears to have been only incidentally directed towards the writers of Portugal.§ The work of Ferreira was probably intended to be completed in a second volume, for the portion published by no means embraces the whole scope of surgery. Its doctrines are professedly founded on those of Stahl, and it carries with it the common appendage of a testimony from the Holy Office that it contains nothing opposed to sound faith or to good morals. Our examination of this treatise leads us to think more highly of the judgment of the author than of that of Almeyda, who seems to have had more than his share of the worst faults of the age that was now receding before a clearer, though still an imperfect dawn. As an example of the character of the work, we may mention that the author has profited by the fact, not always sufficiently adverted to in estimating the value of a therapeutic agent in our own day, that erysipelas, under ordinary circumstances, completes its course in a determinate period, to adopt the imagination that he had made the discovery of an effectual plan of cure. This consisted in the external application of the blood of a dog, or cock; and he is, of course, able to offer a variety of proofs of the efficacy of his remedy, for to such minds, luckily or unluckily, there is evidence to be found in the world for everything, on the mere condition that they do not seek it too fastidiously. We cannot fail to admit the powers of nature, at all events, when he tells us that, being called to Antonio Ioaõ, whom he found affected with what he calls malignant erysipelas of the face,|| he bled him six times, and then repeatedly smeared the whole of the erysipelatous surface with cock's blood, with the result that the patient was well in fifteen days.

To return to Spain; we have, in the System of the Practice of

* Op. cit., pp. 1, 5, 7.

† Cirurgia Medico-Pharmaceutica deduzida de doutrina Stahliana. 4to. Lisboa, 1740.

‡ Cirurgia reformada, dividida em dous tomos. Folio. Lisboa, 1738.

§ The history of medicine in Portugal, like that in Great Britain, remains to be written. We are glad to see, however, from the Portuguese journal, the Gazeta Medica de Lisboa, that the Medical Society of Lisbon have recently unanimously agreed to promote this national department of inquiry, and to publish the results in their journal. We have also remarked with pleasure, in the Gazeta Medica do Porto, the exertions of Dr. Gouvea Osorio, of Oporto, towards the same object.—Gazeta Medica do Porto, 1860, pp. 194, 289.

|| Op. cit., p. 428.

Physic, by Francisco Sanz de Dios y Guadalupe, a graduate of Salamanca, a tolerable compendium of the medical knowledge of the time, drawn up with greater attention to foreign authorities, and among them to our Willis under the guise of Uvilis, than is usual with the Spanish writers of the period. If the first edition of this work, as appears from Morejon, was published in 1730, it must have passed rapidly into favour; for that which we have examined is the third,* and dates in 1739. An important document, in relation to the existing state of medicine in Spain, our inspection of which has not exalted our notions regarding it, was the Pharmacopœia of Madrid, first issued by State authority in 1739,† in the form of a quarto volume of four hundred and eighty pages. A more remarkable specimen of polypharmacy than it contains, in the shape of the Aqua Polychresta, will not easily be discovered anywhere; while the retention of such articles as the cranium humanum, bufo exsiccata, album græcum, secundina mulieris, and a host of the like, shows, at all events, no advance beyond the lowest of the then prevailing conditions of this department of medicine. We need make no especial reference to the considerable, though misapplied, ingenuity of Miguel Rodriguez, a physician of the mechanical school, in his work entitled 'Medicina Palpable,' published in 1743; nor to the 'Medicina Experimentada' of Ignacio Catalàn, which appeared in 1745; nor to the treatise on the Plague, by Juan Diaz Salgado, published in 1756; nor give more than a passing tribute to the learning displayed in the Physico-Medical Dissertations by Ramon Brunet de la Selva, published in 1755 and 1758; nor dwell upon the insignificant work by Vicente de Lardizabal on the Diseases of Seamen, which appeared in 1769; nor on the more deserving 'Medicina Hippocratica' of Francisco Rubio, of which we have consulted the second edition, dating in 1774; nor on the History of Contagions by Antonio Perez de Escobar, published in 1776, which is not, however, without its points of interest; nor on the several treatises on Small-pox, Typhus Fever, and Pleurisy, by Joseph Amar, published between 1774 and 1777; nor on the dissertation on the virtues of the American Agave and the Begonia in Syphilis and Scrofula, by Francisco Xavier Bálmis, which, when published in 1794, excited some attention and controversy; nor, lastly, on the treatise on the Diseases of the Mouth, which is in fact a work on dentistry, the production in 1795 of Francisco Antonio Pelaez; although, with certainly greater labour than profit, we have examined the whole of these for the purposes of this sketch. It is, in truth, first in the works of Andres Piquer that we encounter any very prominent proofs of the better spirit which infused itself into Spanish medicine during the eighteenth century. Nor did this better spirit, with the sole exception of its display in the writings of that author himself, actually rise, during the remainder of the century, anywhere beyond an even mediocrity; only praiseworthy because better

* Medicina Practica de Guadalupe. Folio. Madrid, 1739.

† Pharmacopœia Matritensis, Regii, ac supremi Hispaniarum Protomedicatus auctoritate, nunc primum elaborata. 4to. Matriti, 1739.

than what had gone before, but too insignificant, with the majority, to win a place for them in European fame, though doubtless there were those, within the narrower limits of Spain, who attained often that fleeting notoriety which men miscall fame.

Andres Piquer was born in 1711, at Fernoles, in Aragon, and studied philosophy and medicine in Valencia, where he graduated in 1734. Confessedly, however, he profited little by the instruction which he received from his *alma mater;* for what was then taught there rested upon the jealously maintained, or even revived, traditions of the Middle Ages, futile as to philosophy, and as to medicine, illusory and clinging to arbitrary fictions and fallacies. He became, therefore, like many other great men, his own preceptor, struck out his own course, and earned, as the customary reward of such independence, the opposition of his immediate colleagues, who assailed, with the keenness of a personal animosity, the merits both of his doctrines and his practice. In 1742, however, he received the appointment of the Professorship of Anatomy in the school of Valencia, and in 1751 had conferred upon him the office of physician to the king. He died in 1772, when in his sixty-first year; having achieved, at home and abroad, a high reputation, not less for learning and philosophy than for eminence in the proper science of the physician. In the latter capacity he was acute, judicious, and of exact methods of thought and observation; bringing these qualities to bear on the study of nature, from which, as we have seen, the Spanish physicians had gradually receded since the era of Valles and the other revivers of the Hippocratic doctrines. The writings of Piquer were numerous, and so inspired, could not fail to be important. His first work, under the title of 'Medicina Vetus et Nova,' published in 1734, was received with high favour, and passed through six editions. Afterwards, there appeared in rapid succession from his pen treatises on natural philosophy, on logic, and on moral philosophy; with several monographs on medical subjects, and two systematic works, one on the theory and the other on the practice of medicine. In all of these there was much to reflect credit upon the writer and to improve the public mind: in the last, especially, amid much else that was valuable, the remarks on chronic inflammations were peculiarly worthy of attention. We regret that of his works none have fallen under our own immediate observation, with the exception of his dissertation on fevers, and his illustrated version of part of the writings of Hippocrates. The former of these works was originally published in 1751, and Morejon mentions three editions of it which appeared subsequently, the latest being in 1788. This, however, is in reality a fifth edition; that now before us,* purporting on its title-page to be the fourth, and published in 1777, having escaped the attention of the usually accurate and able bibliographer. The work, in which the author shows himself well acquainted with the investigations of Sydenham, Morton, Torti, and many other physicians of the best, and to him kindred school of observation, was

* Tratado de Calenturas. 4to. Madrid, 1777.

one of great value for its time, and may be consulted with advantage still. His translation of the selected writings of Hippocrates, with its ample and judicious comments, included the 'Prognostics,' and the three first books of 'Epidemics,'* though he was aware that the second of these was of more than doubtful authenticity. His introductions and illustrations to this performance are written with great elegance, learning, and judgment; and we encounter, at frequent intervals, remarks which, from their general truth, or their special appositeness, one would gladly retain in the memory. This work also passed into its second edition; and the first volume, containing the Prognostics, even into its third. The first editions of the several volumes, three in number, appeared in 1756, 1761, and 1770: those before us appeared also separately, and bear the dates of 1769, 1770, and 1774.

What Piquer was to scientific medicine in Spain, Pedro Virgili, his contemporary, was to scientific surgery; but in this not so much by his writings, as through the powerful impetus he gave for its advancement by his successful organization of those separate surgical schools to which we have already referred. One work by Virgili, which we ought not to neglect to mention, is his 'Compendium of Midwifery,' composed for the use of the new surgical colleges; and we direct observation towards it the more willingly, that this department of medicine has generally received in Spain infinitely less than its due share of attention. The posthumous work of Gaspar Casal, on the 'Natural and Medical History of the Principality of the Asturias,' published in 1762, deserves commemoration for his account of the leprosy as it prevailed in that province. The same interesting subject, as it refers to a more southern division of the kingdom, was taken up by Bonifacio Jimenez y Lorite, in a medico-legal dissertation which appeared at Seville in 1766; while only last year, we may add, Francisco Mendez Alvaro, in a little treatise published at Madrid, has shown to what degree the disease is still extant in Spain in the middle of the nineteenth century. Juan de Herrera, in 1766, entered into a critical examination of the plan recommended for the treatment of erysipelas by the application of caustics, and sought to restrict their use to cases occurring in patients of weak habit, or to those in whom the attacks were recurrent and without marked fever. Some observations on 'Medical Education,' which appeared anonymously in the Portuguese language in 1773, but which are understood to have been the production of Dr. Riveiro Sanchez, have attracted and rewarded our attention by the account which they give of the prevailing methods and topics of instruction at the University of Coimbra in the eighteenth century,† and by the valuable suggestions for improvement of which they are made the vehicle. The fact that a translation into Spanish, from the French version of the works of Cullen, by Bartolomé Piñera y Siles, was published in eight quarto volumes in 1791,

* Las obras de Hippocrates mas selectas, traducidas en Castellano é illustradas. Tomos iii. 4to. Madrid, 1769, 1770, 1774.

† Metodo para aprender e estudar a Medicina. 8vo. 1773.

and passed into a second edition, carries with it some significance. Antonio Gimbernat, an excellent anatomist and skilful surgeon, who effected much, along with Virgili, for the advancement of surgical studies in Spain, became well known throughout Europe for his account of the structure of the crural arch, and especially for his description of the ligament or aponeurotic fold which retains his name, and with which he connected a plan for an improved operation in hernia. Gimbernat signalized himself by various other improvements in surgery, and among the rest, invented and used successfully an apparatus for the cure of aneurism by compression. The description, by Juan Manuel de Arejula, of the yellow fever as it prevailed in Cadiz and Malaga at the beginning of the nineteenth century, is a work of high character,* which we have consulted with advantage, and in which the question of contagion, in particular, is treated with great acumen.

In the last year of the eighteenth century, an attempt was made to unite, under a single class of professors, the courses of clinical instruction in medicine and surgery which had been thrown separate by the establishment of the surgical colleges; but already, in 1801, the study and advancement of surgery appearing to suffer, the separation was again directed and accomplished. During the eighteenth century, the doctor in Spain was still, as antecedently, educated and regarded as a man qualified to teach, and so ranked in a class above that of the licentiate, who was merely certified as qualified to practise; thus recognising the very obvious distinction between the knowledge and intelligence which was necessary to devise and to interpret rules, and that which was simply necessary to comprehend and to follow them. The doctorship was thus a post of honour towards which a career of ambition was opened; and the distinction between the grades was not the less real that the teacher failed in his functions, for the effect of this was only to reduce the licentiate proportionately lower in the scale. It was evidently still in harmony with the idea of this distinction that in 1821 the Cortes directed the division of the entire scope of medical studies into four compartments, arranging what were to be held as indispensable studies in the first, those qualified as necessary studies in the second, those substantially useful in the third, and the merely accessory in the fourth.† Great fluctuations, however, still continued to occur in the ordinances for medical education, and in the resulting relations of the profession, among the members of which there consequently prevailed much discontent and dissension. There was an unhappy interfusion of merely political motives, when, in 1830, Ferdinand VII. issued a royal ordinance, creating medico-chirurgical colleges in ten different chief cities of the kingdom. With the intervention of a few minor changes, there was again, in 1843, a fundamental alteration, according to which two principal faculties were appointed, one at Madrid and another at Barcelona; with five subordinate colleges, at Seville, Valencia, Saragossa, Valladolid, and

* Breve Descripcion de la Fiebre Amarilla padecida en las Andalucias. 4to. Madrid, 1806.

† Sámano, Compendio Histórico de la Medicina Española: Apendice, p. 13.

Santiago. Each of the Faculties was to be endowed with no fewer than twenty medical chairs, with a body in addition of twelve agrégés or assistant-professors. The colleges, on the other hand, were to have each only five chairs, with three agrégés, and were destined for the training of a secondary order of practitioners.

The reduction or the annihilation of the privileges of the medical faculties of certain universities, with other provisions of this ordinance, was not fitted to calm the dissatisfaction which still subsisted extensively on all topics relating to medical education and to the distribution of qualifications for practice; hence once more there was vacillation, ending again in an important subversion of the arrangements, which was definitively announced in 1847. According to the ample curriculum, partly newly laid down, and partly merely confirmed, at this period, the licentiateship in medicine was not conferred till after a period of study of seven years: and for the doctorate two additional years were demanded, to be devoted to the pursuit of certain departments of medical knowledge not required for the mere licence, among which was a more extended acquaintance with chemistry and practical chemical analysis, including its application to medico-legal inquiries, as well as with comparative anatomy and certain other branches of natural science, and, lastly, with medical history and literature, and the most approved methods of imparting professional instruction. In 1849, there was still discontent, and still change. The Faculties for medical instruction of the first class were now to be those of the Universities of Madrid, Barcelona, and Seville, the medical faculty of the latter having its seat in Cadiz; while for that of the second class, provision was made in the Universities of Valenčia, Santiago, Salamanca, and Granada.* The regulations for the first class remained as before: for the second class, it was a first provision that no one should enter on the study of medicine who, along with a competent knowledge of Latin,† did not possess the degree of Bachelor in Science, to be conferred upon examination after a course of study of at least two years in logic, the elements of arithmetic, geometry and algebra, the elements of natural history, and the elements of inorganic chemistry. The course of purely medical education, extending over a period of five academical years, each of nine months, having been next completed, the candidate was to be subjected to three successive examinations; when, if found qualified, the title of physician of the second class was to be conferred upon him, under which he attained merely to the right of general medical practice, but could hold no place, and exercise no functions, in relation to medical administration, or to matters of medical jurisprudence, unless in the absence of a physician of the higher order. The physician of the first class was not necessarily to be a doctor of medicine; but he was held debarred from no description of function, with the exception of that of teaching, to be still reserved to the doctorate, a grade for the acquirement of which, as already men-

* Sámano: Apendice, p. 53.

† In 1854, Greek appears noted as added to the other requirements, prior to matriculation as a student of medicine. Semanario Medico Español, 1854, p. 111.

tioned, nine years of study are demanded. Below all was a class of surgeon phlebotomists, who passed through a course of study of two years, and to whom the minor operations of surgery were alone entrusted; but by a decree issued in 1854, this class, we believe, is henceforward abrogated. Pascual Madoz, in his extensive and valuable Statistical Dictionary of Spain, while he confirms most of the statements which we have here derived from Sámano, adds that the doctorate is granted in Madrid only, the privilege of conferring it not being allowed to the two other chief Faculties. In the year 1848–9, the same authority tells us that the matriculated students at Madrid were eight hundred and seventy-one, of whom only forty-nine aspired to the doctorate.* As a basis for this great medical school, existing at what is thus designated as the "Universidad Central," with its thirty-two medical professors and assistant-professors, Madrid, we may mention here, has its great General Hospital of upwards of one thousand five hundred beds,† giving gratuitous accommodation to an average of fourteen thousand patients yearly, with from three thousand to four thousand besides who make certain regulated payments. The mean number of beds occupied is reported to be about twelve hundred. To the confused and fluctuating legislation of which we have thus endeavoured to trace an outline, mingling, as it does, so much of what appears strenuous in aim with what has proved feeble and uncertain in execution, a climax was given by a royal ordinance appointing two chairs for the Theory and Practice of Homœopathy, faith in which, as might be anticipated, is largely prevalent in Spain. Indeed, we learn from Sámano that charlatanry is everywhere rampant throughout the country.

But we must draw our remarks nearer their close, and can allow ourselves to add but little regarding the more recent writers. Luzuriaga, who studied in Britain, and Felipe Monlau, have acquired a high reputation among their countrymen as writers on hygienics; Ignacio Ameller and Felix Janer have written ably on pathology; and there have appeared one or two esteemed works on medical jurisprudence. The medical history of Morejon, which has suggested our present labour, would be a credit to any country, and in the relative copiousness of its bibliographical department we do not know its equal. Most of the publications, however, on practical medicine and surgery, and indeed on nearly every branch of medical science, have been lately, and are now, merely translations; and even these have been almost exclusively limited to French sources. In original monographs on therapeutics, diagnostics, surgery, or midwifery, the modern list is indeed meagre, and, beyond Spain, the names of the authors are everywhere unfamiliar. Thus it is that, whatever portion of a better spirit appears in their medical literature and practice, is not a national spirit, and can only in a very moderate degree be attributed as a national honour. Nothing can show more clearly than this the unfortunate condition of intellectual torpor and dependence into which

* Diccionario Geografico-Estadistico-Historico de España, tom. x. p. 814.
† Op. cit., tom. x. p. 872.

the old energies of the Spanish physicians and surgeons have been lowered. The Spaniards themselves, however sensitively they may repel the insinuation of the wide-spread apathy from abroad, evince their consciousness of it through their having contemplated a confederation of the more zealous among their eminent men, originated in 1846, the main object of which was not merely to promote medical science generally, but expressly to aim at the "creation of a national medicine." Medical periodicals, each advocating liberalized, and often wise and salutary views, have risen and fallen, one after another, in rapid succession, having been unable to secure any sufficiently extensive basis of support, though each has been attempted on a scale that appears sufficiently humble, when compared with that of the journals of even the smallest and least affluent of other European countries. We move coldly among these shadowy representatives of the former vigour and genius of their country, and feel that the tameness that characterizes our theme creeps depressingly over us, in contrast with those heartier sympathies which the earlier history awakened. Still there is, as we have portrayed, a restlessness under their present condition, which necessitates, as it augurs, a further change. Let us hope that their future aspirations may be more real than apparent, and may lead to some proportionate effort, such as we cordially desire, in order to the success that will restore Spain to her just rank as a ministrant in that universal progress behind which she has so long lingered. Yet, doubtless, it will be sanguine to expect, even under the wisest management, any instantaneous regeneration of institutions which have degenerated into a complex system of abuses, such as the former absolutism of rule showed neither the zeal nor the capacity to remedy.

We glean from Sámano some interesting particulars, serving to illustrate the existing social position of the medical practitioner in Spain, which we are the more ready still to quote that they are of a description not usually met with in the pages of a medical history. Thus we gather that the yearly emolument of the medical chairs in the Universities appears to vary from 125*l.* to upwards of 300*l.*, the highest salaries being attached to those in Madrid. The rent of a house, occupied by a physician of respectability in the capital, ordinarily averages from about 30*l.* to 40*l.* a year; while his fee for a customary visit to the middle classes varies from about twenty pence to two shillings, an amount which is usually doubled for the wealthier classes. For consultations, which are very commonly resorted to, and are matters of great formality, the honorarium is from about 12*s.* 6*d.* to 16*s.* 8*d.*, or from three to four *duros*. In the chief towns in the country, on the other hand, the fee for a visit to the middle classes is usually tenpence, which is also doubled for those higher in station. To the medical attendant of the rural districts, payments are often made partly in money and partly in kind. When in the latter, the practitioner is sometimes the happy recipient of a quantity of grain or pulse, and sometimes of wine, or hemp, or potatoes. We may easily believe that not rarely his remuneration is exceedingly meagre, and that he is often unable to equip himself for paying his visits on horseback; so that it is usual to send a horse for him, in express cases, along with the messenger who requests his attendance. In many instances, in consideration of his furnishing the whole services for a given district, he receives a stated salary from the local authorities, which may commonly amount to from 40*l.* to 60*l.*, but sometimes to the double of this sum, from 60*l.* to 80*l.* being probably the average. In some localities, the salary is partly made up by allowances of a house rent-free, firewood, mast for a pig, or pasturage for a horse, with exemption from communal taxes. The barber-surgeon, or surgeon-phlebotomist, when salaried, has from 10*l.* to 60*l.* yearly; a common average being from 30*l.* to 40*l.* He is expected, in addition to his more strictly surgical duties, to attend to the beards of the community himself, or to provide a substitute.* Many of these provisions are loudly, and doubtless justly exclaimed against, as entailing a life of penury, degradation, and hardship. What has been the effect upon them of the general sanitary law, regulating, among the rest, the employment and remuneration of the district medical attendant, which passed the Cortes at the close of 1855, and therefore since Sámano wrote, and of which we have read the full details in a Spanish medical journal,† we have had no adequate opportunity of ascertaining. That it, however, as little as the preceding law, of medical organization of 1854, for a complete transcript of which we have referred to another medical periodical,‡ was not held to present any sufficient guarantee at the time, the many murmurs that arose, and the establishment, almost coincident with the promulgation of the law of 1855, of an association styled the "Emancipacion Medica," destined to protect the interests by consolidating the action of the profession, have afforded sufficient testimony. With the instability of Spanish medical legislation, probably other changes have taken place since, but these we shall not now pause to investigate.

We thus bring to a conclusion our sketch of the history of Spanish Medicine during what may be regarded as its purely Spanish periods. Those who consider merely the place in which the result of our researches appears may think that they have been extended to too great a length: those who regard what has been the width of our field, or in how far our investigation has been original, and who will judge how much, in order that the characteristic features of our theme might be preserved and yet not rendered redundant, it was desirable to include, and how much it was necessary to exclude, with almost equal labour in the rejection as in the selection, will possibly admit that we could scarcely have approached any tolerable success while restricting ourselves to narrower limits. In seeking to trace the leading events, and associate with them the leading names in the annals of our art in Spain, and in attempting to attach to both, in some measure, the impress of their times, we believe that not much has been omitted which it would be just to regard as essential; though much, and many names, certainly, have been inevitably passed over, which the Spanish phy-

* Sámano; Apendice, pp. 164, 177.

† El Siglo Medico, Periodico Oficial de la Real Acad. de Med. de Madrid, Año ii. p. 387.

‡ Semanario Medico Español, Año i. pp. 4, 12.

sician would willingly have seen introduced, and even magniloquently lauded. But the part of the reviewer of a period or of a department of medical history is to render justice, and not to administer flattery. As he proceeds with his task, he learns to look on success and on failure, on fortune and on misfortune, as alike the incidents of a day: and a calm resignation visits him, while he constrains himself to consider in events, not so much the fates of individuals, as the broad results of human effort and progress; for the race lives, while the individual perishes. Not that he can be indifferent to honour or neglect, to merit or demerit; but he teaches himself, or ought to teach himself, to view these in their finality, and more in relation to society than to the individual man, with whom it has now become too late to sympathize or whom he can no longer reach with censure. This truth then alone remains for all available: that an honour unduly paid, or a neglect unduly inflicted—and each is frequently encountered by him in his researches—is alike a wrong to the community, with whom the result of abuse in the distribution of its rewards or of its penalties remains as a permanent injury, while upon the objects of either the effect is but temporary. Thus each receives the lesson to accept his own destiny, whether for favour or disfavour: conscious, meanwhile, that by neither issue is he released from the duty still to struggle onwards, according to his capacity, towards a worthy purpose, not aiming so much at becoming indebted to the world, as to acquire the nobler position, if he dare conceive it, of having rendered the world his debtor.*

* The extended remarks on the "History" of Medicine in Spain, now brought to a close, have so much interest attached to them that we hope in a future number to present to our readers an article with which we have been favoured, upon the *existing* condition of medical knowledge and practice in that country. An account of the actual state of any branch of science or art naturally follows its "history" in bygone times; and in the present instance such a sequence will be rendered the more acceptable as, in comparison with our familiarity with the medical literature of Germany and France, the profession in England are but very imperfectly conversant with that of Spain. The observations in the forthcoming article will be almost exclusively restricted to the modern aspect of Spanish medical matters, being based upon the results of a recent visit to the Peninsula.—Ed.

Review II.

1. *Abstracts of Information on the Laws of Quarantine which have been obtained by the Board of Trade.* Transmitted by the Quarantine Committee of the National Association for the Promotion of Social Science.—1860.
2. *Copy of Abstracts of Regulations in Force in Foreign Countries respecting Quarantine.* Communicated to the Board of Trade by the same Committee.—1860.
3. *Papers respecting Quarantine in the Mediterranean.* Presented to the House of Commons by command of her Majesty in pursuance of their Address, dated May 7th, 1860.
4. *Report of Dr. W. H. Burrell on the Plague of Malta in* 1813.—1854.
5. *Quarantine and the Plague: being a Summary of the Reports on these Subjects recently addressed to the Royal Academy of Medicine in France, with Introductory Observations, &c.* By Gavin Milroy, M.D., &c.
6. *The Cholera not to be Arrested by Quarantine, &c.* By the same Author.
7. *The International Quarantine Conference of Paris in* 1851–2, *with Remarks.* By the same Author.
8. *Sketch of the Operation and of some of the most striking Results of Quarantine in British Ports since the beginning of the Present Century.* By the same Author.—1853.
9. *Quarantine as it is, and as it ought to be.* By the same Author. —1859.
10. *Proceedings and Debates of the Third National Quarantine and Sanitary Convention held in the City of New York.*—1859.
11. *Proceedings and Debates of the Fourth National Quarantine and Sanitary Convention held in the City of Boston.*—1860.
12. *On the Quarantine Classification of Substances, with a View to the Prevention of Plague.* By John Davy, M.D., F.R.S. ('Phil. Trans. Ed.' Vol. XV., Part II.)

The works which form the heading of this article may convey some idea of the attention which has been paid to quarantine, and of the interest which it is, and has for some time past been exciting. Like every other important subject, it has required to be agitated and ventilated, and few have been more so, or with more need. That good will result from the inquiry cannot be doubted; at least we cannot doubt, having full confidence that truth must prevail in the end, that what is false and delusive can only endure for a time, and that "Wisdom is justified of her children." It has been well said, that "the strongest evidence of human progress is the conquest of science over error and superstition." The good we anticipate is, that if quarantine is not abolished entirely as an unmitigable evil, it will be so modified as to change its character altogether, rendering it, not as at present, and as hitherto conducted, vexatious and injurious in the extreme, affording no real security to the public health; but the very opposite, as little annoying and hurtful as possible, and as defensive as circumstances will permit in the way of protection against the spread of infectious diseases.

It is a good omen, we think, that the "Society for the Promotion of Social Science" has selected it amongst many more for inquiry, and that a sub-committee has been appointed to carry it out—a sub-committee composed of individuals the majority of them of the medical profession, men of experience and many of them eminent, and we would hope, all of them to be depended on for exercising their mature judgment, and, as far as that can be, free from bias on a matter so important. Subjoined are the names of these gentlemen.*

The history of quarantine, strictly speaking, setting aside what is vague and in a manner transitory, does not reach back to any very remote period. We should in vain consult any of the ancient historians, or even the historians of the Middle Ages, for traces of it. In the ancient times clean bills of health and passports were equally unknown. The merchant, the traveller, might pass from Rome to Athens and from Athens to Alexandria unquestioned. No officer of health, no policeman stopped him on the threshold of the country to which his curiosity or his business led him. In the best times of Greece and Rome, and long before and after, there were no lazarettos for the imprisonment alike of the healthy and diseased. The Mediterranean was truly a *mare liberum*, not the *mare clausum* which it has since been rendered; the intercourse between its shores was uninterrupted, the intercommunion of the peoples inhabiting them was perfect; they enjoyed all the advantages which that inland sea, that noble high road of nations, afforded. So recently even as the fifteenth century was the first interruption made to this happy state of things. It was in this century that Europe was subjected to great calamities; Constantinople was taken by the Turks, the Greek empire was overthrown,

* B. G. Babington, M.D., F.R.S., President of the Epidemiological Society.
Thomas Bazley, Esq., M.P.
A. Bryson, M.D., F.R.S., Inspector of Hospitals and Fleets.
Sir James Clark, Bart., M.D., F.R.S., Physician to the Queen.
John Davy, M.D., F.R.S., Inspector-General of Army Hospitals.
W. Farr, M.D., F.R.S., Registrar-General's Department.
J. Gibson, M.D., C.B., Director-General Army Medical Department.
T. B. Horsfall, Esq., M.P.
Sir John Liddell, M.D., F.R.S., Director-General Navy Medical Department.
Sir J. R. Martin, C.B., F.R.S., Physician to the Secretary of State for India in Council.
J. O. M'William, M.D., C.B., F.R.S., Medical Inspector Hon. Board of Customs.
Gavin Milroy, M.D., F.R.C.P., late Medical Commissioner in the West Indies and in the Crimea.
Richard Owen, F.R.S., Member of the Institute of France, &c.
Sir William Pym, M.D., Superintendent-General of Quarantine.
T. Southwood Smith, M.D., F.R.C.P., Member of late General Board of Health.
T. Spencer Wells, F.R.C.S., late Surgeon Civil Hospital at Smyrna and Renkioi, &c.
John Wiblin, F.R.C.S., Medical Superintendent of Quarantine at Southampton.

wars of great barbarity prevailed, and epidemics, the so frequent accompaniments of wars, were terribly fatal; and one especially, that of Lues, as at the time it was considered: this broke out and spread with a rapidity as surprising as it was alarming. The idea of contagion had before been gaining ground, and had been acted on partially in the preceding century in times of terror, during the direful visitation of some of the most destructive diseases that ever devastated Europe, such as the sweating sickness, the black death, and the plague. This new pest, as it was then held to be, probably more than any other, attracted public notice, and fixed attention to the subject of contagion and helped to make it popular. The notion so initiated, the writings of a man of great ability and of European reputation as a physician and poet, who flourished shortly after, were well adapted to enforce and confirm. This man was Fracastorius, "the heaven-preserved," who, as the term implies, was born under circumstances which, in a superstitious age, might well excite a strong feeling in his favour, and add to his influence.*

It was in the middle of this century—viz., 1448—that the first code of quarantine regulations was promulgated, and that in the same city in which a few years before the first lazaretto was established. This city was Venice, then the great emporium of trade with the East, and most in danger, consequently, of suffering from an imported contagious disease.

That code appears to have been the model of all subsequent ones up to a very recent period. In it certain things were laid down—viz., that plague, the Oriental plague, against the introduction of which quarantine was first and solely established, is a contagious disease, capable of being propagated by contact, and by contact alone, as regards persons, and engendering a contagious matter, a fomes, capable of adhering to certain inanimate substances, and of being retained by them for an almost indefinite time, without losing its activity; and, on the other hand, incapable of attaching itself to other inanimate substances, and which consequently might be handled, not like the preceding, with risk of imparting the disease, but with absolute impunity, with perfect security. As the name implies, forty days was the time

* It is related of him that he came into the world with his lips so adhering as to require a surgical operation to separate them; and that his mother, when she first took him into her arms, was killed by lightning, he escaping unhurt. His poem, entitled 'Syphilides,' dedicated to Cardinal Bembo, and his treatise 'De Contagionibus,' &c., are the works referred to above. That about this time there were no very definite ideas respecting contagion may be inferred from the circumstance that even a century later Bacon was of opinion that in the instances of supposed infection by contact it was the effluvium which took effect. He says—"Pestem quoque excitavit januarum, rimarum, aliorumque inunctio, non tam ex contactu quam quod homini in more positum, si quid humidi adhærescat digitis, naso illud admoveri."—Sylva Sylv., cent. x. There are other passages to the same effect. What we once witnessed might perhaps have shaken the faith of our great English philosopher in this his speculative view; and might be adduced also as one of the many proofs that the intensity of the contagion of plague is overrated. The incident we allude to occurred in the lazaretto of Constantinople, on the Asiatic shore of the Bosphorus; the medical attendant, a Jew, whilst we were looking on, after opening the bubo of a patient labouring under plague, placed the bistoury between his lips, as a place of rest, without even wiping it, whilst he dressed the incised part, and from the careless manner in which he did it, evidently without thought of danger.

first fixed—and with as little reason as the other conditions—for the probation of those coming from countries where the malady was either existing or suspected of being amongst the inhabitants.

The question will naturally be asked, how it was that these principles of quarantine were established? We have said they were taken for granted, or, we would add, were founded on hypothetical views of the vaguest kind. This, we believe, is strictly true, being adopted at an ignorant time, when medical science was little advanced, when the exact sciences were little cultivated, and when there was a perfect incompetency to solve the several questions involved in the system—questions many of them still perplexing the inquiring mind. What was presumed and made a rule—the mere *dicta* of authority, gaining force with age—came to be called the results of experience, were received as laws and venerable, and almost sacred as such. This at least is the conclusion we have come to after some research, and not merely in books, but in quarantine establishments, in one of the oldest and in one of the newest, that of Malta, and that of Constantinople, where, it might be supposed, if anywhere, that some satisfactory reasons might be elicited from the officials respecting their usages. The curious in these matters, we cannot but think, would draw the same conclusion that we have been compelled to adopt, were they to consult the writers of the period, and those of the highest authority, such as the author already named, Fracastorius. Here, as a specimen, is his definition of contagion: "Si licet aliquo modo contagionis rationem subfigurare, dicemus contagionem esse consimilem quandam misti secundum substantiam corruptionem, de uno in aliud transeuntem infectione in particulis insensibilibus primo facto."* This may almost suffice regarding the doctrines of one who considered so many diseases variously contagious as by contact, by fomites, at a distance, and amongst them phthisis, explaining them all in minute detail, and exact particularity after the scholastic Aristotelian fashion, ringing changes on the words hot and cold, dry and moist, and in greatest difficulty having recourse to stellar influences and occult qualities.

Instituted in the first instance against plague, as already remarked, and for a long while so limited, in recent times the quarantine system has been extended to certain other diseases supposed to be contagious or infectious, especially yellow fever and cholera, on the idea—that, too, a presumption—that by enforcing the prohibitive system, an exemption from the infliction of these diseases may be secured.

The great object now is to collect information respecting the working of quarantine; how far it has succeeded, how far it has failed; what good there is in its rules, what evil; how far its practices are sound, how far fallacious.

We have alluded to one good omen, and a like auspicious feeling may be indulged in from the knowledge of the fact that the inquiry is exciting interest in other countries, especially in the United States and in France, as is indicated by some of the works prefixed.

To do justice to the subject, ample space would be required; limited

* H. Fracastorii: Opera Omnia, p. 77. Venet. 1573.

as we are by time and space, all we shall attempt will be to make our readers acquainted with some of the principal results that have been brought to light, so far as they are clear and definite and admit of practical application.

For the sake of brevity, we shall make two or three observations of a preliminary kind. The first is one admitted by all who have given their attention to the matter—viz., that the classification of substances into susceptible and non-susceptible and doubtful—that old classification—is altogether worthless, and as such may be set aside as, *per se*, vitiating the existing system, and altogether requiring supervision and correction. If any one entertain doubt regarding the justice of the sentence passed on it, we would refer him to one of the prefixed papers—that expressly on the subject by Dr. John Davy, in which he passes in review the several classes, examining the articles composing them, and showing, it may be said, *con rispetto*—to use the apologetic word in the East for a strong expression—the absurdity of the distinctions and the folly of the divisions, and how in its errors it undermines quarantine and renders it altogether delusive. Next, we need barely remark, considering whom we are addressing in this Review, that the diseases on account of which quarantine is enacted, such as plague, cholera, and yellow fever, are, as regards their nature, open to question—whether contagious or infectious, or neither—there being as yet no perfect agreement on the matter, some of the profession holding them to be highly contagious, some non-contagious but infectious, and some neither the one nor the other. Thus, on the whole, leaving the public in a state of doubt, and the subject, as regards legislation, one of expediency and compromise—that best foundation, we are told, for good laws. Again, we would say that those who have any difficulty in adopting this statement will, we are pretty certain, have it removed by consulting the Report of the Commission addressed to the Royal Academy of Medicine in France on Plague and Quarantine, of which a summary will be found, accompanied by some able remarks by Dr. Gavin Milroy, in one of the publications prefixed. Relative to the other two diseases, cholera and yellow fever, it is scarcely necessary to make any reference, the want of accord amongst the profession as to their nature being so notorious. Should there be any one seeking for particular information on this matter, we cannot do better than suggest his consulting two articles in former numbers of this Review, that for January 1848, and that for April of the same year and of the following July, on the contagion of yellow fever, in which he will find carefully and amply considered the opposite views of two very competent observers, both belonging to the same branch of the public service, the naval, with similar opportunities, going over the same ground, and taking the same data. These officers were Dr. M'William and Dr. King, both sent to examine and report on the fever of Boà Vista, which, according to the one, was introduced in the island by the steamer the *Éclair;* according to the other, was not introduced, but was of indigenous origin. These articles, ably and elaborately written, will well repay the reader, and are worthy of re-perusal by all interested in the subject, and especially for the purpose mentioned.

We shall now bring together such information bearing on quarantine as we may be able to extract with ordinary brevity from the public documents—the Blue Books specified in the heading, three in number, for which we are mainly indebted to the Quarantine Sub-Committee of the National Association. They embody the answers to the questions proposed by the committee, and circulated under the authority of the Government. The answers are chiefly from Her Majesty's consular agents and other official persons in foreign countries and our colonies. They are documents to which we attach great importance, both on account of the information they convey, and from their being of so reliable a kind, furnished by individuals as little as possible influenced by theoretical views, and of large experience, and no wise connected—in brief, giving evidence such as in a court of justice would be sure to carry conviction to the minds of our countrymen. They have another recommendation—they are admirably adapted to convey an idea to those who have never travelled of what quarantine is in operation, and what are lazarettos; in short, to give an insight into the whole system, if system that can be called which, under the same name, is now so diverse.

For the sake of order, we shall notice each of these documents apart; and first, that entitled 'Copy of Abstract of Regulations in force in Foreign Countries respecting Quarantine, communicated to the Board of Trade.' What is most remarkable in the quarantine regulations of different countries at present, as made manifest by this abstract, is the fact already alluded to, their want of accordance, hardly any two being alike, having been formed at different times, and promulgated with different intents; some, the earliest, having been directed solely against plague; others, later, against this disease and yellow fever and cholera; others against the two first, omitting the last, from the conviction that cholera cannot be excluded by any quarantine measures. Another noticeable peculiarity is, that the more liberal the government of a country generally, and the freer its institutions, the fewer and the less stringent are the quarantine restrictions. In the Baltic States, in Sweden, Denmark, Prussia, Holland, the regulations formally enacted may be considered almost as a dead letter; so in Belgium, where, to use the expression of the informant, they "are rather nominally than really in force." In the United States of America—employing the term retrospectively—each State of the Union has its own code; all of them, according to a resolution arrived at by the Convention of Delegates held at Philadelphia in 1857, inefficient and often prejudicial to the interests of the community. In Chili and Peru, and along the whole of the western coast of the South American continent, the tendency is to disregard all quarantine regulations, as interfering with the freedom of commerce. In that anarchical country, Mexico, quarantine is under no legislation, the Board of Health having unlimited power, which it sometimes exercises most tyrannically. In the South of Europe, in the old kingdom of the Two Sicilies, the codes are, or were, most elaborate and rigorous. In France and Sardinia, they have of late years undergone revisal; and yet, though somewhat improved, they are still open to great objection: fortunately, however, they are mildly enforced. In the Ottoman dominions—including Egypt, in which, little before 1840, there were no quarantine restrictions—a system has been established as elaborate as could well be contrived, and as inefficient as can well be imagined, being totally in opposition to the feelings and habits of the people.

We must not quit this part of our subject without giving, by way of illustration, an extract or two. Our consul at Malaga, speaking of the evils of quarantine, says, they are here

"Still further increased by the absence of all system or unity of action amongst the Provincial Boards of Health; the law is interpreted according to the fancy of each junta. The Provincial Boards have repeatedly acted, each of them, upon their own judgment, and in contradiction of the superior junta at Madrid."

On the authority of our consul-general at Havanna, it is stated, that

"All vessels, without exception, leaving that port for Spain between the 1st May and the 1st October, must proceed to Vigo, and there perform a quarantine, usually of fourteen days, although no yellow fever was in Havanna at the time of departure."

At New Orleans there is a regulation similarly irrational:

"From or about the 15th April, all arrivals from Rio Janeiro, the West Indies, and the Gulf of Mexico, are liable to a quarantine of not less than ten days, whether the bill of health from those places be 'clean' or 'foul.' This quarantine continues usually to the end of October or beginning of November. After that date, and until the next proclamation of the Governor, all vessels are allowed to enter this port at once, unless there is actual sickness on board, without reference to their port of departure, or whether any contagious disease existed there or not."

From our own experience, if we may be excused referring to it, we can speak of the inconsistencies of quarantine and its abuses. We shall mention but a very few of the many. On the same voyage we have been allowed "*pratique*"—that is, liberty to land, at one port in Sicily, and have been refused it at another but little distant, and only two or three days later; and this, not from any dread of our importing disease, but under the apprehension, on the part of the civil authorities, of our bringing Lord Cochrane, who was then in the Mediterranean, and in a schooner very like that in which we were voyaging.* At Lipari, on landing, we were met and welcomed by our vice-consul, and shaken heartily by the hand, before we had *pratique*, but in the *absence* of the health officer; as soon as this official appeared, our friend drew back, cautioning others to do the same, till our papers were duly examined and approved. At Constantinople, at a time when the

* He was then on his way to Greece to join in the war of independence. The persuasion of the authorities at Catania—where the incident above mentioned occurred—was that, were he allowed to land in Sicily, the people, in their abhorrence of the Bourbon tyranny, would have risen and have proclaimed him King.

quarantine authorities were boasting of the exemption of the city from plague, in consequence (as they maintained) of the new regulations, we met at the breakfast table a stranger who had landed on the shore of the Bosphorus, and had preceded his vessel, and who hearing of her arrival, said he must hasten to the Parlatorio to join the master to obtain *pratique*.

We will not further task the patience of our readers with other incongruities; were only half of them which have come to our knowledge detailed, they would fill a volume of no small size. But, in connexion with quarantine, we must not altogether omit mention of lazarettos. These buildings, set apart for the reception and close confinement of persons under suspicion of infection, or coming from countries where the diseases dreaded have prevailed, or in communication even with such countries, are commonly anything but what they ought to be; not only is comfort disregarded in them, but often the health of their inmates. Too often, indeed, they are in a state more likely to engender disease than fitted for the preservation of health. Medical men recommending patients to visit Sicily or any part of the South of Europe, for the benefit of a mild winter climate, would do well to keep this in mind. The following is well adapted to enforce the caution. It is an account of the treatment of passengers arriving in steamers from Marseilles in 1853–54, at Nisida, one of the lazarettos of Naples, and was given by Mr. Ewart, then residing in that city, and addressed to the 'Times' of January 10th :

"They [the passengers] were all mingled in one dreary room, without compartments and without glass to the windows. In this place they were all condemned to remain ten days. Among them were several English ladies. But the discomfort of their situation was converted into horror when they discovered that in the same building, and separated from their place of exercise by a low wall only, were eight hundred convicts of the worst description, who appealed, and not without threats, for pecuniary assistance. During the stay of our countrymen in this quarantine gaol, several of the convicts escaped."

The lazaretto at Lisbon, as described, and the treatment experienced in it, are nearly on a par with the preceding. The account is given by thirteen remonstrating passengers; it appeared in the public journals at the time (1854), but is too long for entire insertion here: it will be found in p. 16; we shall insert only a few words of it: "On entering the lazaretto at 6 P.M., we were dismayed to find it already full to overflowing, and hence a struggle ensued for shelter, bedding, and provisions, which continued until midnight." The next is an account of a Turkish lazaretto at Beyrout, described by Dr. Robertson, Deputy Inspector-General of Hospitals, as

"Most wretched and in a most unhealthy position. The neighbourhood is low and swampy; the rooms are filthy and damp, being open to the weather; and it is only wonderful that all who enter do not fall victims to disease of some kind, if not to plague. To this state of the lazaretto I attribute the frequent attacks which the attendants suffered during the prevalence of plague." "At Damietta," he adds, "travellers have been obliged to perform quarantine in a miserable shed on the sea-shore."

Dr. Davy describes something worse, a lazaretto which he charitably supposes to be the worst in the Turkish dominions, and it would be difficult to imagine anything worse: "A low hut, not unlike an Irish cabin, divided into three small cells, without fireplaces or windows, with no intended passage for air or light, excepting by the door, and with the naked ground for the floor;" and this at Costangee, on the north-western shore of the Black Sea—with a winter climate as severe now as that described by Ovid in his 'Tristia,' and in all its features as little changed as possible from what the poet witnessed when it was the scene of his banishment. One more description, and we have done with these details; it is of a plague hospital, and is to be found in p. 23 of Dr. Burrell's very valuable 'Report on the Plague of Malta in 1813.' Sir Brook Faulkner writes:

"The result of about half an hour's visit to the Maltese pest hospital on the 2nd June, may convey some faint idea of the sufferings and privations to which those labouring under this horrible disease were subjected. These miserable creatures lay within a very short distance of each other, five or six on the floor of the same room; twenty-eight of them were attended by two convicts. They had no change of linen, and were therefore obliged to lie either without shirts, or in their foul every-day clothes."

We shall now pass on to the other two documents. The one entitled 'Papers respecting Quarantine in the Mediterranean, &c.,' is very instructive in its contents, as descriptive of the regulations enacted for the several quarantine establishments, and is very deserving of being consulted and studied by those who may be desirous of full information on the subject. In following its details, they would find almost in every page confirmation of the remarks we have made as to the want of accord and of efficiency of the quarantine system in the East. We shall give a very few extracts; and first, as showing the evil of keeping a crew on board ship when disease has broken out, and the benefit of landing them; an evil and a good that cannot be too strongly insisted upon. Quarantine was first established in the Principalities bordering on the Danube in 1829 or 1830. The Vice-Consul at Galatz states that "during the whole time the quarantine existed there, about twenty-four or twenty-five years, no case of plague occurred in the lazaretto. But it is on record that the plague was on board of a vessel, somewhere about 1834, and that all the crew died, or all excepting one man." We remember, when in the Ionian Islands, hearing of a similar instance at Zante, and of a like mortality, the crew of a Turkish vessel with the same disease being kept on board, and this under British rule. And in the *Eclair* steamer, that ill-fated ship, we have an example at home of the same kind in a less degree. On her arrival from the coast of Africa, instead of being allowed to land her sick at Portsmouth, where an offer was made to receive them into the well-aired wards of Haslar Hospital, she was ordered to Stangate Creek, there to perform a lengthened quarantine with some fresh volunteers on board, one of whom, the pilot, contracted the fever and died, as well as many of the remaining crew. Dr. Milroy thus describes the event; we quote from the arrival of the steamer:

"Already upwards of one-half of the crew had perished since the commencement of the sickness in July, and every day added fresh victims to the list. It is needless to say that the utmost alarm and depression existed among all on board. The surviving medical officer urged the immediate landing of the crew, as the only means of arresting the terrible ravages of death; and Sir J. Richardson, the physician of Haslar Hospital, expressed his readiness to receive them into the wards of that noble institution; an advice that was cordially seconded by Sir W. Burnett. Had this step been taken, much distress would have been spared, a heavy expense avoided, and what is of far greater consequence, several valuable lives might have been saved. But, unhappily, the fears of our Quarantine authorities prevailed over their judgment."

Other instances might be given and from the documents under consideration of a like excessive mortality in ships from disease if, as when on a long voyage, they were kept at sea from necessity, or, on entering the port, the crews were prevented from landing by the local authorities. Examples of the opposite kind, of which also there are many in these pages, are equally instructive, and on that account, as well as for the pleasure of making them better known, we shall notice one or two of them; and for this purpose we must open the third document, 'The Abstracts of Returns of Information on the Laws of Quarantine.'

"Towards the end of 1852, H.M.S. *Dauntless*, with thirty-three cases of yellow fever on board, was admitted at once [on her arrival at Barbadoes] to pratique, the sick landed and removed to the military hospital of St. Anne's, where they rapidly recovered. They were mingled with the other inmates in the wards of the hospital: no instance of the disease being communicated to the latter or to the attendants occurred, and the garrison remained healthy. The disease had been very fatal in the *Dauntless* before her arrival." (p. 70.)

In a despatch from Consul Kertright, dated Carthagena, February, 1853, he states: "The cases of yellow fever at this port have been exclusively confined to persons landed from the Royal Mail Steam Packet Company's ships, and have no way affected the health of the town." He adds,

"On a late occasion, at the urgent request of Captain Wilson, ten men and two officers of the *Dee* were landed here, suffering severely from yellow fever; as the quarantine regulations formerly in force at this port had been rescinded, owing to the reports of the Board of Health of Great Britain, there was no obstacle to their being landed and placed in the General Hospital, and I have the satisfaction to report that, with the exception of two already in the last stage of black vomit, I sent the whole, including the two officers, on board the ship convalescent, and without the disease in any way affecting the general health of the town."

It is further stated, "In the opinion of the medical men in attendance on board the *Dee*, and concurred in by the Captain and the men themselves, that had they not been landed at Carthagena, it is probable that few, if any, of the ship's company would have been saved" (p. 31.) The next example we shall give is one of extremes, of extreme inhumanity and humanity. The details are so interesting that we shall not abridge them.

"In the summer of 1855, when yellow fever raged with the greatest violence in most of the ports south of Baltimore, the ports to the north and east of

Baltimore, without exception, established a most rigid quarantine upon arrivals from the south. At Norfolk and Portsmouth (in Virginia), situated on opposite sides of the James River, 180 miles distant from this city, the fever raged most malignantly. The inhabitants first sought refuge by flight to the neighbouring towns and villages; but this was soon prevented by the people there, who turned out with arms, and drove them back to their own pest-smitten city. All communication by rail and boat was cut off, and one mode alone remained, viz., by the daily line of steamers from Baltimore to Norfolk, and no impediment was offered by the authorities and people of this city to the arrival of the fugitives. Daily did the steamers convey provisions, medicines, clothing, coffins, &c., and daily did they return laden with fugitives. On arrival opposite the Marine Hospital, the steamers stopped until they were boarded by the health officer, who removed any case of fever that might exist on board, and then allowed her to proceed and land her passengers, &c. Several hundreds of these took up their quarters at once in the hotels of the city. Some sickened with fever shortly after landing. The number of deaths thus occurring was about fifty. Not a single instance was known to have arisen from contagion, all being distinctly traced to those persons alone who had come to this city from the infected districts. The utmost vigilance was employed all the while by the health authorities to thoroughly cleanse and purify the city, particularly all ship-yards, wharves, drains, cellars, &c." (p. 28.)

The next point we shall advert to is a very important one—that of the question of the power of goods to convey the matter of contagion or infection. From the examination of the several reports of the consuls contained in the abstracts, it would appear that, with one exception, there is a general agreement amongst them that articles of merchandize are incapable of becoming media of the kind, and founded on the fact that those whose duty it is to air the goods needing depuration according to the regulations, have never contracted disease, neither plague, yellow fever, nor cholera. The late Sir Frederick Ponsonby, when Governor of Malta, stated, as the result of his inquiries, that there was no instance on record in any lazaretto of a person contracting plague from handling cotton imported from places where the plague was prevailing. And the testimony of Sir W. Pym (he, too, now no more), after careful research at the different lazarettos in the Mediterranean, is to the same effect. The exception alluded to is that of the acting consul, Calvert, at Alexandria, who says,

"Lorsque pendant des epidémies de peste nous avons eu au lazaret des merchandises susceptibles, il y a eu des porte-faix qui, en maniant et en exposent ces merchandises à l'air, ainsi que cela est prescrit par les règlements, ont contracté la maladie, et en sont morts. D'ou l'on est en droit de conclure que la peste se communiquer par les effets ou merchandises susceptibles."

This gentleman, in drawing the conclusion, appears to have forgotten a former remark which he made relative to the infraction of quarantine:

"On ne pourrait éviter des infractions même en augmentant le personnel. Ce fait est suffisamment prouvé par la contrabande qui s'opère journellement sur tous les points de l'Europe où il existe des lignes formées de nombreux gardiens de la douane, et dont le service est fait incontestablement avec des éléments bien supérieurs aux nôtres."*

* The following is a striking confirmation of the well-known fact: When a certain contraband trade, in the time of William III., was carried on between France and England

Another remarkable fact that we learn from these documents is the many places in the Levant, in the very centre, as it were, of the plague-region, which have not for a long time, and some never in the memory of man, been visited by the disease; and these places under Ottoman rule, and consequently peculiarly exposed to the introduction of a contagious disease—places such as Rhodes, Cyprus, Mytilene, Scio:

"Quoiqu'il y ait beaucoup de navires avec des merchandises et des passagers qui ont subi leur quarantaine à Scio avec patente brute de cholera, et que beaucoup de ces passagers soient morts de cette maladie pendant les vingt-cinque dernièrs années, aucun des employés du lazaret, ni des habitants de la ville, n'a été atteint de cholera ou d'autre mal contagieux."

The Vice-Consul at Mytilene reports:

"Providence has saved this town, and the other inhabited parts of the island, from the various scourges which have decimated many parts of Europe during the present century." Adding: "Should, however, Mytilene unfortunately be visited by what can really be considered an infectious disease, the havoc, owing to the accumulation of putrid filth in the streets and open spaces, would be awful."

Of Cyprus it is stated:

"Les habitants de l'ile n'ont jamais été atteint des maladies pour lesquelles un régime de quarantaine est imposé."

Another important fact afforded by the same documents is, that quarantine, even when rigidly enforced, though there has been for a long period of years an absence of plague, yet has not kept out other diseases of the contagious nature of which there is no question, such as small-pox, and other exanthemata. In Malta, for instance, we are assured on good authority, that in the short space of seven years—1829–1835—in spite of quarantine regulations for their exclusion, that island was twice invaded by small-pox, one epidemic proving fatal to 1500 persons out of a population of 114,000; and also by measles, scarlatina, and hooping cough. And there are other instances recorded of the like kind, which we need not specify, as none of them are more remarkable than this of Malta, where the quarantine system has been so regularly enforced, and under more favourable circumstances as to efficiency than almost anywhere else.

Were we not apprehensive of overloading our pages with facts, we might be tempted to give some of the very many recorded in these documents in confirmation of what we have pointed out as remarkable; and in the instances of yellow fever and of cholera, as well as of plague, showing very strongly, as regards the former, that whilst quarantine measures cannot, that is, have not, kept it out, yet when cases of it have been landed, the disease has not spread.

Besides the information collected in these abstracts bearing immediately on the question of quarantine, the value of which it would be difficult to over-estimate, there is to be found in them many observa-

on the south-eastern coast, all the inhabitants being in the plot, Macaulay informs us: "It was a common saying among them, that if a gallows were set up every quarter of a mile along the coast, the trade would still go on briskly."—Hist. of England, vol. v. p. 52.

tions well deserving the attention of Government, and of the English people generally, respecting our mercantile marine, showing not only the evils of over-crowding in the production of disease, and more especially in the spread of contagious and infectious diseases, but also of the neglect of ordinary sanitary measures in the impairment of the health and efficiency of the crews. Dr. Smith, writing from St. Domingo, states:

"British vessels frequenting Port-au-Prince are, with rare exceptions, very filthy and hygienically bad in respect to their internal sanitary arrangements. The forecastles, where the men are lodged, are generally unwholesome, while the bedding, &c., are dirty and unaired."

Another extract we must give on account of its importance:

"The utmost importance is attached by Professor Bo [of Genoa] to the necessity of improving the sanitary condition of mercantile ships generally, and also of their crews, most of the sickness of such vessels, on arrival, being traceable, in his opinion, to the faulty arrangements on board. On the important subject of the accommodation for the men, he alludes to the great superiority, in point of wholesomeness and comfort of the deck-houses, in most Dutch and American ships, over the ordinary berths in or under the forecastle. They are more easily kept dry, and are, of course, much better ventilated. Nor can the space be encroached on by the cargo, or be tainted with the foul smells either from it or from the hold. The men are more promptly at their posts when suddenly called on deck; and the change of temperature between their sleeping-places and the outer air, a point of no small moment for the preservation of health, is much less considerable. It is a great advantage, also, to have the galley close at hand: their food is better dressed, and their berths are kept warmer and drier in cold and stormy weather. The galley in deck-house ships is invariably very superior to the galley in ordinary merchantmen. There is too often, says Dr. Bo, a marked contrast between the clean and smart look of the outside of many merchant vessels, with the gay and handsome cabins of the officers and passengers, and the dingy slovenliness and discomfort of the quarters of the crew and the foul pollution of the hold. Dr. Bo alludes, also, to the defective clothing of merchant sailors as a frequent cause of sickness and bad health among them. The state of the provisions and of the water-supply on board is another subject of great moment for their welfare. He is of opinion that scorbutic and other cachectic diseases are often due to the impurity of the water, which in many cases he has found to be quite unfit for use." (p. 18.)

There is, also, in these abstracts much that is instructive relative to the condition of sea-ports, in various parts of the world, very deserving of attention and reflection, and especially of those intent on the discovery of the causes of disease, and too often entering on that most difficult subject with a confidence founded only on a very limited experience, and the stronger because so limited.

We cannot quit these documents without expressing our gratitude to the Committee of the National Association, and more especially to the Honorary Secretary of the Sub-committee, to whom we believe we mainly owe them. They do infinite credit to the zeal and ability of Dr. Milroy, and sure we are that nothing but a high sense of duty and the importance of the subject could have moved him to undertake such a task, and could have stimulated him to persevere in the labour.

No candid person reading these documents but must feel convinced of the enormous shortcomings of the old systems of quarantine and of the innumerable evils and losses which they entail,* and of the absolute necessity as regards humanity, as regards the interests of society, as regards the interests of commerce, to have them either entirely put aside, or if, on due consideration, any quarantine measures be held to be necessary, only such should be attempted as are practicable and efficient, and which are likely to have the assent of competent judges of all nations. It should always be kept in mind that the mortality from ordinary diseases vastly exceeds that from epidemic diseases, taking the average as about 100 to 1;† that epidemic diseases themselves are commonly little felt where due attention is paid to sanitary conditions; and as to faith in quarantine for the exclusion of disease, how little ought that to be, reflecting that no preventive measures, however severe, have ever kept out the contrabandists, when tempted by high duties. It would, we fear, be too much to expect that a subject which has been so long under discussion, and on which there have been such opposite views, will soon be settled in the most satisfactory manner, on absolute truth or unquestionable data. We suppose we must rest satisfied if a compromise be made, and that if any quarantine be tolerated, it must be established on that policy, eliminating from it as much of the uncertain as possible, and freeing it as much as possible from that which is vexatious, and costly, and inhuman. As England has set the example of free-trade, and is an example to the world of government with rational freedom, should she not likewise be an example in this matter of quarantine? No nation has the same power of teaching by example, her colonies being situated in every climate, as it were expressly for the purpose in question.‡ And standing so high, how careful should she be to avoid making any false step. Never more, we trust, shall we hear of mistakes like those fallen into in the

* The pecuniary losses are incalculable. Dr. (now Sir John) Bowring, speaking in the House of Commons on the subject in 1841, stated his belief that the losses from quarantine in the Mediterranean alone were not less than two or three millions sterling a year. We learn from Dr. W. Burrell's able and very instructive Report on the Plague of Malta in 1813, which he considered—and we think justly—of indigenous origin, to have entailed, by the rigid cruel measures enforced to confine it, a cost of 232,531*l*.

† It is stated that "all the deaths by yellow fever which have occurred in New York, in Brooklyn, and at the quarantine station combined, within the past fifty years, amount to only six hundred—the same, in round numbers, as we have been accustomed of late to lose annually by small-pox alone."—Third Quarantine and Sanitary Convention, New York, 1859, p. 239.

Under the heading of "Lisbon," it is stated in the abstracts, that "the number of deaths on board vessels at sea from ordinary casual diseases—chiefly phthisis, chronic diarrhœa, hepatitis, apoplexy—exceeded in the proportion of 21 to 12 that from the diseases against which quarantine is specially directed;" and that "in all these cases a quarantine of several days was imposed." It is added, "A vessel from Sunderland and Hamburg, both having clean bills, were detained, for four and six days respectively, in consequence of a death from apoplexy during the voyage." Also that, "In none of the twenty-five vessels which were quarantined for the cholera had any sickness occurred during the voyage." (p. 8.)

‡ What valuable information might be obtained from these colonies were their Governors required by the Secretary of State to give in the Blue-books annually furnished by them a short statement of the chief epidemics which may have prevailed during the year, and also of any events bearing on quarantine which may have come under their notice. Information from foreign countries of the like kind might be required from her Majesty's consuls. Such information together would almost form a summary of the

treatment of the *Eclair*, befitting more a Neapolitan than a British Board of Health.

The length which this article has already reached will not allow us to do more than to briefly notice further the works prefixed to it. Those bearing the name of Dr. Gavin Milroy should be read by every one anxious to make himself acquainted with the subject of quarantine. We consider it a fortunate circumstance that an inquirer such as he is, is the honorary secretary of the sub-committee whose labours we have reviewed. We trust he will continue to hold that truly honourable appointment, and that his exertions will be continued in collecting facts, and through them enlightening the public.

The American works, the 'Proceedings and Debates of the Third and Fourth National Quarantine and Sanitary Convention,' held in 1859 and 1860 at New York and at Boston, are equally worthy of attention. They are highly creditable to the medical profession of the United States, and must be read with interest equally by those who concern themselves about quarantine and the even more important subject of internal sanitary legislation. In the pages of their proceedings a great amount of valuable information will be found, and numerous suggestions opening new channels for research. Their discussions, their debates carried on with earnestness, and displaying oratorical power of no mean ability, have not been unfruitful of result, especially of the third convention, ending as they did in the resolution supported by the votes of eighty-four delegates against six, that yellow-fever is incapable of being propagated from person to person, though in their opinion it may be by fomites. The facts adduced in support of the first part of the proposition were numerous, and to our minds tolerably convincing; but we cannot say so much of the arguments used in support of the latter part—that regarding fomites: "things, not persons." The arguments used were chiefly derived from experience obtained at New York, a city decidedly malarious, where the average yearly mortality is one in every twenty-five or twenty-six of the population, and where solitary, stray cases of yellow-fever are allowed to be of no rare occurrence. We apprehend the distinction made between "persons and things" will hardly be held to be logical; but, apart from this consideration, is not a wider inference or induction hostile to the doctrine? If yellow-fever could be introduced, as supposed, by fomites, and these acting at a certain distance and contaminating the air, how is it that Liverpool has escaped the disease, where at all seasons, in the height of summer as well as in the depth of winter, cargoes of cotton are arriving from the southern ports of the States (would that we could still call them United), one or other of which is so often the seat of the fever? How is it that in so many instances—many of them recorded in the documents before us—it has not spread in countries on both sides of the Atlantic, in which little or no

epidemics of the world. It is sad to think how little has hitherto been contributed by men in authority, whether governors of colonies, consuls, or ambassadors, to the advancement of natural knowledge, especially considering the means at their disposal and the abilities of the individuals, and what has been done by the same class of men in other countries having had their attention called to matters of the kind by the home governments.

effectual attempts have been made to confine it to the spots where it broke out?

It is to be hoped that these conventions will be continued, and that aiding, as they cannot fail to do, if conducted in the true spirit of inquiry, unrelaxing exertions in this country carried on in the same good cause, the results ultimately will be successful. If the unity of Italy and an enlightened system of constitutional government be established, the greatest obstacle will be removed to the forming an international sanitary code of the kind we have hinted at, one founded on principles of hygiène, and equally directed to the removal of common noxious influences and to the mitigation of epidemic atmospheric ones, which in action have with some truth been called "the voice of an inexorable law, unmistakeably proclaiming man's sinful negligence of the laws of health and of life."

Review III.

1. *De la Pluralité des Maladies Vénériennes.* Par M. J. Rollet, Chirurgien-en-Chef de l'Antiquaille, Lyon.—*Paris*, 1860. pp. 76.

On the Plurality of Venereal Affections. By M. J. Rollet, Chief Surgeon at the Hospice de l'Antiquaille, Lyons.

2. *A Practical Treatise on Diseases of the Urinary and Generative Organs in both Sexes.* Illustrated with Woodcuts and Coloured Plates. By William Acton, late Surgeon to the Islington Dispensary, formerly Externe at the Female Venereal Hospital, Paris, &c. Third Edition.—*London*, 1860. pp. 595.

3. *Récherches sur le Chancre Primitif, et les Accidents Consecutifs, produits par la Contagion de la Syphilis Secondaire.* Par Dr. A. Viennois, Ex-Interne des Hôpitaux de Lyon.—*Paris*, 1860. pp. 144.

Researches on Primitive Chancre, and the Results produced by the Contagion of Secondary Syphilis. By Dr. A. Viennois, Ex-Interne at the Lyons Hospitals.

4. *On Different Forms of Primary Syphilitic Inoculation; and on Secondary Syphilitic Inoculation.* By Henry Lee, Surgeon to King's College Hospital, and Senior Surgeon to the Lock Hospital. ('Medico-Chirurgical Transactions,' vols. xlii. and xliii.)

5. *Die Beobachtung und das Experiment in der Syphilis. Beiträge zur Pathologie dieser Krankheit.* Von C. von Hübbenet, M.D., Prof. der Chirurgischen und Syphilitischen Hospital Klinik, &c. &c.—*Leipsic*, 1859. pp. 81.

Observations and Experiments connected with Syphilis. By Dr. C. von Hübbenet.

6. *The Modern Treatment of Syphilitic Disease, both Primary and Secondary; comprising the Treatment by a Safe and Successful Method, &c.* Fourth Edition. By Langston Parker, F.R.C.S. —1860. pp. 403.

7. *A System of Surgery.* Edited by T. Holmes, Esq. Article "Syphilis."—*London.*

8. *De la Contagion Syphilitique.* Par ALFRED FOURNIER, Docteur en Médecine, &c.—*Paris,* 1860. pp. 130.
On Syphilitic Contagion. By Dr. ALFRED FOURNIER.
9. *A Treatise on Syphilis in New-born Children and Infants at the Breast.* By P. DIDAY, Surgeon to the Hospice de l'Antiquaille, Lyons. Translated by G. WHITLEY, M.D., New Sydenham Society.—*London,* 1859.
10. *De la Transmission de la Syphilis par la Vaccination.* Par A. VIENNOIS, Interne à l'Hospice de l'Antiquaille à Lyon. ('Archives Générales de Médecine,' 1860.)
On the Transmission of Syphilis by Vaccination. By Dr. A. VIENNOIS.

THE history of the progress of the knowledge of venereal diseases during the last century presents us with three tolerably well-marked periods. During the first we have the Hunterian doctrine prevailing —viz., that the various forms of syphilis and gonorrhœa depend upon one and the same poison. If any doubt still remain, observes Hunter, with respect to the two diseases being of the same nature, it will be removed by considering that the matter produced in both is of the same kind, and has the same properties, the proofs of which are that the matter of a gonorrhœa will produce either a gonorrhœa, a chancre, or the lues venerea; and the matter of a chancre will also produce either a gonorrhœa, a chancre, or the lues venerea.

In support of his opinions, Hunter relates the following experiment performed upon himself:—Two punctures were made on the penis with a lancet dipped in venereal matter from a gonorrhœa, one puncture was on the glans, the other on the prepuce; on the third day there was a teasing itching on those parts, which lasted until the fifth day. Upon this day the part of the prepuce where the puncture had been made was redder, thickened, and had formed a speck. In another week this speck had increased and discharged some matter. There seemed also now to be a little pouting of the lips of the urethra; there was also a sensation in it in making water, so that a discharge was expected from it; a fortnight from the time of the inoculation on the glans there was considerable itching, and three days later a white speck was observed where the puncture had been made. This speck, when examined, was found to be a *pimple* full of yellowish matter. The chancre on the prepuce broke out several times after it healed up; that on the glans never did break out again, but for a considerable time it had a bluish cast. Ulceration of the tonsils, and copper-coloured blotches on the skin followed these inoculations, and the time the experiment took from the first infection to the complete cure was about three years. This experiment of Hunter's has been explained, and explained away in many different ways. But it has always been left, like the remaining piece of a Chinese puzzle, an awkward fact, which could in no way be made to fit in with the theories of syphilis of the second period to which we shall refer.

The usual way in which Hunter's experiment has been accounted for, has been by supposing that a concealed chancre existed in the urethra which furnished the inoculated poison. In order to clear up this point, we must anticipate some circumstances which will be more fully dwelt upon in the concluding part of this review. It will there be shown that the observations of modern pathologists, both in this country and abroad, have established the fact that the sore which produces constitutional symptoms, which Hunter's evidently did, does not secrete well-formed pus. It is impossible, therefore, that we can conceive that Hunter mistook the secretion of an infecting sore for the discharge of a gonorrhœa. The fact then still remains, that Hunter inoculated what he believed to be gonorrhœal matter (and he would naturally select a well-marked case), and produced local and general syphilis. This fact is in direct contradiction to all the teaching of the second period in the history of the progress of syphilis to which we shall allude; and, moreover, the kind of sore produced and the period at which it appeared are at variance with all that was generally, if not universally, received for so many years as the result of M. Ricord's influential and wide-spread opinions.

The accuracy of Hunter's experiments and observations forbid us for a moment to conceive that the results of his case are not fairly and fully given. We might refer to some of his other experiments, and see the care and accuracy with which they are recorded, although it may be they have, in some instances, been misunderstood by his successors: we will adduce one instance only. "A man," says Hunter, "had been affected with the venereal disease a long time, and had been salivated, but the disease broke out anew. He was taken into St. George's Hospital affected with a number of pocky sores, and before I put him under a mercurial course, I made the following experiment: I took some matter from one of the sores upon the point of a lancet, and made three small wounds upon the back where the skin was smooth and sound, deep enough to draw blood. I made a wound similar to the other three, with a clean lancet, the four wounds making a quadrangle; but all the wounds healed up, and none of them ever appeared after." Now, this experiment of Hunter's has been repeated a very great number of times, and always with the same result; and the conclusion drawn from the experiments by Hunter's successors has been that secondary syphilis is not inoculable. This proposition —namely, the non-inoculability of secondary syphilis — was one upon which Ricord and his followers based many of their opinions, and one upon which a course of lectures was recently delivered at the Medical Society of London. But what was Hunter's own conclusion from his experiments? It was that the matter derived from a secondary syphilitic sore was "not capable of acting in some respects *on the same body* or same state of constitution as that matter does which is produced from a chancre."* How correct and careful Hunter's conclusion was, and how erroneous the conclusions of those who thought they were following in his track, we shall hereafter attempt to demonstrate. It will not, then, explain Hunter's experiment just quoted either to say that there was a chancre in the urethra or that his observation was not accurate. The stubborn facts which he

* Hunter on the Venereal, p. 292, ed. 1786.

has narrated remain, although they have been ignored or explained away, during some thirty years, by those who profess (although not always correctly) to be Ricord's followers.

For an explanation of Hunter's experiment we must again anticipate somewhat of that which will be more fully considered in a subsequent part of this article. Ricord showed, by a great number of experiments, repeated in various ways, that the inoculation of gonorrhœal matter on the skin was followed by no result, and he concluded that it was, therefore, incapable of producing syphilis. A link in the chain of reasoning is here, however, wanting. It has now been demonstrated that the secretion of one kind of syphilitic sore is not capable of being inoculated, to use Hunter's expression, on the same body that produced it. But it would be manifestly absurd to conclude that this kind of sore was incapable of being transmitted by inoculation at all. Now, in one case, as in the other, those only have, as a rule, been inoculated who have already had the disease, and therefore it is clear that the fact of the non-inoculability of the secretion can only be proved, in one case as in the other, by trying it on patients not previously affected. That Ricord drew a clear line of distinction by his experiments between the ordinary gonorrhœa and one form of chancre every one must admit; and all praise is due to him in this respect. But we say that it is not absolutely proven that a discharge from the urethra (independent of any sore) cannot be inoculated, any more than it is proved that the secretion from an indurated sore cannot be inoculated. In both cases the fact must be tested by observation or experiment upon patients who have not previously been affected. The secretion from a primary or secondary syphilitic sore produces no effect when inoculated upon the same body; we might naturally, therefore, expect that it would produce no effect when applied to the urethra of the same patient. But it does not follow that this same secretion will produce no effect when applied to the urethra of another patient, nor that the result of that application may not in its turn be again communicated; nor are we altogether wanting in facts in illustration of this subject. Cases* have recently been published in which the contact of a secretion, which undoubtedly gave rise to an infecting sore, also produced in patients before uncontaminated a discharge from the urethra. This discharge was observed to differ from the ordinary discharge of a gonorrhœa, and is described by Mr. Henry Lee as being "more viscid and tenacious in its nature, in not being accompanied by the same amount of ardor urinæ, in its short duration, and often abrupt termination."

What, let us now ask, would be the effect of inoculating *this* secretion, produced by the contact of matter which had also produced an infecting sore? What would be the result of inoculating this upon a patient untainted by syphilis? To this important question we have no recent experiments which will furnish an answer, but it appears more than probable that the solution was furnished many years ago by Hunter's experiment upon himself; and we feel sure that the

* Medico-Chirurgical Transactions, vol. xliii. p. 67.

truthfulness of Hunter's description, and the accuracy of his facts, as far as his experiments go, will hereafter be fully acknowledged.

During the second period which we have marked out the doctrines of Ricord were triumphant, and were represented in this country by Mr. Acton's work, and more recently by Mr. Maunder's accurate and careful translation of Ricord's lectures.

Instead of one syphilitic poison which gave rise alike to gonorrhœa and the various forms of syphilis, M. Ricord very clearly demonstrated the existence of two poisons, one of which would produce gonorrhœa, and the other syphilitic ulceration; this ulceration, which Ricord so carefully and accurately represented and described, always, or with rare exceptions, follows a very definite course. The diseased action commences within twenty-four hours of the application of the poison. The skin is red and slightly raised; a vesicle forms, and then a pustule; and the sore left when this breaks has the well-known characters of the soft or suppurating chancre.

It appears that neither Ricord nor any of his followers recognised for many years any other form of inoculation than the one now described. Although much stress was laid upon the different physical characters of two classes of chancres when fully developed, yet even as late as the year 1858, M. Ricord describes them as originating nearly in the same way, as being alike in having no period of incubation, and as originating of necessity by contact from a similar primary syphilitic disease. In these and most other particulars Ricord is followed by Acton. Now, in all the experiments performed by Ricord, his conclusions were deduced from experiments made on patients already affected; and the results which would follow similar experiments in those not thus affected were entirely overlooked. One kind of disease only was observed, and that as it occurs in patients whose constitutions were already under the influence of previous disease. It is now known that such patients are seldom liable to take syphilis which has infected their constitutions, a second time; and that when it does occur, it assumes a very modified form. So that, after all the experiments that have been made, and after all the discussions which they have provoked, it appears that Ricord and his followers gave their attention exclusively to one kind of syphilitic inoculation, and that that kind was one which produced only a local disease, not followed by any constitutional symptoms.

The foregoing observations apply only to M. Ricord's experimental illustration of the subject. Practically he distinguished very accurately the different kinds of chancre, and laid down with great clearness the appropriate treatment for each. These practical distinctions are followed by Mr. Acton, and where M. Ricord stops, Mr. Acton stops also. In the review of the second edition of Mr. Acton's work (*see* Review, July, 1851, pp. 162 *et seq.*), we had occasion to remark how closely Mr. Acton had followed M. Ricord; and we are still compelled to treat what we find in the third edition of Mr. Acton's work as the opinions, by no means always acknowledged, of M. Ricord. We will give an illustration taken at random. M. Ricord says, in his 'Leçons sur le Chancre':

"Le caustique employé doit être sur toute la surface du chancre et un peu au-delà. . . . .

"J'ai à vous proposer un agent nouveau, d'une merveilleuse efficacité, et à l'abri de tout reproche. Ce caustique, c'est l'acide sulphurique uni à la poudre de charbon vegetal, dans les proportions necessaires pour former une pâte demi solide." (pp. 50–51.)

Mr. Acton says, after a quotation from Ricord, which he *does* acknowledge:

"The caustic should be spread over the whole surface of the chancre, and a little beyond it. . . . .

"The caustic I prefer is sulphuric acid combined with powdered vegetable charcoal, in the proportions necessary to form a semi-solid paste. This paste applied to chancres by means of a little box wood, glass, or ivory modelling tool, to be purchased of artists' colourmen, immediately dries and forms a black crust, which adheres to the tissues, and is one with them, only falling off several days after the application, generally at the end of a fortnight." (pp. 304, 305.)

In some instances, indeed, it would have been much better if Mr. Acton had followed the progress of his teacher more accurately. Thus,

In the year 1838, M. Ricord writes, in describing the development of a chancre:

"Dans les premiers vingt quatre heures, le point piqué comme dans la vaccine, rougit; du second au troisième jour il se tuméfie un peu et présente l'aspect d'une petite papule qu' entoure une auréole rouge; du troisième au quatrième jour, l'épiderme soulevé par un liquide plus ou moins trouble prend la forme souvent vesiculeuse, offrant, à son sommet, un point noir, résultat du dessèchement du sang de la petite piqûre; du quatrième au cinquième jour, la sécrétion morbide augmente, devient purulente, la forme pustuleuse se dessine, et son sommet, en se déprimant lui donne un aspect ombiliqué qui la rapproche de la pustule de la petite vérole a cette époque, souvent l'auréole, dont l'etendue et l'intensité s'étaient accrues, commence à s'éteindre ou à diminuer, sur tout si la maladie ne fait pas de progrès; mais à partir du cinquième jour, les tissus sousjacents, qui souvent n'avaient encore subi aucune influence, ou étaient seulement légèrement œdemateux, l'infiltrent et durcissent par l'épanchement d'une lymphe plastique qui donne au toucher la resistance, la sensation élastique de certains cartilages." (p. 89.)

In the year 1860, Mr. Acton writes on the same subject:

"During the twenty-four hours succeeding the operation, the inoculated point becomes red; in the course of the second and third days the surrounding parts are slightly swollen and assume a papular appearance, or already traces of a vesicle are seen on the summit; on the third or fourth day a fluid, which is more or less transparent, is observed beneath the epidermis, and a distinct vesicle becomes apparent where the papula previously existed, and a dark dot is seen in the centre, owing to the coagulation of the blood which had escaped through the puncture of the lancet; from the fourth to the fifth day the vesicle assumes a pustular character, and a distinct depression is seen in the centre, so that it represents very distinctly at this period the small-pox pustule. The red areola, which has been hitherto gradually augmenting in intensity, now as gradually fades away; and the cellular tissue, which was slightly œdematous, becomes infiltrated with plastic lymph." (p. 270.)

From the observations which we have previously made, our readers will be aware that the plastic lymph which gives to the chancre its characteristic hardness does not occur in the kind of chancre here described. The characters of the two kinds are here unfortunately mixed together. And in Ricord's later lectures and works he has carefully omitted any mention of this effusion, which upon his own showing could not take place a second time in the same subject; and which, had he produced it for the first time, in any case would have been followed by secondary symptoms. This blunder in Ricord's early writings is not only copied without acknowledgment by Mr. Acton, but is maintained for years after the real author had abandoned it.

Nor can we say more in favour of the illustrations contained in Mr. Acton's work. These are not in accordance with English taste; and we doubt whether they would not be too disgusting even for the French school of medicine.

The third period in the advance of the knowledge of syphilitic diseases is by far the most important, and is indicated by the appearance of most of the works which we have placed at the head of this article. We have here distinctly recognised *in their origin* the different forms of syphilitic disease, and especially the difference in the mode of development of the infecting and of the non-infecting sores. It has been shown by Mr. Lee and M. Rollet that that chancre which infects a patient's system commences by a pimple, an abrasion, or a crack, around which the specific thickening takes place: a *pustule* is no essential part of this process, and when it does occur it appears to depend upon some accidental circumstances, and in all cases the sore soon ceases to yield a purulent discharge, and the characteristic induration, called by Mr. Lee "the specific adhesive inflammation," is established. The secretion, therefore, as noticed by Professor C. von Hübbenet, as well as by the authors we have already quoted, furnishes a diagnostic test of the kind of disease. This addition to our means of distinguishing different kinds of primary syphilitic sores is very valuable; for ever since the induration was proposed as the one test of an infecting sore, there have been many experienced surgeons who have openly expressed their dissatisfaction with this mode of diagnosis, and even those who have most strenuously advocated it have themselves allowed that the induration might be simulated by many accidental causes; and we are further informed that in one particular kind of infecting sore it is absent altogether.* It has been, moreover, shown that this kind of primary syphilis differs in its origin from the kind which was described by Ricord as the only mode of development of primary disease, in regard to the time at which it appears after inoculation—a period of incubation has been shown to exist between the time of contact of the poison and the appearance of any local disease. This varies from a few days to as many weeks; and it is not unimportant to observe in passing, that in Hunter's experiment first quoted, and which has been ignored by so many subsequent writers, both the

* See Holmes' Surgery, vol. i. p. 402.

papular form of the disease in its origin, and also the period of incubation, are carefully noted.

The most interesting circumstances connected with this form of disease are doubtless those which refer to its inoculability, and the way in which this may be modified by various circumstances. The following extracts are from the article Syphilis, in 'Holmes' Surgery.'

"A medical student having become diseased for the first time, inoculated himself on the thigh, and presented himself a few days afterwards. The original sore was then surrounded by a circular, hard, button-shaped induration, and yielded very little secretion from its surface. The inoculation was at first a red pimple containing a point of white lymph in its centre, but it soon assumed the characters of the original sore. In this case the inoculation was performed as soon as the disease had manifested itself.

"From this instance it is evident that a sore affected with the specific adhesive inflammation will yield a secretion capable of being inoculated during its earliest stage, before its specific action upon the patient's system has been developed, and that the result of that inoculation may be a specific hardened sore accurately resembling the original. . . . . . .

"The inoculation, then, of the secretion of a sore affected with specific adhesive inflammation may take place, but is not easily performed when once the patient's system has been affected with syphilis. When successful, the results are different from those which follow the inoculation of the secretion from naturally suppurating sores. In the latter case each puncture produces a pustule, which will, by repeated inoculation, produce its like a great number of times. In the former the inoculation, as a rule, fails, and succeeds only under circumstances of accidental irritation. Even then it can be repeated a very limited number of times, and the results obtained are of comparatively trifling description.

"There is, however, reason to believe that these same inoculations, if practised upon a patient whose system is not already affected with syphilis, would give rise to well developed primary infecting sores.

"It is, then, clearly shown that the primary form of this disease, which may be readily inoculated at first, after the lapse of a short time loses, in a great measure, its inoculability upon the patient himself, although by artificial means this is capable of being called into fresh activity for a certain period." (pp. 404, 409.)

It appears, then, evident that in the so-called process of syphilisation the series of actions which have been produced have not been of the same nature as that which produced the constitutional disease; at least if we accept the doctrine of the indurated chancre being the disease which infects a patient's constitution. The two morbid actions are in fact so distinct that they have been considered by MM. Diday and Rollet as not only separate diseases, but as depending upon different poisons.

The contagious ulcer known by the name of simple or chancroïd sore, says M. Rollet, is inoculable almost an indefinite number of times on the patient who has it: a simple chancre may be reproduced on the same patient by a series of inoculations with scarcely any limit. Those who practised syphilisation have often made a hundred inoculations, all of which succeeded. One gentleman had the courage, in a sorry affair of this kind, to give himself two thousand chancres.

It is only a local disease that can be thus transmitted: contagious diseases that affect the whole of a patient's constitution cannot, in their very nature, be thus transported from one part of the system to another. Such a translation can necessarily be effected only where a *part* of the body is affected. It is this transmission which explains at once the pathology of those specific suppurations in the lymphatic vessels and in absorbent glands which are so frequently observed. On the contrary, the syphilitic ulcer, the primary sore, is never inoculable on the patient who has it. It may be communicated from one individual to another, but it is never "*auto-inoculable.*" The attempts which have been made to inoculate the syphilitic chancre upon the patient himself have always failed; and they have been tried a great number of times in the Hôpital du Midi, at the Antiquaille, in England, in Germany, everywhere without result. When anyresult has followed, it has been a slight and insignificant inflammation only at the inoculated point. This then is a fact well worthy of notice, and one which has been established by observers sufficiently numerous and sufficiently trustworthy to place it beyond a doubt.

We can now understand how it is that several successive syphilitic chancres do not develop themselves upon the same individual; how it is that the lymphatic vessels and glands do not become inoculated from a real chancre; and even in those rare cases where the syphilitic bubo does suppurate, the wound which results does not secrete an auto-inoculable pus.

Not only is the syphilitic chancre (M. Rollet throughout uses the term syphilitic to express the infecting variety of the disease), not only is this incapable of being transmitted from one part to another of the patient who has it, but it is not inoculable upon any syphilitic subject at any period of his disease, whether primary, secondary, or tertiary.

Thus the syphilitic chancre is as far from resembling the ordinary venereal ulcer as it approximates in its characters to other general virulent diseases. It resembles for instance the cow-pox, which is not inoculable on the patient who has it, or upon another individual previously vaccinated. (pp. 19–22.)

M. Rollet thus maintains that this chancre is never (*à aucune époque*) auto-inoculable, but it appears from a quotation which we have previously given, that during the early period of the disease the secretion may be again inoculated upon the same individual. This fact, as well as the non-inoculability of the secretion at a later period, was established in a paper published by Mr. Henry Lee in the pages of the 'British and Foreign Quarterly Review,' as far back as the year 1856. (No. xxxvi. p. 500.)

Dr. Alexandre Viennois, a pupil of M. Rollet, has well recorded his master's labours in his thesis on the results of secondary syphilitic inoculation. Dr. Viennois says that the records of science had long contained certain observations of nurses contaminated by the children whom they suckled; of adults contaminated from mouth to mouth; and of infants inoculated by vaccination. A series of inoculations had been recorded by Wallace, Waller, Vidal, Rinecker, and others, in which the product of secondary affections had been used. It was only necessary to study these facts—that is to say, to interpret them correctly, in order to arrive at the truth, and to substantiate one of the most important laws that was ever announced in the science of syphilography.

But first it was necessary to get rid of the old Hunterian error, reproduced and exaggerated by Ricord; an error general at that period. Hunter and Ricord both studied primary syphilis in its effects when inoculated upon the patient himself: as well might they have studied the development of the vaccine vesicle by inoculating the secretion which it afforded a second time upon the same subject, or upon another patient previously vaccinated. M. Rollet, assisted by Dr. Viennois in May, June, and July, 1858, studied separately the effects of the contagion of the primary chancre; of the chancre produced by the contact of secondary syphilitic sores, and especially of the secondary chancre on the breast; of the inoculated chancre on the mouth; and finally, of the chancre produced by artificial inoculation. Some of the results of these investigations must conclude our present article. The first case mentioned by Dr. Viennois we will give in detail as the type of many others.

Mme. X. was seen by her medical attendant, together with her husband, in May, 1857. She then had a papular syphilitic eruption over the body. She had lost her hair and had scabs on her head, with enlargement of the sub-occipital gland; mucous tubercles existed in the mouth; there were greyish patches on the tonsils, but no disease in the parts of generation. The husband was in perfect health, and had never had any venereal disease.

This lady had been delivered of a healthy child on the 30th of October, 1856. She had had her breast drawn several days in succession by a woman in the neighbourhood, who was paid for her attendance. In January, 1857, a crack formed on the left nipple, which was followed by enlargement of the glands in the axilla. Upon examination, a large cicatrized induration was found at the base of the left nipple, and in the armpit were two glands enlarged to the size of nuts, but not painful.

The woman who had drawn this lady's breast was found to have had secondary symptoms, and at the time she was attending Mme. X. had had mucous tubercles on her throat for six months.

The lady's child was also affected with secondary syphilis through an ulcer on its lip.

Fifteen other cases are then tabulated, in which an indurated sore was communicated to the breast of the nurse by the child. The transmission in all these cases was supposed to have been by means of the secondary disease in the infants, and quite independent of any primary syphilis.

An important fact maintained by M. Rollet, Dr. Viennois, and also described at length by Mr. Henry Lee in vol. xliii. of the 'Medico-Chirurgical Transactions,' is that the secondary syphilitic inoculation in such cases exactly resembles the primary indurated chancre.

M. Diday has indeed endeavoured to establish the fact that there are differences which will allow a primary inoculation to be distinguished from a secondary one. He says that the period of incubation produced by the contagion of secondary syphilis is longer than that which precedes the primary chancre. In reply to this M. Rollet asserts that in the twelve cases which have been collected, where artificial inoculation from a secondary affection has taken place (in persons not previously affected) the mean period of incubation was twenty-six days. In the three cases in which inoculations from primary indurated sores have taken place in like manner, the mean period was twenty-four days.

M. Diday, however, affirms that in chancre produced by secondary syphilis, the ulceration does not occur so soon (after the appearance of the adhesive inflammation); that the ulceration, when it has taken place, remains more superficial; that it disappears in a shorter time; that the induration which accompanies it has less consistence, and is of less extent than when arising from the inoculation of primary chancre. M. Diday, moreover, maintains that the constitutional syphilis derived from a secondary affection is longer in developing itself after the first appearance of the chancre than when resulting from a primary disease; that the symptoms produced are less severe and of shorter duration; and finally, that they are not again contagious. These opinions appear to M. Diday to account for the epidemic of the fifteenth century having gradually abated. He believes that the transmission of the secondary affections reproduced the disease in a modified form, and thus secured the patients against the more formidable results of direct primary inoculation, and he further concludes that, practically, prolonged and severe specific treatment might be dispensed with in cases where from the history of the case or other means there were reasons for believing any particular case of disease to have been derived from a secondary affection.

The subject which will present the features of the greatest interest to the profession in England with regard to the inoculability of secondary syphilis is undoubtedly its relation to vaccination. Dr. Viennois, both in the 'Archives of Medicine,' and also in his work on secondary inoculation, cites many cases in which he believes that the syphilitic poison has been communicated together with the vaccine. He believes that if the lymph of the vesicle only be taken, that the cow-pox alone will be communicated, but that if the blood of a person infected constitutionally with syphilis be inoculated at the same time as the vaccine lymph, then that both diseases may be communicated. Under these circumstances the cow-pox would develop itself first, and after the usual period of incubation, the syphilitic tubercle would appear on the vaccinated part, and be followed by secondary symptoms.

The following are the conclusions arrived at by Dr. Viennois:

1. Syphilis has been observed to follow vaccination a great number of times, and that from nearly the commencement of vaccination, by authors worthy of credit,—French, English, German, Italian, &c.

2. When a person is vaccinated who has latent syphilis, then the

syphilis may develop itself under the influence of the vaccination. Such affections have been observed a great number of times, and consist of constitutional affections of various kinds, such as the papular, the vesicular, or the pustular eruptions. It is then never a primary chancre that is produced upon the inoculated point.

3. If vaccine matter *alone* is taken, the vaccine vesicle alone is produced.

4. If some blood be taken at the same time from a subject who has constitutional syphilis, the same inoculation may transmit two diseases—the vaccine poison with lymph, the syphilitic poison with the blood.

5. In those cases of which Dr. Viennois says there are numerous examples, the vaccine vesicle is developed first, because its natural period of incubation is shorter, and its natural duration less.

6. The first change by which the syphilitic inoculation is recognised is an indurated ulcer accompanied by multiple indolent bubo; in fact, with all the characters of a primary syphilitic chancre. This succeeds the vaccine vesicle, and occurs on the same spot.

7. After this primary chancre, secondary symptoms appear in their ordinary course, as they would after a primary disease communicated in any other way.

8. When the two poisons are mixed together and intentionally inoculated, one poison does not destroy the other, but each produces its own specific effect, at its own proper time.

9. The vaccine fluid is therefore under these circumstances but a vehicle for the syphilitic poison, which may dilute and divide it, but in no way interferes with its specific action.

10. It is therefore highly important that vaccine matter should never be taken from a suspected patient, and in new-born children that the lymph should not be taken before the age at which hereditary syphilis ordinarily shows itself.

11. If circumstances should necessitate the use of lymph from a patient of whose condition the surgeon cannot be sure, he ought to take the greatest care to take the pure lymph only, without any admixture of blood or of other fluids.

12. Under no circumstances ought a healthy person to be vaccinated from a patient known to be syphilitic.

13. These precautions are so much the more necessary, as from a single syphilitic patient a great number of persons may be vaccinated at a time, and may all have syphilis communicated to them.

14. The precautions now mentioned are sufficient to prevent any future misfortunes, inasmuch as syphilis is not communicated by the vaccine matter, but by the vaccinator; and the observance of them would completely disarm those who are opposed to vaccination.

Review IV.

1. *Ueber Idiotie mit besonderer Rücksicht auf das Stadtgebiet Salzburg. Pathologisch-Anatomische und Statistische Studien zur Naturgeschichte dieser Volkskrankheit.* Von Dr. F. V. Zillner, M.D.A.D.N. 1857. Verhandlungen der Kaiserlichen Leopoldinisch-Carolinischen deutschen Akademie der Naturforscher.—*Jena*, 1860.

Idiocy, with Especial Reference to Salzburg and its Suburbs, &c. By Dr. Zillner.

2. *Untersuchungen über die Entwickelung des Schädelgrundes im gesunden und krankhaften Zustande, und über den Einfluss derselben auf Schädelform, Gesichtsbildung und Gehirnbau.* Von Rudolf Virchow, Prof. der Pathologischen Anatomie, &c. &c., zu Berlin.—1857. 4to, pp. 128.

Development of the Base of the Skull, in Healthy and Diseased Conditions, and on its Influence on the Form of the Skull, &c. By Rudolf Virchow.

3. *Ueber den Cretinismus, namentlich in Franken, und über Pathologische Schädelformen.* (Vorgetragen in der Phys. Med. Ges. zu Würzb., 1851.) Von Rudolf Virchow, Prof., &c. &c. Gesam. Abhand., 891–939.

On Cretinism, particularly in Franconia, and on Pathological Forms of the Skull. By Rudolf Virchow.

4. *Ueber die Verbreitung des Cretinismus in Unterfranken.* Von Rudolf Virchow, Prof., &c. (Vorgetragen in der Phys. Med. Ges. zu Würzb., 1852.) Gesam. Abhand., 939–969.

On the Spread of Cretinism in Lower Franconia. By Rudolf Virchow.

5. *Zur Entwicklungsgeschichte des Cretinismus und der Schädeldifformitäten.* Von Rudolf Virchow. Ges. Abhand., 969–997. 1856.

On the History of the Development of Cretinism, and on Deformities of the Skull. By Rudolf Virchow.

Zillner begins his work with these words: "The natural history of idiocy is still very deficient." This deficiency, which all psychologists acknowledge, we have to thank him for having materially diminished. Under idiocy, he says, we usually reckon "every case of injury, defect, check, interruption, backgoing, or complete arrest of the development of the infant mind." This is a good practical definition, though it does not define the *extent* of the injury or defect. To the medical eye, however, idiocy presents itself as "every diseased condition of the cerebro-spinal system which (as a *sine quâ non*) is associated with an arrest or disturbance of the mental development."

In much, but not in all, of what Zillner says of the error of drawing too specific and too wide a distinction between idiotismus and cretinismus, we concur, and we think he has done right in including in one class cretins, idiots, and imbeciles, though admittedly the physical and psychical phenomena may differ considerably in the different classes, not only in degree, but in kind and origin.

Our own view of the matter is briefly this. All cretins are idiots, but all idiots are not cretins. Cretinism is, in short, a form of idiocy, of which the mental manifestations are to some extent peculiar, but which is recognised chiefly by physical characteristics, and which occurs with great frequency in particular localities, in which goître also nearly always, if not always, abounds, showing it to be under territorial influences, and therefore, as a *form* of idiocy, to be truly endemic. While, however, it occurs *with great frequency only* in such localities, isolated cases of cretinous idiotism are to be found here and there throughout the whole of our own country. Nor does it follow that every idiot in a cretinous locality is a cretin. We have personally had evidence that this is not the case. In such places are to be found, as well as the cretinous, the true microcephalic idiot, the hydrocephalic, the choreic, the epileptic, the paralytic, &c. In short, while in these localities territorial influences originate a form of idiocy, those social causes, from which other forms may be regarded as springing, are neither absent nor inoperative, but give the same results there as elsewhere.

We do not know a people among whom idiocy does not appear, nor do we believe that such will be found in or out of Europe. Ignorance of, or violation of, the laws of health, are practically universal, and one result is idiocy. In proportion to the non-observance of these, and more especially to the non-observance of such of them as relate to the management of infancy, so will be the frequency of this great calamity. Even the strictly territorial causes are under a certain control.

As a disease, then, idiocy is general—more or less frequent everywhere. Here and there it occurs with exceptional frequency, without assuming any particular type, and depending on the social condition and habits of the people, and not on the locality. Or it *may be* (and we believe it so is) that this social condition and these habits of a people dwelling in a particular place may be such as to render one type of idiocy exceptionally frequent. The degree of the ignorance and the character of the violation of the laws of health differing, so will the degree and character of the result differ.*

It is a mistake to regard idiocy strictly as *congenital, opposed to acquired* insanity. Practically this is not, and cannot, be done. It matters little whether the mischief has occurred during intra-uterine or the early period of extra-uterine life, *quoad* the result. In the majority of cases, an examination of the patient will not show the period from which the disease dates. Nay, there are many cases of dementia which could not be distinguished from idiocy without

* Whether we should use the term endemic in such cases is not quite clear. The derivation of the word—from ἐν, among, and δῆμος, a people, thus referring to the people rather than to the place—might justify it, though the *usus* might not accord its support.

the aid of the patient's history. This we have occasionally felt in examining fatuous men, thirty to forty years old, whose dementia dated from and was connected with the arrival of puberty.

Wherever the mind is obliterated, or injured, or impaired, before it has undergone any very appreciable development, be the cause what it may—territorial, social, or accidental—there we have idiocy. Whatever the theory, this we find to be the view habitually acted on, and we think it serves all useful purposes. It does not prevent idiocy from being contrasted as a disease of *privation* with dementia as a disease of *deprivation*—the contrast having a substantial, though not an absolute, accuracy.

We are so little acquainted with the statistics of idiocy, that any contribution directly or indirectly bearing on the subject is of value, while facts so carefully collected and analysed, as are those of Zillner, are peculiarly so. We find, from his researches in Salzburg, that out of 100 idiots, 40·5 are male, and 59·5 female. This differs from the proportion of the sexes in the general population of the place, which gives in 100, 47·3 males, and 52·7 females. We have therefore a somewhat greater tendency to this disease in the female than in the male sex; explained, perhaps, by the greater mortality of male children during the first year of infant life. Acquired insanity also shows a greater proportion of females, 100 cases giving 45 males, and 55 females.

An interesting connexion between crime and insanity occurs in connexion with the proportion of the sexes affected with idiocy. As just shown, the female sex has a greater liability both to this and to other forms of insanity. We understand Zillner to mean that this greater liability is more manifest in Salzburg than in the country generally. Now, it happens that crime among women there also exceeds the general average by five per cent., the exceptional excess in idiocy and madness having a corresponding excess in crime.

Again, as regards the social position of the parentage, he finds that a larger proportion of idiots belongs to the poor or lower class of the community than to the higher, more prosperous, and independent class. It is important to find figures supporting this, which we believe to be true in all countries. Poverty is undoubtedly a cause of idiocy. Dr. Jarvis well says, that it "is not a single outward circumstance, the mere absence of worldly goods." Poverty has a broad grasp, and affects life, health, intellect, and morality, as well as estate. The proportion of the two classes in Salzburg having been ascertained, it is found that 10,000 of the well-being give 25 idiots, while 10,000 of the poor give 64, a difference too great to be accidental.

The extent to which idiocy prevails is variously estimated—including only the more marked idiots, it reaches 3·16 in a thousand, or 1 in 316; but, if the imbecile, facile, and weak-minded* be included, it amounts to 6 in a thousand, or 1 in 166 of the population. Acquired insanity demands asylum accommodation for 1 in 692, but this,

* Minder Weltläufige.

of course, is below the figure which would represent the extent of the disease.

Goître affects 2 per cent. of the new-born, 6 to 9 per cent. of those at a school-going age, and 17 per cent. of men between the ages of twenty and twenty-two. Of 96 men and 184 women in adult life in the public charitable institutions, 21 per cent. of the men and 61 per cent. of the women were affected. It appears that idiots are not more liable to this disease than are others living under similar health-conditions, having as little exercise, being as much confined to the house, and being as imperfectly nourished.

The education of the people cannot be much neglected, since 98 per cent. can "read, write, and count."

With these facts as a preface, we shall proceed to analyse and comment on his important chapter on the Causes of Idiocy. In treating of these he divides his subject in the following manner:

1. *Constitutional*, family, or hereditary idiocy.
2. *Traumatic*, where the skull or its contents have been injured.
3. *Congestive* idiocy.
4. *Idiocy through exhaustion*, from deficient nourishment, &c.
5. *Toxic idiocy*, from the use of substances whose action is more especially on the nervous system.
6. *Malarial* idiocy.
7. Idiocy arising from other or accidental causes.

1. *Constitutional idiocy.*—Wherever, whether in the branches of a family or in a community, we find many cases of epilepsy, insanity, stammering or stuttering, chorea, defects of the senses, marked short-sightedness, deafness, dumbness, or such like ailments, there, according to Zillner, we may expect to find idiocy similarly abounding; and he quotes Dagonet, as having made the same observation in reference to Stephansfeld, near Strasbourg.

On turning to a former part of the work, we find that in Salzburg, to which his observations are confined, short stature, dwarfage, deformity, the toothless upper jaw, flat feet, knock-knees, and such-like affections occur in 24 per cent. of the men between twenty and twenty-five years; while in 25 per cent. of the military recruits, anomalies of the muscular system are found. Epilepsy affects 3 in 1000 of the population. Short sight is very frequent, and imperfect hearing in the young no rare occurrence; and defective articulation, as observed by Knolz, is common in all its forms.

In a community abounding in such physical defects as those described, we should be prepared to find psychical defects proportionally abounding. We have ourselves had several opportunities of witnessing this coincidence—a coincidence which our present knowledge could have predicted, since the conditions which originate the one set of defects are recognised as effective in producing the other—bodily and mental health being all under one set of laws, the violation of which imperils the whole health, though in one case the manifestation of the injury may be found in dwarfage or deformity, and in another in idiocy, madness, or, to a certain degree, in crime. The man who is the intelligent guardian of his bodily health, is, *ipso facto*, a good guardian of his mental. The reverse, at any rate, is wholly true.

We could ourselves have given many other bodily defects in curious association with idiocy, observed in the parents, brothers, sisters, uncles, aunts, or cousins of idiots; such, for instance, as the high-arched palate, short or unequal arms or legs, one testicle, club foot, hare lip, cleft palate, supernumerary toes or fingers, squint, &c. &c.

Zillner does not tell us to what extent the hereditary cause holds in his field of observation; but we are left to infer that it is there, as everywhere, largely operative. Of the effects of marriages of consanguinity he says nothing. Almost all writers assume such marriages to tell seriously against the offspring; but we are not in possession of much definite information on the subject. The papers of Bemis and Devay, though able and elaborate, are nevertheless deficient. So far as Salzburg is concerned, this probably plays an unimportant part. Since 1817, the increase of the population is as 100 : 160; and it was ascertained that out of 1000 inhabitants of the poor and middle class, only 356 were born in the place, and of the better class still fewer. It follows, therefore, that for forty years (1817 to 1857) there must have been a constant, large, and beneficial infusion of new and strange blood. The extent to which illegitimacy prevails undoes much of the good that might be expected to flow from this; the legitimate being to the illegitimate as 3 to 1. Married couples are few, only 103 in 1000 of the population, and they are not very fruitful. Marriages are contracted late in life; by men at thirty-three on an average, and by women at thirty.

It has been shown in our own country, and is capable of further proof, that illegitimate children are born under circumstances unfavourable to health and vigour. Their mothers are exceptionally liable to puerperal mania. They themselves are less viable, and the mortality amongst them is greater. Out of 1000 living-born children of each sex, in Salzburg, there died during the first year,

Boys . .	301	Legitimate.
Girls . .	238	
Boys . .	350	Illegitimate.
Girls . .	305	
	298	Mean.

The mean is an absolutely large mortality, and indicates weakly children at birth, or conditions after birth unfavourable to life. But the mortality of the bastards greatly exceeds the mean, and proves them to be at birth, or after it, still less fortunately conditioned.

2. *Traumatic idiocy*, where the skull or its contents have been injured by a blow, or fall, or pressure, or by any other such accident.

The passage of the child's head through the pelvis, and the pressure to which it is subjected in all labours, but especially in protracted ones, are alluded to under this head. We have ourselves seen many cases of idiocy which were undoubtedly referable to this cause, and several where it was stated that animation had been suspended at birth, or that instruments had been used in the delivery. Whether the eating of the fruit of the tree of *knowledge* enlarged man's head out of proportion to the other parts, and in like manner the head of his offspring, and so brings difficulty in parturition, and consequent calamities, we cannot say; but, so far as our own observation goes, idiocy arising from difficult or abnormal births is proportionally more frequent, and not less frequent among the lower and uneducated classes than among the higher.

Zillner points out with great truth that improper management of pregnant women, an ignorant, negligent, or interfering midwifery, a careless or rough treatment of the infant, as they raise the rate of infant mortality, so also do they become causes of idiocy, and of other defects and weaknesses. A proper understanding of the laws of health by the people generally, the recognition that child-bearing is a physiological process, and that the management of infancy ought to be, and can be directed by natural laws, would diminish this source of the greatest calamity which befalls man.

3. *Congestive idiocy.*—He regards the seat of the mischief as either in the skull bones, the brain-membranes, or the brain itself. With reference to the first, he states that of twelve skulls examined, the greater part showed a thickening of the bones, an obliteration or synostotic union of the sutures, or osteophytes. These accidents may have their origin in intra-uterine, or the early period of extra-uterine life; prefaced or accompanied by febrile symptoms, heat, and often redness of the scalp, convulsions, &c.

When the membranes are involved, he calls it meningeal idiocy; and when the brain-mass, cerebral idiocy. To the introduction of such terms into medicine many objections might be taken. Altogether, in this part of his work, we think Zillner has not had his usual fortune in the selection of terms.

4. *Idiocy through exhaustion.* (Erschöpfung.)—This is that common idiocy which follows hydrencephaloïd disease, so frequent and so fatal in our cities, so often traceable to incorrect views as to the feeding and general management of children, and in a great measure so truly a preventible disease. Very many cases of idiocy in this country we have traced to this origin; and the grave receives many, who make a partial recovery under the wiser treatment of the affection now common, but who never become strong, and, after months or a year, pass away before the extinction or impairment of mind has been acknowledged by those who always unwillingly make any such acknowledgment.

5. *Toxic idiocy,* from the use of substances whose action is chiefly on the nervous system. Zillner's remarks under this head are brief. He draws attention to this cause of idiocy, and specifies laudanum and brandy as coming within the class of substances referred to. In all our large towns sedatives containing opium are habitually given to children, chiefly to those of the lower class. The evil effects of these have been often pointed out, and have been well described by Marshall Hall. A sallow colour, lashless red eyelids, bloated lips, a puffed,

flabby, and wrinkled face, an aged and haggard aspect, a dull, stupid, idiotic expression, a shrivelled, puny, and emaciated body, are among the symptoms which he gives, and form a sad picture, which we have seen. If the child is not cut off by the supervention of active disease, imbecility or idiocy often remains as the sequel. Here, again, ignorance and error inflict upon the inoffending individual a fate worse than death, and burden society with a helpless member.

6. *Idiocy from malaria or vitiated air.*—Zillner treats this subject at considerable length, and with the ability which characterizes the whole essay. He shows that Salzburg is surrounded with all the conditions necessary for originating the marsh miasm. In a former part of the work he gives a most complete account of its climate, the characteristics of which he shows to be, a greater winter cold and less summer heat than would be inferred from its geographical position; considerable and rapid changes of temperature and barometric pressure; great difference of temperature, according to locality—on the sun and shade side of the mountain; great fall of rain; excessive humidity; much cloud and mist; heavy dews; wet springs and summers; prevalence of thick gloomy weather; and little movement in the air, with frequent calms. We have given his conclusions in detail, because in treating of such matters, sufficient attention has usually not been given to the consideration of climate.

The physical and geological peculiarities of the locality he describes fully, and they are certainly sufficient with such a climate to engender marsh miasm. Intermittent fever, in fact, though less frequent than it was formerly, is still an endemic disease, as are also enlargements of the spleen, and the other allied ailments.

He attempts to show a connexion between marsh fever and acute affections of the thyroïd gland, as Virchow has also very ably done. The history of epidemics furnishes him with proof, Thus, in reference to Salzburg, it is recorded that "between August and October, 1847, acute swelling of the thyroïd appeared along with intermitting fever;" and "in October, 1849, typhus, intermitting fever, erysipelas, swelling of the thyroïd, and scarlatina." It is observed, too, that conditions which lead to the manifestation of acute goître, often also lead to greater enlargements in chronic cases, although, as a rule, chronic goîtres increase in winter and attain their maximum in spring. Another argument rests on the season of the year at which these different diseases, between which it is attempted to establish an alliance, chiefly occur. Acute goître presents itself usually in warm weather, or summer, and so also do marsh fever, erysipelas, &c. The maximum of general disease in the community, however, is attained in winter and spring, indicating the leading character of the climatic constitution of Salzburg to be towards thoracic disease. Another interesting fact is observed in the hospital statistics, which we note, though not quite relevant to our subject. It appears that, as the population of the place has become more dense, so the proportion of hospital entries to the whole population has increased.

These malarious influences are especially active in the production of that form of idiocy which we call cretinism. The importance attached to them by Zillner and Virchow, we are convinced, is not too great. It is possible, nay, we think probable, that they are not identical with marsh miasm, which may exist in great force where goître and cretinism are absent. But it is certain that many of the conditions necessary for originating the one are required for the other. There is a growing conviction, that neither the temperature nor the hygrometric state of the atmosphere, nor the want of sunlight, nor the geological condition of the soil, nor the physical configuration of the locality, nor its vegetable productions, nor the qualities of its waters, nor the habits of its people, taken separately or independently, can explain the occurrence of endemic goître or of cretinism. But it is more than possible that three or four, or more, of these, with other unknown conditions, may unite in producing a miasm which shall cause goître alone, or goître and cretinism. The "impression" of Sir John Forbes is more or less exactly that of most recent inquirers. He believes—

"That its cause is some form of that unknown local influence or thing commonly recognised under the name of miasma or malaria, and which operates on the animal system as a poison, producing special modifications of function, and special changes of structure according to certain special conditions, which, however, are like itself, unknown. As the unknown thing, which we term malaria, or miasma of marshes, under certain circumstances gives rise at one time to simple ague, at another to fatal remittent fever, &c., and produces at times a morbid enlargement of the spleen, at others diseases of the liver, &c.; so I can imagine, that some other malaria or unknown thing or influence of local origin, may be the cause of ordinary bronchocele, of the aggravated bronchocele or goître of the Alps, and also of cretinism."

Zillner then proceeds to draw attention to what he calls the chamber or house miasm, as a cause of idiocy. We think, however, that he falls into an error in making this identical, or nearly so, in its nature and effects with that miasm which originates goître, cretinism, and also, according to many, marsh fever. We believe that, given all the unfavourable conditions which he describes as occurring in the dwellings of the poor, and which he makes the parent of the miasm, in a district where goître and *cretinism* are unknown, these diseases would still be unknown, though the evil effects of such conditions, as seen in the production of *idiocy*, would still exist in full force. They are to be found in our own country, and in our cities are largely operative. They give rise to hydrencephaloïd disease, passive hyperæmia of the brain, typhoïd disease, and dysenteric affections; they interfere with nourishment and growth, lower the vital power, make dentition difficult, and in a thousand other ways lead indirectly to the occurrence of idiocy. Zillner gives this as one explanation of its working, and we adopt it as sufficient. It is an undoubted fact, as he says, that cleanliness, ventilation, regulated warmth, suitable nourishment and exercise, the observance, in short, of the rules laid down by Combe in his "Management of Infancy," would there, and here, and everywhere, tend to diminish the frequency of idiocy, arising from this and from many other causes.

7. His last division is idiocy arising from other or accidental causes,

embracing all those cases which cannot be referred to the other divisions. Zillner states that epidemics of hooping-cough are frequent in Salzburg, and that idiocy has been observed as the sequel. We have met many cases of deaf-dumbness and of idiocy which have arisen from this cause.

We think, however, that he has made an important omission in not pointing out that scarlet fever, measles, small-pox, and typhus fever, are *very frequently* the origin of idiocy. In our country, at least, this is certainly true. One-fourth of the cases of deaf-mutism in Great Britain, and two-fifths in America, are cases of acquired deafness, and are generally referable to these maladies, and we believe that nearly twenty per cent. of our idiocy can be traced to the same diseases—diseases which we regard as more or less *preventible.*

It is, indeed, gratifying that this epithet can be applied to so many of the causes of idiocy. Zillner states that traumatic, congestive, toxic, and miasmatic idiocy are certainly capable of being diminished in frequency. Were such a diminution occurring, it is interesting to know, as he points out, that it would in all probability first show itself in a lessening of the number of imbeciles, while the amount of idiocy of a graver type would remain but little affected.

It is of moment to find a writer of note laying so much stress on the importance of the judicious management of infancy, as a preventive of idiocy. He thinks that wherever the care of the children is carried out in an intelligent, steady, and persevering manner, there the risk of idiocy diminishes in a high degree, even where there may be an hereditary or constitutional predisposition.

Pathological anatomy, prior to the researches of Virchow and Zillner, had done little for this form of mental disease. The post-mortem examination of idiots does not often fall in the way of those who are interested in the subject, and who are able carefully to make and to record such examinations. The majority of idiots die in private houses, where friends or relatives would oppose a sectio cadaveris; but even when granted, the private practitioner is too busy to undertake an investigation where *every thing* would have to be looked at, and which would be more than usually difficult and tedious if satisfactorily done.

The first anomaly to which Zillner alludes, is the congenital atrophy of the brain as it occurs in the true microcephalic idiot. It appears to affect chiefly the grey substance, the convolutions being often shallow, and sometimes nearly absent. It is believed to be frequently the result of active disease during fœtal life, and is generally accompanied by premature closure of the sutures of the skull.

There is also a secondary atrophy of the brain, following inflammatory processes in the brain-mass itself, or in the membranes, or resulting from the pressure of a slowly removed external hydrocephalus or hydrops of the arachnoïd, or effusion of blood during the act of birth, or such like causes. An attack of pleurisy, pneumonia, bronchitis, hooping-cough, &c., may and do induce head affections whose end is this form of atrophy. Atrophy of one side of the brain also occurs. A beautiful illustration of this is described and figured by Virchow.

Hyperæmia of the arachnoïd and cerebrum, the probable cause of hypertrophy of the brain, is another anomaly. He believes this often to result from injudiciously treating children, from enveloping them in featherbeds, from letting them lie and sleep near a hot fireplace, from their year-long life in hot, damp, and ill-ventilated rooms, from setting them down under a hot sun, from overloading the stomach with food, from the habitual use of opium or alcohol as "quieters," from accidents during birth, and indirectly from various diseases of the thoracic and abdominal viscera.

Anomalies of the membranes next come under notice. We have hyperæmia of the arachnoïd; hæmorrhage in the arachnoïdal sac; anæmia of the brain membranes and brain, as in Marshall Hall's hydrocephaloïd disease; dropsy of the arachnoïd; inflammation of the membrane of the blood vessels, or phlebitis of the brain sinuses; acute hydrocephalus the sequel of measles, scarlet fever, hooping cough, &c.; both forms of chronic hydrocephalus; and inflammation of the dura mater, leading to thickening of the skull bones, to osteophytes, and to premature closure of the sutures.

Abnormalities of the bones of the skulls of idiots are of great interest, and have received a most philosophical examination at the hands of Virchow, to whose researches Zilluer often refers.

Among the anomalies of least importance are the permanence of the frontal suture, as in chronic hydrocephalus, and the appearance in excess and in unusual positions of Wormian bones.

He gives the measurements of twelve idiots' skulls found in the Salzburg Museum, in a table constructed on the model of that used by Virchow, after whom he also compares these measurements with each other, and with much the same results. Most of the original owners of the skulls are noted as cretins or half cretins.

Taking the height as from the anterior border of the foramen magnum to the vertex capitis, and the length as from the root of the nose to the occipital protuberance, and the temporal diameter as the breadth, we have,

A mean	length of . .	17·47,	ranging from		15·5	to	19·2.*
„	breadth of . .	12·7	„	„	11·6	„	14·0.
„	height of . .	13·65	„	„	12·9	„	15·4.
„	circumference of	52·75	„	„	49·0	„	56·8.

Comparing these measurements with the normal skull, according to Virchow, we have as follows:

	Idiot.		Normal.
Length	17·47	...	18·38.
Breadth	12·70	...	12·13.
Height	13·65	...	13·00.

This shows in the idiot a diminished length, but increased breadth and height.

The next measurements relate to the sutures, and show as follows:

* The measurements are given in centimetres.

	Idiot.		Normal.
Frontal suture . . .	12·4	...	13·5.
Sagittal ditto . . .	13·06	...	12·25.

The short frontal suture indicates a flattening of the frontal arch.

Again, the coronal suture in idiots is 23·7 as compared with 24·4 in the normal. This, with what preceded, shows that the whole frontal bone is usually small in idiocy.

The measurements from ear to ear over the anterior fontanelle in idiots is 32·84, and in the normal skull 34·63, to the disadvantage, again, of the idiot's *fore*-head.

Among the heads of idiots which he examined, were illustrations of macro- and micro-cephalus, of brachy- and dolicho-cephalus, with several of the intermediate and compound forms. In very many of these there was closure or synostotic union of the sutures—thus, in the twelve skulls examined, there appeared partial or complete closure of the sagittal suture eight times, of the lambdoïdal five times, of the coronal five times, &c.

The condition of the various sutures in the individual cases is given, and it is interesting to find Virchow's conclusions borne out in the relation between the shape of the head and such conditions of the sutures—viz., their normal permanence, or pathological premature closure. Anomalies of other osseous structures are also alluded to as frequent.

An irregular position, and early decay and loss of the teeth of the upper jaw, is noted as common. In thirty to forty per cent. of the idiots of this country, this is to be observed, and is always associated with a smallness and narrowing of the upper jaw, and a lofty palate. The close connexion of the maxillary bones to the bones of the skull will make any interference with the growth of the latter affect the development and form of the former; and we consider that the importance which Virchow attaches to the tribasilar bone (sphenoïd and occiput), as that on which the skull superiorly and the face inferiorly are built, receives great support from the frequency of this abnormality of the upper jaw in idiots. As illustrating other anomalies, Zillner says that of 63 idiots examined, there occurred defective speech in 27, defective hearing in 17, defective walking in 8, defective sight in 4, goître in 15, &c.

We cannot conclude our notice of this work without again expressing the high opinion we entertain of its value. It will add greatly and lastingly to the reputation of the writer.

The author of the last four works at the head of this article is well known to the profession as one of the most philosophical and productive thinkers of our time. Whatever he does, he does well. He observes accurately, collects industriously, reasons soundly and cautiously, and writes agreeably. Can we say more in his praise? Of his works generally this is true, and of those now under notice we are inclined to think peculiarly so. In their perusal we go with him step by step in the research, admiring the extent of collateral information which he brings to bear, the pains-taking gathering of relevant facts, his temperate and careful reasoning, and insensible arrival at his conclusions. These are especially embodied in the last-written work, which appears in a separate form, and has numerous and well-executed drawings. The other papers are included in his collected writings, published in 1856, and are also illustrated.

It will not be possible for us in the course of a review to give a full analysis of this research. It can scarcely be stated in fewer words than he has himself employed. We shall endeavour, however, to convey to our readers some idea of its nature, and of the important conclusions to which it leads.

In considering the development of the skull, the starting-point is this: The suture-substance itself furnishes the material of ossification, the *stroma* for a deposition of the lime salts, so that, under ordinary circumstances, a skull-bone can only increase equally in all directions when this bone-originating suture-substance lies on all its sides. If, then, adjoining skull-bones be soldered together by a premature and complete ossification of the interlying suture (by synostosis), a limit is set to further growth in that direction. If this happens to many sutures at the same time, a microcephalous skull results. If it only happen to one suture or to a part of a suture, an abnormal, asymmetrical, or deformed shape follows. Compensation, however, can occur through an excess of growth in those parts, to which no such limit is set, so that there may exist deformity without diminution of space in the brain cavity.

To pathological skull-formations he applies the terms used by Retzius and others in describing the skulls of the different races.

It is easily understood, then, how synostosis of the sagittal suture, by stopping the growth of the parietal bones on that side, and leading to a compensatory growth on their anterior and posterior margins, will take from the breadth of the skull and add to its length, and so produce a dolichocephalic head.

Again, ossific union of the coronal suture, setting a limit to the growth of the frontal bone in a backward, and to the parietal bones in a forward direction, with a compensating development on the superior and posterior borders of the parietal bones, will add to the width and take from the length, and result in brachycephalus, of which it will give the platycephalic variety. A similar premature union of the occiput to the parietal bones will also cause brachycephalus, but will give the pachycephalic variety. Synostosis of the coronal and lambdoïdal sutures at the same time, would give a pure brachycephalus. Nor is it difficult to see how other shapes or deformities may arise. In all forms, if the deficient growth in one direction is not fully compensated by an increased growth in another, microcephalism may co-exist.

He then proceeds to give proof that these principles hold good, and the proof appears to us wholly satisfactory. Nearly thirty woodcuts are used in illustration of this part of the subject.

He does not, however, maintain that all errors in the development of the skull can be referred to this cause. At times the skull simply conforms itself to the growth of the brain, as may happen in micro- and macro-cephalism. This refers to pathological conditions. In health the two things harmonize. Brain anomalies may lead to skull anomalies, while in other cases the latter may determine the development of the brain. Microcephalism *can* exist without synostotic union of any of the sutures of the upper bones of the head, but this is not frequent.

He is not led at this stage of his inquiry to conclude that the form of the cretin skull is specific. It may be macrocephalic with hydrocephalus; microcephalic with primary deficiency of brain development; or synostotic—contracted obliquely, longitudinally, or transversely—with or from inflammatory or hyperæmic conditions of the coverings of the brain during fœtal or early extra-uterine life. In all cases the space for the cerebral mass may be diminished, either directly or through effusions.

It is here that the importance of the base of the skull comes forward. From the investigation of synostosis of the coronal and sagittal sutures, he goes to the posterior and lateral, and from these to the consideration of the base of the brain—*quoad* the premature ossific union of its sutures.

Here, however, a difficulty occurs. Most of the sutures of the upper part of the skull remain open till advanced life; so that, even in an adult skull, premature synostosis can be determined. It is otherwise with the sutures at the base of the skull. The anterior and posterior sphenoïd, though not united in fœtal life, are joined soon after birth, and at puberty, or thereabout, the union between the sphenoïd and occiput is complete. To fix a premature synostosis in these cases, and to determine their mode of occurrence and their effects, examinations must be made of the fœtal and of the child's head. Virchow's position fortunately gave him opportunities of doing this.*

In measuring the different parts of the base of the abnormal skull, he found that the same form of alteration of the upper part might exist with altogether different and opposed conditions of the base; and that in fact there was no constant relation between the conditions of the two parts. For instance, in a microcephalous skull, the distance from the foramen magnum to the root of the nose was 10·15 centimetres; while an exquisite hydro-macrocephalic skull measured only 9·95, and a cretinic hydro-macrocephalic only 9·05. He also found that the sum of the lengths of the individual bones forming the base of the brain was not the same as, nor did it bear any constant relation to, the length of the *line of the base*. The basilar portions of the occiput and the sphenoïd are often tilted up against each other; and later researches showed the size of the angle so formed to be influenced by

* We must here remark that Virchow's "Grundbein" is the "Os tribasilare," consisting of the "os basil. post," or the "os basioccipitale" of Owen; the "os basil. med.," or the "os basisphenoïdes" of Owen; and the "os basil. ant.," or the "os præsphenoïdes" of Owen. The earlier an ossific union takes place between these bones, the sooner will their growth endways be interrupted.

premature synostosis of the spheno-basilar articulation, &c. He then examines the relations of the differences or anomalies observed at the basis cranii, to the position and development of the bones of the face, and he finds these relations to be important and close. Applying all this to the peculiarities observed in the faces of the cretins, he is able to anticipate their explanation—an anticipation which was verified by the examination of a new-born cretin, where complete synostosis of the occiput to the sphenoïd, as also of the anterior and posterior sphenoïds, was found.

His chapter on the influence of the tribasilar bone on the face formation, to which allusion has been made, is full of interest. Prognathismus he clearly shows to be associated with certain conditions of this bone, involving premature synostotic union of its components. The absolute size of the jaw, however, he admits, may produce this form of face. We think it probable that the prognathism of race will be found to depend on this; while that of idiocy, which is a pathological state, may depend on the connexions of the jaw with the base of the brain.

But not only is physiognomy in a sense dependent on the condition of the tribasilar bone; the arch of the skull appears also to be frequently influenced by it. This may be seen by taking the half of a skull with a long basis (a vertical section), and applying it to the opposite half of another with a shorter basis, so that the anterior edges of the foramen magnum of the one shall be exactly opposed to that of the other. It will then be found, in the skull with the shorter basis, that the frontal wall falls back, that the root of the nose is more depressed, that the anterior and spheno-temporal fossæ are smaller and flatter, while the posterior is greater. In short, the whole fore-part of the head will be reduced, with a compensation in the length of the back-head.

We think it more than probable that the peculiar physiognomy of cretins, more especially the depression and breadth of the root of the nose and the prognathismus, are capable of explanation through these researches.

We have merely attempted to indicate the progress of this investigation, and to direct attention to some of the points of greatest interest. To his observations on cretinism in Franconia, though of great value, we have not alluded. The whole forms a contribution to the physiology and pathology of the skull, and to the natural history of cretinism, of the highest value, for which, as for many other things, we have to thank the distinguished author.

Review V.

1. *Klinik der embolischen Gefässkrankheiten, mit besonderer Rücksicht auf die ärztliche Praxis.* Von Dr. B. Cohn, Arzt am Allerheiligen Hospital und Docenten an der Universität zu Breslau.—*Berlin*, 1860. 8vo, pp. 696. With four coloured plates.

Clinical Observations on Embolic Diseases of the Vessels, with special reference to Practical Medicine. By Dr. B. Cohn.

2. *Bidrag till läran om Emboli.* Akademisk Afhandling af Albert Lindström.—*Lund*, 1856. 8vo, pp. 69.

A Contribution to the Theory of Emboli. An Academic Essay by Albert Lindström.

3. *Bidrag til Læren om Emboli.* Af A. Brünniche, Reservelæge ved Frederiks Hospital, Hospitals-Meddelelser. Anden Række, Förste Bind.—*Kjöbenhavn*, 1856. pp. 26.

A Contribution to the Theory of Emboli. By A. Brünniche, Physician Extraordinary to Frederiks Hospital.

The existence of the morbid condition recently designated by the term *embolism* was, to a certain extent, known at a very early period of the history of medicine, and had been repeatedly established by the observation of cases and the results of post-mortem examinations. Thus a necessary connexion has long been recognised between the closing of an artery and the simultaneous gangrene of the part supplied by it. In diseases of the heart, mortification of the extremities had often been seen to arise, and in such instances the afferent artery had been found in a distant situation obturated, or, as it was then expressed, in a state of inflammation. But, above all, certain apoplectic and suffocative attacks had been connected with and explained by the formation of polypi in the heart, and their transposition into the corresponding vessels. For Virchow, however, it remained to place the whole theory of emboli upon a firm foundation, and Dr. Cohn therefore naturally divides its history into three epochs, the first embracing the observations of those who laboured anteriorly to Virchow's time; the second, the contributions of Virchow himself; and the third, those of still more recent writers.

"*Epoch anterior to the time of Virchow.*—Even Galen* states, in relating the case of a person who had died suddenly of suffocation after having long suffered from palpitation of the heart and from a feeling of oppression, that he had often met with a similar occurrence in other persons labouring under cardiac affections. Theoretically, he suggests as the cause of death a sudden obturation of the smooth or proper pulmonary arteries by bodies which were subsequently indicated as polypi. However, it was reserved for the sixteenth century, after anatomy had become more general and its importance was more fully recognised, to enlarge the circle of observations both of diseases of the heart and vessels in general, and of the morbid condition at present under consideration. Vesalius† remarks upon an enormous polypus in the left ventricle which had grown until it had acquired the weight of two pounds (!), and

* De loco affecto IV. p. 295. † De Gangrena et Sphacelo, c. 4, p. 775.

had given to the heart itself a form and size similar to those of the gravid uterus. On this occasion he reminds his readers that with the growth of this unnatural mass in the left ventricle gangrene of the extremities is often combined; that this disease betrays its presence only by irregularity of pulse, but never by any pain in the heart. Lancisi, too, mentions the case of an individual labouring under cardiac disease, whose right hand had suddenly become gangrenous without any external cause; the arm was amputated, and the patient survived twenty-six months. So far had experience thus early demonstrated the combination of disease of the vessels and gangrene. The special doctrine of emboli, however, of the obstruction of the vessels by foreign bodies, was first based upon the theories suggested in reference to the formation of polypi within the vascular system." (p. 4.)

Dr. Cohn quotes passages bearing on this subject from Bartoletti,* from Kergringius (1670), and from Bonet, and, in the following century, from Boerhaave and his commentator, Van Swieten, and Morgagni. We thus arrive at the nineteenth century. Still, no great advance was made in the appreciation of the results of post-mortem examinations, or of the experience of older writers. Every plug found in any part of the vascular system was considered as a *local* product, the result either of inflammation or of some other process. A wholly new theory of arteritis and phlebitis was then promulgated, the essence of which consisted finally in the formation of a coagulum, while it was considered beside the question whether a morbid condition of the vascular wall was at the same time established or not. Cruveilhier was the principal advocate of this view; his short and pithy proposition is well known: "*La phlébite domine toute la pathologie.*" Thus the idea of embolism was again completely suppressed, as if it had never existed, until, in 1828, Alibert † reported a case of gangrene of the extremities and arterial obstruction in the left upper extremity by coagula, and directly demonstrated the identity of the latter with formations in the left auricle of the heart. But this work, too, remained quite unnoticed. Immediately afterward appeared a second from Victor François,‡ in which the part of emboli was decidedly taken up, and the disease was with much clearness shown to proceed from sudden arterial obstruction caused by foreign plugs which had been set free. But of the proper, true nature of arteritis, and of the relation of the embolic plugs to it, this writer had as little idea as many investigators before and since his time; the coagula which had been carried along were still considered only as products of an arteritis developed in distant places: "Il vous semble donc bien prouvé," he says, "que les caillots et les concretions sanguines tirent leur origine de *l'inflammation de la tunique interne* ou commune du système à sang rouge, et que ces corps parfois détachés de leur siège primitif, peuvent se porter dans des rameaux ou de troncs artériels et les inflammer par leur présence."

What little way the views of François made, is best seen from a

* Method. in Dyspnœam. Bononiæ, 1630.

† Recherches sur une occlusion peu connue des vaisseaux artériels considérée comme cause de gangrène.

‡ Essai sur la gangrène spontanée.

great classical work published by Tiedemann in 1843, on the 'Constriction and Closing of Arteries in Diseases.' In speaking of the etiology of his subject, the author mentions, under the fourth head, coagulations of the blood, which, like stoppers, obstruct the canal of the arteries. These may be either processes of the fibrinous layers from aneurismatic sacs extending into the arteries, or they may be produced in arteries in whose walls earthy concretions have formed, in consequence of the latter projecting through the inner coat into the canal of the vessel, and of the deposition of layers of fibrin upon their sharp points. Here there is no thought of embolism, neither is there any mention of François or his predecessor, Alibert.

Three years later appeared Hasse's work on obstruction of the cerebral arteries as the proximate cause of a form of softening of the brain,* wherein the author not only discusses embolic facts, but moots and investigates questions with great precision, which have as yet received no better answer or exposition. In the following year (1847), Pioch published in the 'Gazette Médicale de Paris,' p. 672, a case manifestly depending upon emboli. In a patient labouring under hypertrophy of the heart and valvular induration, several sudden arterial obstructions had occurred during life in the brachial, vertebral, and right and left crural arteries. These latter had led to gangrene of the extremities. The patient died some months later, but there was no post-mortem examination. Legroux at the same time related in the 'Gazette Hebdomadaire' (*Sur les Polypes du Cœur*), a case of articular rheumatism with endocarditis, concretions in the left side of the heart, and obstruction of the left subclavian artery, the aorta, iliaca, &c., without gangrene.

Bouillaud, in his 'Maladies du Cœur,' is still nearer to the point, concluding his chapter on polypi of the heart (t. ii. p. 618) in the following words: "Dans quelques cas aussi, il est *probable*, que des concretions du cœur sont expulsées dans le système vasculaire. Mais c'est trop s'arrêter sur une question pour la solution de laquelle nous manquons d'un nombre suffisant d'observations exactes."

We now pass to the second period. In 1847 first appeared in the 'Archiv für Physiologische Anatomie,' an essay on arteritis, wherein many hitherto commonly received views were combated, and the embolic character of certain products, previously considered to be of inflammatory origin, clearly shown. In this paper the author, Professor Virchow, came to the following conclusions: "The primary occurrence of older coagula (fibrinous plugs), formed long before death in the pulmonary artery—namely, where the obstruction of the artery precedes or is independent of the changes in the parenchyma, is with respect to the coagulation in the arteries always secondary. These plugs have arisen in a part of the vascular system anterior to the lungs in the circulation—that is, in the veins and the right side of the heart, and have been carried by the current of blood into the pulmonary artery." These views were supported by a long series of experiments, in which,

* Ueber Verschliessung der Hirnarterien, &c., 1846.

by the introduction of animal substances or of pith of elder, Virchow produced

"Violent pneumonias, commencing with inflammatory hyperæmia, and causing the rapid deposition in the pulmonary vesicles of fibrinous exudations, which either underwent purulent metamorphosis or became gangrenous. With the advance of these changes, pleurisy was soon developed at the periphery, at first producing fibrinous, coagulable, and viscid exudations over the affected portion of the lung, but rapidly and as it progressed towards the other side of the chest, accompanied with enormous increase of hyperæmia, extravasations in the parenchyma of the pleura, and large, watery, hæmorrhagic exudations, with preponderating tendency to ichorous metamorphosis in its cavity. At the affected part of the lung the pleura became gangrenous, and finally gave way, and pneumothorax set in. The whole series of phenomena was developed in not quite five days. Nothing similar followed the introduction of large pieces of Indian rubber into the pulmonary artery." (p. 15.)

To this work, which was in great part published as early as the year 1846, were naturally added, "Investigations on inflammation of the arteries in general." "The inner coat of the latter, it was long known, had no vessels; how then could an exudation or coagulation be produced through it? Was this really to be regarded as inflammatorily effused, or as a product of local states of the current blood, or lastly, was it to be supposed that a process took place here, similar to that described above as occurring in thrombosis of the pulmonary artery? Wherein did the essence of true arteritis consist? Certainly not in the deposition of an exudation on a free surface in the bore of the vessel; abscesses had long since been found between the inner and middle coats without a simultaneous analogous production in the cavity of the artery. A satisfactory elucidation of these questions was essential to the further discussion of the pathological anatomy of the embolic process. As we have stated, gangrene of the extremities had often been found coexistent with obstruction of an afferent arterial branch. Not merely was the cavity of the artery filled with a partially bleached, partially broken up, and apparently puriform plug, but the arterial wall itself was in a state of pathological destruction, exudations of various degrees of organization were discovered in its coat, fatty detritus of its elements existed. Was the obstruction in this case primary, and did it by chemically or mechanically irritating properties secondarily cause the inflammation of the vessel, or was the relation inverted, or lastly, might both the obstruction and the inflammation be considered as the joint effects of a common third cause?" According to Virchow, the inner layers of the arterial wall are not permeable to an exudation from the capillaries of the arteries, capable of coagulating on their free internal surface. The inflammatory product always exists in the first instance between the cellular and the middle or between the latter and the inner coat. This last must be perforated before pus can be deposited in the cavity of the vessel. The coagula, which are sometimes, but by no means constantly, found in the cavity of inflamed vessels, are only products of local stases, often caused by a roughness of their inner surface, by alteration of structure and relaxation of their elementary parts, the results of altered nutrition; and are either general, when whole segments of an arterial system are filled with them, or locally circumscribed; while in other cases, again, the coagulum adheres only to one wall, with the effect of narrowing the passage. This latter lesion is never of an embolic nature. On the contrary those coagula, which exist without simultaneous lesion of the arterial wall or adjoining capillary circulation, are never formed on the spot, but have been separated from a distant point in the circulation, and have been carried with the current of blood as far as they could go. These are the genuine forms of emboli. They are always found in places where a larger arterial trunk, by giving off branches, suddenly acquires a more constricted caliber, the cavity of the vessel behind it is in general empty to the periphery, the arterial wall locally is for the most part intact. The state of things is however quite different in the coagulations producing general obstruction, where all the branches and twigs of an entire section of the arterial system are without exception filled with homogeneous thrombi. These are the results of absolute stasis of the capillary circulation. Such is a brief sketch of Virchow's researches upon arteritis.

The works of the third period are, with few exceptions, all based upon Virchow's theses, they are for the most part only confirmatory of his views, without essentially enlarging the circle of ideas upon the subject. Bennet's report of cases of nervous diseases, published in the 'Monthly Journal of Medical Science,' for April, 1850, contains an interesting and careful statement of the relations between arterial obturation and softening of the brain. Among other works which specially claim our attention and contribute to the real advancement of science in connexion with this subject is a paper by Dr. Kirkes, "On some of the Principal Effects resulting from the Detachment of Fibrinous Deposits from the Interior of the Heart, and their Mixture with the Circulating Blood," 'Lancet,' June, 1852. The author gives the following as the principal results of his numerous observations: 1. That it is undoubtedly possible that fibrous concretions may separate during life from the valves of the heart; and that they, 2ndly, may cause obstructions of particular peripheric organs, whether internal or external; and 3rdly, that by simple admixture with the blood, they may have a poisonous and decomposing influence, similar to that of the typhous and pyæmic ferments. In the lungs apoplectic foci were developed, those well-known infarctions, and, as remains of these, broad fibrinous masses; and on the arterial side were found the metastatic fibrinous wedges, hitherto generally, although vaguely and without much foundation, referred to capillary phlebitis. Among other things, Dr. Kirke's explanation of the petechial spots frequently met with in endocarditis and pyæmia, both on the external skin and on different mucous membranes, and even on the peritoneum, and whose origin he refers to capillary emboli, is particularly interesting. In an essay of Meckel on carditis, too,* reference is made to the connexion of certain peripheric affections of the lungs and spleen with diseases of the heart.

The periodical literature of the following year contained some special

* Deutsche Klinik, 41.

observations on this subject. Among these may be particularly mentioned three cases of hemiplegia, caused by embolic obstruction of a cerebral artery. Similar observations are communicated by Dr. Burrows in the 'Medical Times,' February, 1853, but this writer, notwithstanding the hyperæmia of the softened portion of the brain distinctly pointed out by himself, is of opinion that this softening was produced by the cutting off of the supply of blood by anæmia. To his report of these two papers, Eisenmann* appends some remarks, particularly with reference to the hyperæmia constantly found in red softening with arterial obstruction. Hyperæmia and stasis are developed here as in other organs, according to him, when collateral branches find access to the channel of the obstructed artery; where this does not take place, collapse ensues. Softening of the brain after emboli is therefore not caused, like senile gangrene, by want of blood, but by serous effusion in consequence of hyperæmia. In Günsburg's 'Zeitschrift' (Band v. p. 202), Dr. Cohn himself reports an instance where softening of the brain depended on plugging of the *arteria fossæ Sylvii*, and Bierck records a case of heart disease in which complete hemiplegia depended on plugging of the left vertebral artery and of the artery of the fossa of Sylvius, the walls of these vessels being perfectly sound. This softening of the brain is by Traube† considered to be a pre-eminently necrotic process, depending on interrupted nutrition and arrested metamorphosis of tissue, consequently on an absolute stasis of the current of blood, and not on hyperæmia through the collateral vessels. Accordingly, instead of energetic antiphlogosis, he recommends a more stimulating mode of treatment, with a view of, if possible, establishing a collateral circulation.

In the same year appeared the first work of any extent upon a form of emboli the existence of which was before scarcely suspected. It is well known that certain severe forms of intermittents give rise to considerable accumulations of pigment in the blood, which, by obstructing series of vessels of various sizes, occasion numerous anatomical and functional disturbances. These dark bodies are for the most part primarily developed in the spleen, whence they are conveyed to the vena portæ, gradually increasing in circumference, though some of them preserve their microscopic minuteness. Planer, of Vienna, had an opportunity of observing an epidemic of such intermittents, and of establishing the presence of pigment in the blood. Thus, the existence of a form of capillary emboli, previously little known, was for the first time scientifically established. In such cases we find in the brain numerous little point-like sanguineous extravasations, the simple results of the necessarily increased pressure of blood behind the obstructed parts. Something similar is met with in the capsules of the kidneys, laceration of the hyperæmic capillaries and bloody urine—only in a few instances does the affection lead to true morbus Brightii.

As in Traube's essay, already quoted, the attempt was made to formulize a diagnosis of embolic softening of the brain, the following year brings under our notice several good works in reference to dia-

* Canstatt's Jahresbericht, Band iii. p. 60. † Deutsche Klinik, 1853, No. 44.

gnosis in connexion with other parts of the system. Thus, fibrinous coagula in the heart, the so-called polypi, again become the subject of much discussion. These had long been left unnoticed, and the possibility of their formation during life had even been doubted; but now an accurate scheme of symptoms was again fixed upon, by which they were said to be characterized. Richardson's paper on "The Diagnosis of Fibrinous Concretions in the Heart,"* and Lavirotte's "Note sur un nouveau signe pour servir au diagnostic des Concrétions Fibrineuses du Cœur,"† are works based on numerous observations and on impartial estimation of the many theories previously advanced. The formation of coagula in the right side of the heart is attended with symptoms of exhaustion, the pulse becomes small and intermitting, the countenance is collapsed, the muscles are rendered powerless. The patient may be able to inhale deeply, and still suffers from apnœa, the veins become highly turgid, and sudden death occurs if large concretions separate and suddenly obstruct the pulmonary artery. Coagula at the left side of the heart were said to present another chain of symptoms. Unusually stormy action of the heart, signs of pulmonary hyperæmia, expectoration of blood and imperceptible pulse, coldness of the extremities and convulsions, were here chiefly observed. Nevertheless, in spite of all these labours, we meet with no symptom pathognomonic of this affection as contrasted with valvular disease.

From this period, too, obstructions of the pulmonary artery have been more frequently observed. We may refer to a paper by Hecker,‡ and particularly to an essay by Klinger, "Beobachtungen über die Verstopfungen der Lungenarterie durch Blutgerinnsel."§ In connexion, moreover, with melanæmia, numerous examples of capillary emboli have been communicated; and in Frerich's 'Clinical Treatise on Diseases of the Liver' may be found a very excellent and full examination of this subject, with corresponding and very instructive illustrative cases.|| In 1856, Dr. Cohn published¶ some of his numerous experiments on rabbits and dogs bearing upon the subject of his present work. At about the same time, Baron Gustaf von Düben, of Stockholm, recorded a case of emboli of the left cerebral carotid artery, extending into the arteries of the corpus callosum, the fissure of Sylvius, and the communicating artery, and producing a focus of softening, as large as a walnut, in the corresponding hemisphere: the starting point of the affection was coagula adhering to the aorta. During life, hemiplegia had suddenly occurred, attended at first with slight fever. A second attack gave rise to sudden loss of consciousness and of the power of speech, and proved fatal at the end of five days, with symptoms of pressure on the brain and general paralysis. Lastly, an essay of special value by Panum, on the "Forms and Causes of Sudden Death

* Lancet, April, 1855. † Gazette Médicale de Lyon, 1855, No. 18.

‡ Deutsche Klinik, No. 36.

§ Archiv für physiol. Heilkunde, von Vierordt, Heft 4, 1855.

|| The English reader will find the observations referred to at p. 314 of the first volume of the New Sydenham Society's edition of Prof. Frerich's important work.

¶ De Embolia ejusque sequelis.

by Emboli," appeared in Günsburg's 'Journal für med. Klinik,' Heft vi. 1856. This writer found sudden death to attend emboli of the pulmonary artery, of the coronary arteries of the heart, and of the arteries of the brain. Obstruction of the cerebral arteries is seldom instantaneously fatal, even when large arterial branches become impassable to the current of blood. But it is the first series, obstruction of the pulmonary artery, which is pre-eminently the cause of sudden death by emboli. The fatal result is by this author attributed to paralysis of the nervous centre, in consequence of a deficient supply of blood, and is said to be caused by only a very limited number of possible occurrences :—1. Obstruction of all the smaller vessels and capillaries, such as we can effect artificially by waxy injections, and as is perhaps occasionally observed at the bedside in melanæmia; 2, by the penetration of air in large quantity into the venous system; and 3, by coagula of blood, large or numerous enough absolutely to prevent everywhere the access of blood to the lungs. In the anxiously-expected second edition of Rokitansky's 'Pathological Anatomy,' the subject of emboli is treated of in a rather short and almost superficial manner; and this observer maintains, in opposition to all contrary statements, *the possibility of a free effusion on the inner vascular coat,* without destruction of the latter, *in consequence of arteritis,* which may give rise to the formation of obturating coagula.

The cases and remarks on treatment contained in M. R. Schützenberger's essay are interesting.* In his conclusions as to treatment, this author points out as indications:—1, the solution of the plug; 2, the possibility of establishing collateral circulation; and 3, preventing the transference of fresh thrombi. He himself employed bicarbonate of soda, and found the collateral circulation considerably developed, but the proper thrombus untouched. "Etait-ce en raison de l'état généralement sain des parois arterielles, ou en raison de la fluidité plus grande du sang, qui aurait empêché la formation de coagulations secondaires trop étendues? Je ne sais."

At about the same time, the subject of emboli came under the consideration of a scientific association of medical men connected with the Parisian hospitals, whose proceedings are reported by Dr. Bohier, in the 'Union Médicale' for 1857, Nos. 71, 72, 96, 98. The commission states that arterial obstruction is certainly the most frequent cause of the spontaneous gangrene of a limb, but that it may be complicated with obliteration of the veins, giving rise to œdema; while, if the arterial obstruction exists alone, the gangrene assumes the dry form. This theory is, however, supported neither by experience nor by physiological laws. Dr. Cohn can as little coincide in this view, as in the second fundamental idea of the report, according to which gangrene is to be attributed only to arteritis.

"Hence we see," he adds, "with regret, notwithstanding all the above-quoted experiments, to which may be added an 'Exposé of the Theories of Professor Virchow,' by Lasegue, in the 'Archiv. gen. de Méd.' for October,

* De l'obliteration subite des artères par des corps solides ou des concrétions fibrineuses détachées du cœur ou des grosses vaisseaux à sang rouge, March, 1856: Gazette Médicale de Strasbourg, Feb. 1857.

1857, how little progress Virchow's true doctrine, based upon reasonable deductions, has made in France. It is therefore no wonder," he adds, "that an essay such as that of Fritz—'Obturation Metastatique des Artères Pulmonaires' ('Union Médicale,' No. 54), in which *one* case of emboli of the pulmonary artery through coagula conveyed from the vena iliaca dextra is described rather in detail, should excite special attention, although ten years previously an infinitely greater number of much more interesting communications on the same subject had been made by Virchow." (p. 32.)

Dr. Cohn remarks further on the non-estimation in France of extra-Gallic contributions, and proceeds to show that Blondet had, in 1851, in the 'Union Médicale,' Nos. 114–130, published a series of essays on fibrinous concretions of the heart, considering the latter not always as simple cadaveric productions, but as bodies long since formed during life; thus going too far, and reverting to opinions refuted so long ago as in the last century. In other respects he states the theory of emboli according to Virchow, and produces a work similar to Schützenberger's, a repetition of generally known propositions. In opposition to these articles, Professor Forget, of Strasburgh, now for the first time comes forward with all confidence. He had, in the year 1850, in his 'Précis des Maladies du Cœur,' designated emboli and gangrene of the extremities caused thereby as "un fait péremptoire et environné de preuves suffisantes," at a time when Virchow's labours were still unknown to him; MM. Legroux and Bouillaud had, in the year 1827, put forward similar ideas, and Forget therefore denies to Virchow so far the merit of priority. An animated debate had even arisen in July, 1851, in the Société Médicale des Hôpitaux de Paris, the results of which may be best characterized by quoting the following passage:

"Il importe que vous ne laissiez pas passer sans contrôle les faits, qui se glissent dans la science à l'aide de certains mots. . . . . . Je ne veux pas que nous soyons trop persuadés par les grands mots *venus d'outre Rhin*, comme *ceux d'embolie, d'urémie*, et qui tendent à donner une très fausse direction aux idées, etc."

Dr. Cohn quotes from Professor Forget's work at some length, because, he says, "it affords us a striking example of the lightness with which in France they pass sentence on foreign, especially German, contributions to science."

An incomparably better essay appeared about the same time in the same Journal,* in the detailed description of arterial polypi by Legroux, physician to the Hôtel Dieu. In a new version of the justly celebrated work of Durand-Fardel, the translator, Dr. Ullmann, of Würzburg, puts forward rather interesting ideas as to the relation of softening of the brain to emboli; after a lengthened dissertation, he inclines finally to Eisenmann's view. A fresh case of capillary emboli by Beckmann is to be found in Virchow's 'Archiv,' Band xii., corresponding in all its details to the two described by Virchow. In the 'Deutsche Klinik' (45 and 46) Von Gerhard suggests some views both novel and worthy of attention in reference to thromboses of the

* Gazette Hebdomadaire, 1857, 45, 48, 50.

cerebral sinuses in children, not only with respect to pathology and diagnosis, but also to their etiological relations.

In the last year which comes under our notice in this historic sketch, no very important works have been published on the subject of this review. Most of those which have appeared are only confirmatory of what was already known. Baron Gustaf von Düben, in the 'Hygiea,' enters into a consideration of the causes which, in endocarditic coagulation, direct the bodies swept along in the circulating current now to one, now to another organ. This appears to him to depend—1. On the situation in which the plug was formed. 2. On the size, form, and thickness, of the same. 3. On the angle at which the lesser vessel branches from the greater. 4. On the magnitude of the stream of blood in the afferent vessel. Virchow's 'Archiv' for the same year contains interesting papers by Grohe and Heinr. Wallmann of Vienna. Professor Burrow, at the end of an essay on Amputation,* suggests an idea of highly practical interest. Among the many causes of the frequent unfavourable termination of this operation, he classes the firm bandage strapped immediately after the operation round the limb, and which must compress the veins. Coagulation, of course, takes place in the latter. On loosening the bandage after some days, it is possible that the coagula may no longer obliterate the cavities of the vessels, and that through the motions of the limbs or the force of small lateral currents they may be carried towards the heart. Not pyæmia, but emboli, would then produce the fatal result. This ingenious hypothesis should be subjected to the strictest investigation. Lebert, in his Clinical Reports,† gives several highly interesting cases of spontaneous gangrene of the extremities with arterial obstruction, and also verifies the existence of probably embolic thrombosis of the ophthalmic arteries diagnosed during life. Graefe‡ in like manner describes an embolic process in the eye, diagnosed by him during life by means of the ophthalmoscope. Dr. Cohn concludes his historic sketch with the observation, that although scarcely a decennium has elapsed since the appearance of Virchow's work on obstruction of the pulmonary artery, the subsequent literature of the subject is now very extensive; and that although much still remains to be cleared up, particularly with reference to the physiology of the mechanism of the secondary processes, it is interesting to see that in Paris, in spite of all opposition, the truth and importance of embolic processes have at length obtained a just recognition. The subject announced for the Portal prize for 1860 was the following: "Des obstructions vasculaires du système circulatoire des poumons, et applications pratiques qui en découlent."

In his second chapter, Dr. Cohn proceeds to consider the sources of emboli. It is clear that embolic bodies may form either in the venous, the arterial, the portal, the lymphatic, or the chylous system. They may accordingly produce local obstructions and manifold functional disturbances in the lungs, the arterial vascular system, the liver, or the

* Deutsche Klinik. † Virchow's Archiv, Band xiii. pp. 165 et seq.
‡ Clinique Européenne, 1859, No. 14.

lymphatic glands. They may likewise pass through the right side of the heart without becoming attached there, and without producing endocarditic symptoms, and may thus reach the pulmonary artery. In the examination of this subject, one of the first inquiries which naturally suggests itself is, under what conditions does a separation of fibrin in the blood take place? The author is thus led to a general consideration of the phenomenon of the coagulation of the blood. According to Virchow and others, the presence and combination of the oxygen of the air is absolutely necessary to this coagulation, whether without or within the body; but the author shows by experiment that coagulation is possible without the access of atmospheric air. Hence he infers that the access of oxygen to the blood cannot be the essential cause of the coagulation of the latter. Blood, in fact, does not coagulate, although fully exposed to the air, until a retardation of its current takes place; so long as it continues to move with a certain velocity, it retains its fluidity. Dr. Richardson's theory, in like manner, the author rejects as wholly unfounded, and he even questions the correctness of the decision which gave the Astley Cooper prize to his essay instead of to that of Professor Brücke of Vienna. We have devoted so much space to our abstract of Dr. Cohn's historic retrospect, which appeared to us both interesting and important, and so much of the more practical portion of his volume still remains before us, that we cannot at present enter upon an analysis of his own numerous experiments in reference to this question of coagulation. But he awards to Dr. Richardson the merit of having shown the existence of ammonia in the blood; "to have proved that it is contained therein and in the breath in larger quantity than was hitherto supposed, remains as a benefit conferred upon science by his worth." At the same time, he utterly denies that the ammonia of the blood has anything to do with the coagulation of the latter. Still he acknowledges that, with respect to therapeutics, Dr. Richardson's theory may hereafter find its application: "that ammonia added in certain quantities to coagulated blood is capable of again rendering it fluid, is a fact, of the truth of which we may at any time convince ourselves. If this be the case, may we not in the preparations of ammonia find an agency capable of safely dispersing coagulations occurring in the body? Might not the long-known absorbent properties of some ammoniacal preparations, particularly of the formulæ used externally as liniments, herein receive their final, physiologico-chemical explanation? May not the efficacy of the same, particularly of the chloride of ammonium, in diminishing plasticity—that is, in lessening the coagulating power of the fibrin in inflammatory affections—be thus accounted for?"

Neither does the author accept Brücke's theory, which makes the fluidity of the blood depend upon the vital influence of the heart and of the vascular walls. But while Dr. Cohn thus rejects all others, he does not advance any distinct theory of his own, but contents himself with laying down the principles which may, in his opinion, "pave the way for the future final solution of this interesting and, pathologically speaking, infinitely important question."

According to Virchow, the conditions under which the formation of thrombus occurs may be classed in two principal groups: 1. Interruption or constriction of the caliber of the vessels from ligatures, the pressure of tumours and exudations, cicatrices, &c.; solution of the continuity of the vessels—for example, in venesection, separation of the placenta, &c.; dilatation of the same in aneurysms or varices; absolute diminution of the force of the heart, marasmus, in fevers, cachexias, anæmia. 2. Altered molecular attraction between the particles of the blood and of the surfaces with which they come in contact—as in considerable lesions of nutrition, particularly inflammatory, of the walls of the containing vessel; contact of the blood with bodies foreign to the constitution of the vascular wall—gangrenous destruction, for example, in the lungs. The elementary admixture of the blood forms only a subordinate cause, belonging to the second series.

Dr. Cohn devotes a large portion of his volume to the special consideration of emboli of several of the arteries of the body, as, for example, those of the brain, the medulla spinalis, the eye, the heart, the liver, the stomach, the spleen, the kidneys, the extremities, and the vena portæ. For such details, however, as well as for his observations on some other important subjects, we must refer our readers to the original work, believing it more consistent with our limits now to endeavour to lay before them a brief *résumé* of the chapter on the general pathology and treatment of emboli, with which he concludes his book:

"Embolism," observes Dr. Cohn, "that is, the obstruction of particular vascular branches by bodies transported from distant parts of the circulating system, is both a frequent and fully established morbid condition.

"It has its source in the whole venous periphery, in the arterial circulation, in the subdivision of the vena portæ, and lastly, also within the lymphatic and chylous vessels." (p. 661.)

Emboli are distinguished from local autochthonous thrombi, the results of inflammation, by the fact that the latter are slowly developed, and close only partially, never completely, the canal of the vessel; they consequently produce the symptoms rather of retarded or limited than of suddenly annihilated circulation:

"The special causes of emboli may be stated as the following: 1. Softening in the thrombus itself; 2, the formation of processes of the same, the latter being exposed to a free current. 3. All causes which accelerate the circulation of the blood, and particularly those which increase the general pressure of the blood, so as to enhance it in the affected locality.

"The forms and elements of embolic bodies are extremely various; they are most frequently of a simply fibrinous nature, and as such undergo the most diverse metamorphoses; soften, become fatty, ossify, or organize to form connective tissue, &c. More rarely they are constituents of tubercle or carcinoma; most rarely of all are they entozoa. The entrance of air into the current also produces phenomena perfectly identical with those of other emboli of whatever kind. I have, however, never witnessed in living individuals spontaneous fermenting processes in the blood capable of leading to the formation of freely circulating gases and so to coagulations." (p. 662.)

"While the products proceeding from the venous system meet with their final and impassable limits in the capillaries of the lungs, those of the arterial system in the periphery of the body, those of the branches of the vena portæ in the liver, those developed in the lymphatic and chylous currents are early arrested in the glands. Only in rare cases will the thoracic duct convey solid coarse bodies; in cases, in fact, where the glands themselves are deeply affected, and give rise to pus and other inflammatory products, carcinoma, or tubercle." (p. 662.)

"Bodies remain within the vascular canals and the two ventricular cavities, without producing anatomical or functional disturbance of the latter. Arhythmic movement of the heart, syncopal lesion of its action, are entirely results of further secondary disturbances, and are never the effect of direct endocardial contact. It is only in rare cases, where a thrombus happens to interweave itself among the trabeculæ of the heart, that it occasionally gives rise to local inflammation and even to the formation of abscess. Such observations are exceedingly rare, and are not satisfactorily authenticated." (p. 663.)

We must distinguish two forms of emboli: 1, those of the larger and smaller vessels; 2, those of the capillaries. There are secondary processes, which can be explained only by assuming the existence of the latter, and in many instances this has been directly demonstrated, both by the microscope and by experiment. Only in rare cases and in particular organs are the secondary products identical. Capillary emboli present this peculiarity, that they never occupy the entire capillary system of an organ, but that when present to the greatest extent they still permit the circulation of free currents. The deposit of pigment after intermittents, a great proportion of the pyæmic metastases, and especially acute endocarditis, are the lesions which afford us examples of these forms with a certain constancy.

The results of emboli are: 1, the immediate effects of the simple interruption of the current; 2, the independent peculiar series of metamorphoses of the impacted bodies *per se;* 3, the phenomena of irritation of the adjoining vascular and parenchymatous elements to which they give rise; 4, *vice versâ*, those of the reflective influence of a secondary alteration of the parenchyma on themselves; and lastly, 5, general morbid conditions of the system as a whole, whether through the disturbance of function of an organ, the sudden so-called plethora of the corresponding column of blood, or the resorption of elements rendered purulent or gangrenous.

The phenomena of simple interruption of an afferent current differ according to the following circumstances: 1, according as the embolus occupies a trunk and its branches or a capillary vessel; 2, according as the plug renders the caliber of a vascular canal quite impermeable, or still permits a current for a longer or a shorter time; 3, according as collateral action is possible or not; 4, according as the functional or nutritive current, or both together, are interrupted; lastly, 5, according to the specific function of the affected organ itself, its amount of blood, its solidity, and its importance to the system at large.

The results of the absolute closure of an afferent current are: anæmia, anæsthesia, paralysis, depression of temperature, arrest of function, diminution of turgidity and of volume of those parts supplied by the vessel, loosening of their elements. A condition accordingly sets in, which has been correctly termed a local cadaverization. Where sensitive nerves exist in the immediate neighbourhood of the obstruction, they become irritated by the constant pulsation of the column of blood above the point of obstruction, producing hyperæsthesia and very violent neuralgia. The arteries collapse behind the obstruction, a thrombosis almost always forms, usually extending to the next superior current of any size, and generally presenting a conical surface, with the apex turned towards the heart. This thrombosis is more voluminous and is more rapidly developed in proportion as the individual is wasted and the blood is richer in coagulable fibrin. The venous current is next retarded and eventually wholly arrested, the vascular walls collapse. This diminution of the canal is, however, not possible in all organs. In the lungs, for example, it can only partially take place, as inspiration exercises a dilating influence on the venous walls; and in the brain it is conceivable only when the corresponding space is filled with serum. In the brain the systems of vessels are so distinct, that the obstruction of one never induces so active a collateral circulation as to create vicarious œdema in the neighbouring parts; the veins here can consequently not collapse, but must on the contrary become varicose. It is extremely rarely that the blood flows backwards out of the veins situated next above; this takes place in fact only when the nutrition of the venous wall has been early impaired, or the surrounding parenchyma has become decidedly atonic and necrosed; in such cases this secondary supply of blood may be followed by excessive dilatation of the veins, and even by hæmorrhage and œdema. If an organ is long cut off from all connexion with the general circulation, necrosis sets in, the cellular elements break up, blood is effused, becomes decomposed, and mixes with the remains of the parenchymatous elements to form a more or less dark-coloured pappy mass, which becomes a focus of inflammation. The whole process is called the mummification of dry gangrene. The connective and elastic tissue is that which longest resists destruction.

In the nervous tissue (softening of the brain), the medulla of the individual primitive fibres is seen to coagulate; these become turbid, varicose, and their contents are occasionally metamorphosed into fat. Cartilaginous tissue loses its normal lustre, and in its cells we find fat and pigment. In the bones the Haversian canals are narrowed and pigment is deposited, particularly in the inter-corpuscular substance.

Capillary emboli produce essentially different effects. They are either harmless, when they obstruct only a few capillaries of a system, or they develop hæmorrhages under two quite different conditions; either the injected mass possesses in itself properties mechanically or chemically so irritating that immediate perforation and free extravasation follow every embolic act, or the lateral pressure in the free capillaries in the same system becomes suddenly so much more powerful, that an analogous result ensues. The name of hæmorrhagic infarction is given to both conditions.

This last process needs some explanation with reference to its anatomical basis. The older opinions, that it is the result of capillary phlebitis for the most part hypothetical and seldom distinctly demonstrated, or that such infarctions are absolute capillary stases caused by emboli or by products of fermentation in the blood, are both false, neither satisfactorily proved, nor at all probable. The first view depended partly on Cruveilhier's well-known proposition: *La phlébite domine toute la pathologie.* As the latter theory has been shown to be unfounded, the hypothesis of the formation of infarctions based upon it must likewise fall to the ground. Usually the afferent veins proceeding from an infarction contain fluid blood, seldom thrombi. The circulation within these vessels in most cases still continues, consequently secondary arterial thrombosis is by no means constant. The peculiar further processes which we can trace in an infarction from its first stage of the black projecting knot, to its cicatrization or suppuration, are not conceivable without an at least partially open current. In cases, too, where a secondary arterial thrombosis is developed, a certain circulation is kept up by collateral currents. In this instance also, the venous blood within the infarction is in a fluid condition. Almost for the same reasons the idea of an absolute capillary stasis caused by emboli is to be rejected. Something, blood or exudation, must be added, pressing the decomposing tissues aside, and with their remains forming a resistance in the projecting knot.

The conditions which have hitherto been called infarction are of three kinds: 1, *simple extravasations;* 2, *inflammatory exudative infarctions;* specific emboli, such as especially purulent or ichorous products, rough, mechanically irritating endocarditic or calcareous particles, readily develop infarctions of this series in situations where the resistance of the parenchyma offers a relatively greater obstacle to hæmorrhage. In this manner the most frequent form of the well-known pyæmic development of abscess appears to arise. If to this, hæmorrhage from the corroded little vessels adjoining or within it supervenes; or if, on the other hand, to the simple extravasations above considered inflammatory products are eventually superadded, a third form arises, which has been justly termed, 3, *hæmorrhagic inflammatory infarctions.* These are by far the most frequently observed.

Some organs have a double vascular system; one exhibiting a purely functional circulation, the other an almost wholly nutritive current. They have sometimes even a distinct venous outlet; for the most part, however, the two currents coalesce in the capillary structure itself. Where such peculiarities exist, the following laws prevail:—Whenever emboli affect the nutritive current, texture and function suffer simultaneously, the latter only mediately, inasmuch as its physiological normality is possible only with perfect integrity of nutrition. If, on the contrary, the functional stream is excluded, only arrest of function ensues; and as every inactive parenchyma usually atrophies after a longer or shorter time, simple atrophy presents itself in this case also mediately and gradually. In such instances we never meet with necrotic degeneration; on the contrary, there is frequently hyperplasia of the interstitial connective tissue. The function of organs is therefore dependent on a normal condition of both currents; their nutrition, on the contrary, is connected more intimately with the nutritive than with the functional stream. The former may gradually act vicariously for the latter; the latter, on the contrary, can scarcely supply the place of the former, because it usually contains venous blood. That in such a vicarious action of nutritive currents the functional product is essentially different from the normal, is not only probable, but has often been proved.

When with embolism of an afferent trunk, blood is still able to penetrate into the affected organ, either through imperfect closure of the vessel, or by the establishment of a collateral circulation, or finally, through accessory, functional, or nutritive currents, the further effect is first of all modified in the following manner: either the above-mentioned lesions (anæmia, anæsthesia, paralysis, &c.) set in equally—this rare case is observed only in emboli of the larger pulmonary branches, of the cerebral carotid, or of some of the arteries of the extremities, or at least some of these lesions occur—or, lastly, a peculiar hyperæmia arises, leading in some instances to œdema and extravasation. This latter case happens, according to Dr. Cohn's experience, only where *one* efferent vein exists for both the main and the collateral currents, and especially when embolism of a nutritive current is in question. In the first case there is usually coagulation of the blood of the capillary veins, and this must of course exercise an obstructing influence on the collateral vessels. In the second case there is previously a collapse, an atony of all the parenchymatous elements. This profound textural disease leads necessarily to arterial congestion, on the one hand; and to venous, cyanotic, œdematous swelling on the other. The less the force of the collateral action, the more certainly will the latter appear, and *vice versâ*. Usually there is loosening and atony of the textural elements. If the collateral circulating columns are strong, these elements may not only be quite restored, but not infrequently hypertrophy ensues, as such a circulation has a tendency to exceed the physiological limits; if, on the contrary, the circulation is too weak, persistent complete softening, or proper vegetative death, ensues.

The structure and function of an organ, its amount of blood, its degree of solidity, its relations to adjoining organs and to the system at large, exercise an important influence. 1. The more abundantly an organ is supplied with blood, the more difficult will it be to compensate for the loss of the principal source of that supply, the more easily and more rapidly will the organ mortify, and the greater will be its tendency to putrid gangrene, rather than to mummification. 2. The more solid the parenchyma is, the stronger the interstitial or peripheric capsular connective tissue, the less readily will the development of collateral connexions take place, and the greater will be the security against hæmorrhagic processes from the latter. 3. The function of an organ has its influence. Not infrequently specific products are developed, which have a peculiar effect on the process, on the metamorphosis of its products, &c. Of this kind are urinary elements in the kidneys, biliary congestions in the liver, abnormal ferments in the digestive canal, &c. 4. The relative situation of an organ; and 5, the general state of the patient's health, will obviously have their influence.

But the embolic matter may be of a specific character, and may mechanically or chemically irritate the vascular wall with which it is in contact, producing inflammation of the wall, formation in its coats of a purulent exudation tending to perforation, secondary inflammation of the surrounding parts, &c. 2. The embolic body may soften and break up in simple or fatty detritus, or by secondary disease of the surrounding parts. 3. The *vis a tergô* may perforate the embolus, an event which may be followed by either of two very opposite results—(1), the circulation may again become freer, with restitution of function and nutrition; or (2), portions of the plug of various sizes may be swept away, to form fresh emboli in more peripheric arteries, or in the capillaries. Thus, an embolus of the internal cerebral carotid artery admitted of relative integrity of the life of the brain during six months, when the subsequent separation of particles and formation of new emboli in the arteria fossæ Sylvii in a few days produced a fatal result by softening of the brain.

"In capillary embolism, too, the specific nature of the emboli may produce particular results. Thus, simple capillary emboli have been found to produce either no disturbance or infarctions, but if they possess a specific irritating nature, inflammation, simple lobular abscess, and metamorphosis of the infarction to hæmorrhagic abscess may ensue. If the capillaries afford great resistance, if the texture of the organ is very firm and dense, not readily admitting of extravasation, a process analogous to inflammation sets in in the hyperæmic structures, and lobular metastasis, with rapid transition into necrosis and abscess, takes place. But the formation of abscess in cases of capillary emboli may occur also in another mode; that is to say, the blood poured freely out of the vessel in a primary infarction may contain in itself the specific purulent elements, and these may have a fermenting influence, just as the poison of a corpse may gradually develope a considerable pustule. Corrosive matters external to the vessels always act more intensely than when within their canals; the elastic walls of the latter offer an enormous resistance.

"In experiments with quicksilver, in which the vessels have been perforated, and the metal has been brought into immediate contact with the parenchyma, much more intense inflammation is developed than is caused by a plug contained in the arteries, even when the plug is impregnated with tartar emetic or chloride of zinc. The metastatic abscess caused by capillary emboli must, in reference to its mode of formation, be distinguished from the kind developed by purulent emboli of larger vessels. In their final anatomical result they are, however, perfectly identical. Whether and how far carcinomatous or tubercular metastases are connected with corresponding capillary emboli has not yet been satisfactorily ascertained." (p. 678.)

Embolism exercises its influence on the system at large. This takes place, 1st, through the arrest of particular functions not unessential to the integrity of the organism. 2nd. Through the irritation of sensitive nerves in the affected organ, and their reflex relations to other parts and functions, producing ague, fever, convulsions, neuralgias. 3rd. By the sudden diminution of the circulating current, and the backward surcharge and plethora so developed, giving rise to congestions, syncope, asphyxia. 4th. By softening, gangrene of organs, and the consequences of resorption: septithæmia, fresh emboli. Most of these relations are manifest. Softening of the brain, embolism of the pulmonary artery or vena portæ, must necessarily affect the general economy so deeply, that fresh morbid conditions cannot be remote.

The *diagnosis* of emboli may in general terms be said to depend on —1. The recognition of the source whence they may be formed. 2. On the demonstration of the local diminution or sudden and total disappearance of the original thrombosis, as well as on the occurrence of atypical rigors, with or without swelling of the spleen, with perfect febrile remission. 3. On the sudden occurrence in the function of a given organ of a series of disturbances capable of being fully, though it may be not exclusively, explained with reference to emboli.

The indications of *treatment* are—1, either to dispose to the total solution of the deposited embolic body, or to its higher organization into the in general much less injurious filaments of connective tissue. But as by the artificial production of marasmus (hunger- and inunction-cure), we should more frequently lead to softening, often to purulent and ichorous destruction, than to the desired total solution of the embolus—that is, to secondary, new, and very pernicious forms, the organization to connective tissue remains the only desirable metamorphosis. The more powerfully the force of the heart presses a fibrinous plug against the vascular wall, and the more healthy the individual is, the more certain is such organization. Hence our object should be to give tone to the tissues, and to stimulate the innervation of the heart. The *second* indication would be to compensate for the loss of the direct, by the establishment of collateral circulation; but as it appears to be a tendency of the latter far to exceed the normal limits, and that not infrequently hæmorrhages and many other often inflammatory disturbances of nutrition are so produced, we should be led, in opposition to the first indication, to seek in the depression of the heart's action, and of the lateral pressure, the possibility of the restitution of an adequate circulation. But it is only by the attentive observation of each case that we can hope to form a rational treatment. The development of a collateral circulation, which requires a certain time, is essentially connected with the general state of nutrition. The stronger the constitution, the more rapidly will such a development take place. It must therefore be our aim to avoid all injurious influences capable of leading to a general or local disturbance of the circulation. It is only in cases of plethora, where there is threatening œdema from collateral hyperæmia, with co-existent hypertrophy of the heart, and where we have to contend with a very early stage of the process, that it would be allowable, by the direct abstraction of blood, local or general, by cold applications to the præcordia and to the affected part, as well as by strong derivation to the periphery, to attempt to depress the vascular irritation. Digitalis is, under such circumstances, for the most part inefficacious; its desired action is too uncertain and usually sets in too late. Where, on the other hand, anæmia or profound cachexy, the result of previous disease, already exists, where there is stenosis of the ostium venosum sinistrum, where nutritive degeneration of the muscular structure of the heart essentially impairs the power of the organ, where all febrile or other vascular excitement is absent, where the series of phenomena, which had already commenced to improve, becomes stationary or even unfavourable, where the existence of atheroma of the vascular system has been proved or is probable, success can be expected only from a stimulating treatment. In individual cases, the mode of treatment will vary according to the particular symptoms; the same embolic softening of the brain may to-day so increase in danger by presenting indications of peripheric inflammation, that a powerfully derivative treatment may appear to be indicated; and yet in a few hours complete cerebral paralysis may follow in its track.

But we have also, in the *third* place, to direct our attention to the more general sudden or gradual disturbances of the circulation which may occur. Thus, an enormous overloading of the cavities of the heart with blood, along with paralysis of this organ, may be impending, and leave us for the time no resource but in the powerful remedy of venæsection. In chronic cases a derivative treatment will be the most efficacious. Where a specific alteration of the blood has been produced by embolism of any organ, especially of a secreting organ, we can meet it only by, if possible, directly combating the specific poison, or by a tonic treatment with quina and iron, combined with the best possible diet.

The *last* indication is, to prevent the return of the occurrence. All causes capable of quickening the circulation of the blood, or of increasing its pressure, every mental or corporeal excitement, every sudden or violent exertion of the muscles surrounding a vessel filled with soft thrombi, should be avoided. Digitalis, quina, and opium have, under such circumstances, in Dr. Cohn's hands, proved the most efficient agents. Local treatment by compression, cold applications to the region of the heart, absolute rest and the horizontal position, the avoidance of heating drinks and of inflating or too rich articles of food, are particularly to be recommended.

We have thus endeavoured to lay before our readers as full a sketch as our limits permitted, of the history and bibliography of the theory of emboli, as well as of the pathology and treatment of the lesion. In doing so we have borrowed most largely from the work of Dr. Cohn. We have done so because his volume is, we believe, not only the most recent, but also the fullest and most comprehensive which has appeared upon the subject. We have been obliged, however, to confine ourselves to a general statement of results, and to pass over, without more special notice, the details of the numerous valuable original investigations of the author.

The Scandinavian works whose titles we have placed at the head of this Article, are excellent and faithful *résumés* of what was known of the subject of this Review at the period of their publication; and from them also we have derived much valuable assistance in the compilation of this notice.

A few words, in conclusion, as to nomenclature. It has been proposed to substitute for the term embolus, plural emboli, the neuter noun embolum or embolon, plural embola. We do not see the advantage of the proposed change. Embolus = εμβολος, *cuneus qui immittitur in aliquid* is, we think, sufficiently expressive. The word embolism, which we have occasionally used in the present notice, as well as in a former volume of this 'Review,'* is perhaps, unscientific as it may sound, a fair translation of the German *die Embolie.*"

Review VI.

1. *Du rôle de l'Alcohol et des Anesthétiques dans l'Organisme; recherches expérimentales.* Par Ludger Lallemand et Maurice Perrin, Médecin-Majors, &c., et J. L. P. Duroy.—*Paris,* 1860. pp. 432.

On the Actions of Alcohol and Anæsthetics in the Organism; Experimental Researches. By MM. Lallemand, Perrin, and Duroy. —*Paris,* 1860.

2. *On Chronic Alcoholic Intoxication.* By W. Marcet, M.D., F.R.C.P., Assistant-Physician to the Westminster Hospital.—*London,* 1860. pp. 172.

3. *On the Action of Foods upon the Respiration during the Primary Processes of Digestion.* By Edward Smith, M.D. ('Transactions of Royal Society,' 1859.)

It is a common observation, that whilst our knowledge of disease has, during the present century, advanced more than in all previous ages, our acquaintance with the means of combating it is scarcely if at all better than that of our forefathers. This is not strictly true in respect of empirical treatment: our more correct diagnosis, and the joint value given to experience by statistics, certainly enable the modern practitioner much more often to apply the right remedy in the right ailment than of yore. But the accusation has some justice on its side as against the scientific part of Therapeutics. The classifications of the last century, with their expressive vagueness, redolent of the half truths of many an exploded theory, fairly represent still our knowledge of materia medica. Any attempt to better the nomenclature is only useful as an act of humility by showing us our ignorance.

The reason of this is partly that our ancestors had observed much more about the action of drugs than of disease; pathology had lagged so far behind, that when once started it has been making up lost ground by gigantic strides, while therapeutics, being more on a level with the then existing state of science, has not had so far to travel. But a stronger reason lies in the difficulties of the subject; the most essential part of the influence of physical agents over life lies in their action upon formative nutrition, and the instrument they employ to effect this is usually the nervous system, at once the most impenetrable as to function and the most mysterious tissue of the animal body. To repress over-sanguine hopes of rapid advance, to show what pitfalls lie in the path, and what ignes fatui of mechanical, chemical, and cellular

* Vol. xviii. p. 325.

theories will try to delude us and our children, it is proposed in the present article to sketch a history of the investigations made during our own generation into the action of a drug which, from its wide-spread use and abuse, has justly excited more attention than any other. Let us remember that what has been done for alcohol, and much more too, must be done for all our materia medica before we can realize Willis's dream of a Pharmaceutice Rationalis.

The Absorption of Alcohol.—The earliest experiments on alcohol demonstrate that it is capable of absorption by other tissues than the mucous membrane, and that when thus absorbed it produces the same effects as when introduced into the system in the ordinary way. Fontana produced paralysis and death in frogs and turtles, equally by injecting alcohol beneath the skin as by administering it by the stomach.* M. Roger produced drunkenness in rabbits by injecting alcohol into the peritoneum;† and M. Orfila, by the vapour of alcohol given through the respiration. But still, both he and Brodie, in 1811,‡ found that introduced by these unnatural paths it acted less energetically and less quickly than in the stomach, and hence they conclude that its main poisonous action is on the extremities of the nerves, by whose agency it paralyses the brain more immediately than through the blood, producing in this way that shock which proves fatal when very excessive doses are taken into the stomach. But it may be observed that these experiments do not affect the question whether the other intoxicating effects, independently of the shock, may not be mainly induced by absorption.

About this time chemistry, having proved itself a good servant to physiology, was sometimes allowed to usurp the rank of mistress. It was observed that alcohol coagulated albumen, and hence it was hastily concluded that it could not enter the living blood without arresting the circulation and mechanically causing death. To account for its disappearance from the intestinal canal, MM. Learet and Lassaigne,§ in their researches published in 1825, knowing that at a temperature above 68° diluted alcohol when mixed with organic matters undergoes acetous fermentation, suggest that this change takes place in the stomach, and that spiritous liquors are taken into the blood as vinegar. Magendie‖ showed afterwards that it is only concentrated alcohol which causes death by coagulating the albumen of living blood, having injected sixteen grammes of half-and-half brandy and water into a dog's jugular vein without injuring the animal, though pure alcohol acted as an instantaneous poison. A more complete evidence was afterwards exhibited by Dr. Ogston, who detected alcohol by its smell in the ventricles of the brain, and soon afterwards by Dr. Percy in his Thesis,¶ published in 1839, in which he announces that in some

* Treatise on the Venom of the Viper, vol. ii. p. 371.

† Dictionnaire de Médecine et de Chirurgie pratique, tom. i. p. 291.

‡ Philosophical Transactions for 1811: On the different Modes in which Death is produced by certain Vegetable Poisons.

§ Recherches physiologiques et chemiques pour servir à l'histoire de la Digestion. Paris, 1825.

‖ Leçons sur les phénomènes physiques de la Vie, tome iii. p. 55.

¶ An Experimental Enquiry concerning the Presence of Alcohol in the Ventricles of the Brain.

experiments on intoxicated animals he was enabled, by means of its smell and combustibility, to trace alcohol in the fluid distilled from the blood, the bile, the urine, the liver, and the substance of the brain. He states also, that the quantity found in the latter situation was greater than elsewhere, so that "it would almost seem that a kind of affinity existed between alcohol and the cerebral matter." It is singular how little attention these researches of Dr. Percy seem to have attracted. The notion of the conversion, as a rule, of spirit into acetic acid during absorption, seems to have got complete hold of the minds of Continental physiologists, in spite of a remark by the sagacious M. Bérard,* in 1848, that the phenomena of drunkenness prove to his mind that its exciting cause is taken up unchanged into the nervous tissues.

The only test employed to detect the presence of alcohol in the above quoted researches, were its odour and inflammability. Every chemist's nose is not so keen as Dr. Percy's, and people are apt to question the existence of a faculty which they do not possess; while sufficient distillate to burn can only be obtained when excessive quantities of spirit are ingested. It was therefore a considerable step in advance when Dr. Strauch,† under the directions of Professor Buchheim, employed chemical reagents for its detection; though these are not perhaps entirely conclusive as tests, as they proved too much. He found alcohol in the urine of an intoxicated dog, as well as in the various parts of killed animals.

Partly, perhaps, the evidence of the passage of alcohol through the system was overwhelmed by the great names of Tiedemann, Gmelin, Woehler, Royer-Collard, Bouchardat, and Sandras, whose sensations were less sharp than Dr. Percy's, and who were unable to detect it in the renal secretion by the smell. But the most powerful agency doubtless was the brilliant and broad-viewing mind of Liebig, to whom the chemical constitution of the substance, its inflammability, and obvious incapacity for forming tissue, naturally suggested its classification as a food taken for the purpose of keeping up the animal heat by its combustion. Now, if it is to be burnt or oxidized, the steps of oxidation require to be described, and the world hailed as a complete account of the transaction the researches of Duchek,‡ who, in 1853, led us to the conclusion that alcohol goes through a defined series of oxidation, first becoming aldehyde, then acetous, then acetic acid, then, perhaps, partly oxalic acid, and, finally, carbonic acid, producing at the same time, of course, a considerable quantity of water by the hydrogen which it parts with in its various transformations.

This elicited in the course of the next year a reply from Rudolf Masing,§ another pupil of Professor Buchheim's. In the first place, he answers that the principal test used to declare the presence of aldehyde and the absence of alcohol was the odour. This, he says, is

* Cours de Physiologie, fait à la faculté de Médecine de Paris, tom. ii. p. 518.

† De Demonstratione Spiritus vini in corpus ingesti. Dorpati Liv. 1852.

‡ Ueber das Verhalten des Alcohols im Thierischen Organismus: Vierteljahrsschrift für praktische Heilkunde, zehnter Jahrgang, Band iii.

§ De mutationibus spiritûs vini in corpus ingesti. Scripsit Rudolphus Masing. Dorpati, 1854.

very fallacious, for when mixed with animal tissue the distinction is difficult to make. Moreover, he himself and others, when they detected any spirituous smell at all, thought it more like spirits of wine. Secondly, the dogs experimented on by Duchek were killed by the shock of the poison before it had time to get into the tissues. Thirdly, he denies that the production of metallic silver from nitrate of silver and ammonia, is any sure test of the presence of aldehyde, inasmuch as in one of his experiments on a dog who had taken no spirit, the distillate from the tissues exhibited this reaction.

Masing did not confine himself to criticizing his predecessors, but applied to the search for alcohol in the tissues a series of experiments which appear fairly conclusive as to the fact of its passing unchanged through the system. The tests he used, and the line of argument to which he applied them, are as follows:—

First, he finds that a stream of air, at a moderate temperature, passed over different parts of the body of animals killed without having alcohol administered to them, has no effect in deoxidizing chromic acid into oxide of chrome. The test-liquor he used consisted of one grain of bichromate of potash dissolved in ten drops of sulphuric acid.* It retained its red colour unaltered. Secondly, he finds that when the animals have had alcohol administered to them before death, on the application of the same test the red fluid very rapidly assumes an emerald-green hue. Thirdly, this easily vapourizable product he finds not to be aldehyde, by the test previously used by Duchek; for though the precipitation of metallic silver is no proof of the presence of that substance, yet its non-precipitation is a proof of its absence. Fourthly, he shows also that it is not acetic acid.

The argument, therefore, is advanced to this point, that after the ingestion of alcohol, and then only, something is absorbed into the tissues which smells like alcohol, which burns like alcohol, which has the chemical reaction of alcohol, and which is not aldehyde or acetic acid. The burden of proof seems to lie with those who can show it to be something else.

By the application of the same test he and Strauch found alcohol also in the urine of dog and man, and Schlossberger detected by the smell methylated and amylated alcohol in the blood and urine.

In præscientific times the presence of alcohol in the breath was never doubted. In the "Beggar's Opera," Peachem saluting Mrs. Trapes, says—"One may know by your kiss that your ginn is excellent." We are not surprised, therefore, to find even MM. Bouchardat and Sandras acknowledging this from olfactory evidence, and Professor Buchheim showing it by the chromic acid test.† We believe Dr. Dundas Thompson seven or eight years ago also published his discovery of alcohol in the urine, but we cannot lay our hands on his paper.

Quite recently, MM. Lallemand, Perrin, and Duroy have repeated Masing's experiments. We wish they had quoted his treatise, and not

* This test is derived from the experience of dyers, who use alcohol to form the green oxide of chrome in the manufacture of chrome alum.

† Masing's Treatise, p. 33.

led others by their language to the inference that they claim the merit of being the first to apply the chromic-acid test to physiological chemistry. More than one distinguished physiologist, and more than one lecturer on medicine, have been thus led into error. Yet Mr. Masing's treatise is seven years old, is well-known in Germany, and has been quoted in this Review. We regret that the authors have not named their predecessor in the same path, but we must feel grateful to them for transporting us from the cloudy regions of crabbed Latin into the logical luminosity of their native tongue. The arrangement of the book is excellent, the illustrations of the forms of instruments used most graphic, and the chapters of social, pathological, and therapeutical applications very pleasant reading. We miss, however, in the latter an acquaintance with the original labours of Boecker, Hammond, and others of recent date, which would often have enforced and sometimes modified their dogmas.

We owe to the repetition of these experiments by the French chemists the observation of how very moderate quantities of alcohol can be detected in the excretions. If a man swallows a tablespoonful of brandy or a glass of wine, and then half an hour afterwards breathes for five minutes through the test apparatus, or analyzes his urine, both secretions are found to contain the drug. It is not merely *an excess* which passes through the system, a very important consideration to bear in mind. We owe to them also the knowledge that even through the dog's tough hide and hair the delicate vapours will pass. But by trying it on such an animal, they have been led probably to underrate the action of the skin in our naked race. They had not experimented on man, and therefore believe the cutaneous exhalation to be more inferior than it really is to the pulmonary. *Apropos* of a paper on this subject at the Society of Arts last January,* Dr. Edward Smith enclosed in an india-rubber bag the arm only of a gentleman who had taken a glass of brandy, and passing a current of air through it, got evidence of the exhalation of alcohol quite as quickly as by the breath.

Messrs. Lallemand, Perrin, and Duroy, in adopting Masing's method, have attempted to carry it much further than he considered desirable—namely, to make it a quantitative as well as qualitative test. At this stage of the inquiry, therefore, we ought to pause and examine its exact value. From experiments made in Dr. Chambers' laboratory at St. Mary's Hospital by Mr. Hall Smith (see the "Original Articles" at the end of this number of the Review), it would appear most ill-suited to demonstrate the amount and only approximatively demonstrative of the presence of alcohol. Several other organic matters present in the dried vapour of urine, in the breath, and often even in the air of a room, which could not possibly contain alcohol, will, after various intervals of time, change the test liquor from red to green. It is only by its *rapid* conversion, and by the brilliant emerald tint of the liquid, that evidence is afforded. Moreover, it appears from these experiments that the adherence of alcohol to aqueous solutions is much stronger

* Journal of the Society of Arts, Jan. 19th, 1861.

than is usually reckoned on, and that it remains at the temperature of 140° in distilled water till the whole is evaporated. Now it is evident that rapidity is a question of degree, that it is impossible to fix a moment at which you are sure you have alcohol present, and a later one at which you are sure you have not—that to draw a line of demarcation in the transitional tints from emerald green to apple green is equally impracticable—and that in substances like organic matters, it is impossible to say when all the spirit is gone. On these grounds we should recommend great caution in the use of the test, and the rejection of all idea of making it quantitative.

Physiological Action of Alcohol.—It is clear that we must cease to regard alcohol as in any sense an aliment, inasmuch as it goes out as it went in, and does not, so far as we know, leave any of its substance behind it. It remains for some hours in the body, and exerts in that time a powerful influence. What is that influence, and over what tissues is it exerted? "A stimulant to the nervous system." On the nervous system doubtless, and especially on the mental functions of the nervous system, every experimenter, from the first patriarch downwards, would agree that its prime action is evident. But what is a stimulant? It is usually held to be something which spurs on an animal operated upon to a more vigorous performance of its duties. It seems very doubtful if, on the healthy nervous system, this is ever the effect of alcohol, even in the most moderate doses and for the shortest periods of time. In a series of researches made with another object, Dr. Edward Smith has recorded very minutely the sensations experienced after brandy by a temperate man with a fasting stomach.* We find the first noticed to be lessened consciousness, and lessened sensibility to light, sound, and touch. Then there comes a peculiar sensation of stiffness with swelling of the skin, which is noticed particularly in the upper lip and cheeks. This is very unlike a spur to extra exertion. In a patient at present under our care, the same peculiar sensation of stiffness, and also the objective phenomenon of rigidity of skin without loss of sensation, is produced by the pressure of injured bone on the fifth nerve in the skull.† It is a partial paralysis of a sensitive nerve, and cannot in any sense be considered an increase of vigour.

Dr. Smith also records among the "early effects" a relaxation of the dartos and other muscles connected with the reproductive system, which Ovid elegantly and Shakespeare coarsely assign to the later and more obvious influences of drink, "*Vina parant animos Veneri,*" but not bodies, as they "take away the performance." The sphincter of the bladder also was relaxed, and to this the observer attributes the increased micturition during indulgence.

It is true that there is noticed also an increased rapidity of pulse;

* Transactions of the Royal Society, 1859, p. 732.

† In this lady another facial characteristic of intoxication is exhibited. Ordinarily, the affected side has a slight diffused fixed colour, the other being pale: but when she blushes, the healthy cheek is coloured quickly and deeply, while the diseased one remains unaltered. This is analogous to the unabashed front of an early stage in alcoholic exhilaration.

but that cannot be regarded as an evidence even of locally-augmented vital action, for of all patients those especially exhibit it who have the weakest hearts and are most enfeebled by disease. A diminution of force is quite consistent with augmented quickness of motion, or may it not be said that in involuntary muscles it implies it? The action of chloroform is to quicken the pulse, yet the observations of Dr. Bedford Brown,* on the circulation in the human cerebrum during anæsthesia, clearly show that the propelling power of the heart is diminished during that state. The heaving and bulging of the brain were quieted during a severe operation on the skull, the surface became pale, and the hæmorrhage ceased.

The rate of respiration was in almost all Dr. Smith's experiments lessened, and though there was a sensation of greater depth of inspiration when attention was drawn to it, yet the variableness in the amount of respired air renders it probable that this is a purely subjective, or at most an irregular phenomenon.

It is unnecessary to go through the symptoms of advanced intoxication by alcohol: physiologists have always taught, as confirmed by all experiments, that large doses immediately, and small doses after a time, depress the nervous centres, and that the cause of death is the cessation of the muscular respiratory movements. What we wished particularly to mark is, that the *primary* action is anæsthetic—a diminution of vitality in the nervous system.

A pithy series of experiments, by Dr. Marcet, were read before the British Association at Aberdeen, in 1859, and are an excellent complement to those by Sir Benjamin Brodie, previously quoted. He ties the aorta of a dog, and shows that alcohol injected into the stomach does not produce intoxication; he ties the aorta imperfectly, and intoxication *is* produced; hence concluding that the spirit acts on the nervous centres principally by being conveyed to them in the blood. Yet are the nerves not entirely non-conductors; for he finds that frogs in whom the circulation is left free die quicker of the poisonous shock when the nervous communications are complete than when mutilated. Very telling experiments these, and crucial.†

The exhilaration of mind is also an anæsthetic phenomenon. It is nothing more than a blunting of the sensations to the little half-felt corporeal pains and the thousand petty cares and ambitions of daily life. The intellect is said to flash forth brighter with wine; but analyze coolly the wit of a convivial party, and you will find it generally as poor as the beautiful poetry you seem to make in dreams, and which will never scan when remembered waking. The exceptions in proof of the rule are cases common enough in literary life, where "the corruptible body presseth down the soul, and the earthy tabernacle weigheth down the mind that museth on many things." The nervous system of a Johnson or a Hood requires to be freed from the hourly burden of pain before it can emit its coruscations.

Probably neither the highest manifestations of bodily vigour, nor

* American Journal of the Medical Sciences, Oct. 1860.

† See Medical Times, March 3rd, 17th, and 31st, 1860.

the most precious productions of the intellect, are elicited by such agency. Yet would the public weal suffer most irreparable loss if it were not in common use. A civilized man, to whom life is of such inestimable value, often acquires and retains that life under conditions which would render another animal's existence impossible. An unsound horse gets shot or used up in a cab; a dyspeptic wolf gets worried and eaten by his friends; but a William of Orange, or a Wilberforce, though they tremble at a breeze, survive to be blessings in their old age to their country. Probably very few of us workers in this busy metropolis would enjoy our average health of mind and body without instinctively obstructing from time to time the paths by which one portion of the frame disturbs another through the sympathetic system or reflex action. Health is not the predominant vitality of this or that organ, not even the abundant vitality of all, but the general harmony and perfect balance of one against the other. And as in a faded picture or piece of dim tapestry we tone down some one spot or colour which has resisted time, in order to bring the whole into keeping, so in the body we may find it good to reduce the life as exhibited in one function, where in the others it is not equally active. Doubtless it is better to resuscitate all the lost tints, but that is easier in pictures than in the animal body. The consequence of toil, mental even more than corporeal,* is to increase destructive metamorphosis; and in a large majority of the population it is apt at times to increase it beyond a rate with which the glandular and digestive systems can keep pace. An over-fagged man loses his appetite, and secretes not enough gastric juice to digest the little he does eat. His expenditure is above his income. True, he may recruit himself by sleep, but several hours are thus consumed, and the increase of the body being postponed, flesh is unnecessarily lost. A more efficacious way is to arrest over-rapid metamorphosis during the latter period of labour or after its conclusion by the instinctively adopted anæsthetic.

We are advised by the Son of Sirach to "deliver all things in number and weight." As regards the effects of alcohol manifested by the nervous system, it is difficult to follow the injunction; but the degree of metamorphosis can be measured by numerals. In October, 1854, an article in this journal showed how far this had been done by means of the labours principally of a German medical man.† Since then an inquiry of the same sort has been carried on by Dr. Hammond, of the United States army.‡

* The Rev. S. Haughton calculates that a man employed in mechanical labour discharges on an average 400 grains of urea daily, of which 300 is derived from vital work—that is, from keeping up the life of the body—and 100 from the muscular exertion. If a man employed in headwork, he estimates the excretion at 533 grains; 300 as before from vital, and 233 from mental work, and the small quantity of exercise necessary to keep the body in health. The numbers are purely approximative, and the subject requires further research. The Dublin Quarterly Medical Journal, Aug. 1860.

† M. Lallemand (p. 212) quoting this review, calls Dr. Boecker "physiologiste anglais." The mistake is not our fault, as we drew special attention to the fact that this true philosopher was during his labours a hardworking physician, surgeon, and accoucheur (as his title-page tells us) in a German village.

‡ American Journal of the Medical Sciences, Oct. 1856.

Dr. Boecker's experiments were on the effects of alcohol when added to a regulated average diet; Dr. Hammond's extended to ascertaining its influence on an under-fed and on an over-fed system—that is to say, when the body was losing weight during deficiency, and when it was gaining weight during excess of nutriment. In repeating for purposes of comparison the German's experiments, he found that in five days, under a just sufficient diet, he grew in weight about half a pound more when he took half an ounce of alcohol with each meal than in five days when he omitted it. This was accompanied by a diminished excretion of carbonic acid from the lungs, and of urea and sulphates from the kidneys. There was a certain degree of disturbance of the general health, impaired appetite, and indisposition to activity. The use of a similar quantity of alcohol was then repeated, with such a diminution in the quantity of food taken as had been ascertained to be capable of reducing the weight at the rate of a quarter of a pound daily. And then came into play the true use of alcohol. It was found capable not merely of arresting this diminution, but even of causing a slight increase in weight; while the health remained perfect, and the feelings vigorous. In contrast to these stand the experiments made while a superabundant diet was adopted. When such a quantity of food was taken as was calculated by itself alone to increase the weight, the addition of alcohol, by restraining the excretions, caused headache and feverishness, heated skin, and disturbed sleep.

It would appear, then, that for perfectly healthy persons alcohol is useful when either the amount of food accessible is insufficient, or when extraordinary exertions make a demand which no ordinary stomach can satisfy. As we said before, perfection of health is rare; and we believe we are right in estimating as a decided minority those who can digest and convert into nutriment sufficient food to supply the calls which the social life of advanced civilization makes upon the body. Such is the conclusion arrived at by Dr. Boecker in a series of experiments on water. "I believe," says he, "for my part, that the majority of men eat too little rather than too much. If our working classes were better fed, they would certainly be less often ill."* If this is true, we may say that alcoholic drinks rightly employed do not indeed enable the world to get more work out of its population, but they enable it to be got with less injury to the individual than would accrue were they not used. And this defence the instinctive and proverbial philosophy of mankind generally attributes to the calmative, gladdening, care-chasing action of these agents on the nervous system.

The diminution in excretion, especially of urea, has by some been attributed to the lessened appetite, and consequently lessened ingestion of food induced by the disturbed health. But, in point of fact, unless a person is in the habit of eating very largely, spirituous liquors in moderation do not diminish the appetite, and Dr. Hammond tells us by weight the quantity of victuals he consumed. Also, it has been said that the preservative chemical effect of the alcohol on the nitrogenous articles of diet prevents their being normally acted upon by

* Untersuchungen über die Wirkung des Wassers. Bonn, 1854.

the gastric and other digestive juices, so that the experimenter has really less to eat, though he swallows equal quantities. As regards Dr. Boecker's and Dr. Hammond's experiments, this is a plausible argument; but we hope in a future number to lay before our readers a series showing that on the continuance for a considerable period of alcohol, its arrestive power ceases, the body becomes habituated to it, and excretes as much, or even more, than before its use. This, of course, could only happen in consequence of a vital act—a resistive reaction. A mere chemical resistance to solution in the food could not be altered by habit.

Moreover, it is found that other drugs, whose only resemblance to alcohol consists in their acting on the nervous system, also arrest metamorphosis, prevent loss of weight during deficient food, and increase it abnormally under full diet. Tobacco-smoking, for example, has been tested by Dr. Hammond, and has been found to produce precisely similar effects on the excretion of water and urea. Opium is well known to diminish the amount of urine, and we cannot suppose that the list is limited to those whose influence is either marked enough to strike the eye, or which have been tested by experiment at this early stage of the inquiry.

Again, there are peculiar states of the nervous system affecting the assimilative functions, in which alcohol does not exert its arrestive power. Under its use, equal quantities of food seem to afford more nutriment, and more metamorphosis occurs. We will cite a single example:—A prostitute, aged twenty-two, had been in the habit, during the year that she had been on the town, of frequent tippling to drown care and deaden her general feeling of ill-health. Standing by her bed one day, she suddenly fell on to it, not from loss of sense, but from paralysis of the right arm and leg, and the right side of the face. Two days afterwards, when brought to the Hospital, her leg had partially recovered, but the arm and face were still paralytic. No drugs were ordered for her; she was kept throughout on broth diet.* The daily analysis of the urine exhibited the following remarkable deficiency of excretion:

Quantity of urine and several of its constituents excreted during twenty-four hours, in grammes and cubic centimetres:

Date.	Quantity in cubic centim.	Specific gravity.	Urea, in grammes.	Chloride of sodium.	Phosphoric acid.	Sulphuric acid.
Aug. 17	252	1018	5·915	1·389	·409	?
19	880	1006	12·729	4·400	·396	?
20	240	1014	4·529	2·040	none	?
21	270	1007	3·429	·337	·061	·206
22	360	1011	5·280	2·340	a trace	·212
23	1000	1007	15·323	3·500	·540	·878
24	1280	1007	14·504	4·480	a trace	·715
27	570	1008	9·405	1·425	a trace	·436
28	1030	1007	10·979	3·862	·494	·596
29	730	1010	9·252	3·285	a trace	·423

* Two pints tea with 3 oz. of milk and sugar; 12 oz. bread; 4 oz. boiled meat; 1 pint broth; ¾ oz. butter; 1 pint gruel.

During the last four days of this period considerable improvement was taking place in the general symptoms, and the patient was able to walk about freely in the ward and use her arm. To test the question whether in a person used to alcohol the addition of it to the food would under these circumstances arrest metamorphosis, she was given three ounces of brandy daily, and the same dietary continued. The effects are shown below:

Date.	Quantity in cubic centim.	Specific gravity.	Urea, in grammes.	Chloride of sodium.	Phosphoric acid.	Sulphuric acid.
Aug. 30	1320	1008	13·645	5·120	trace	1·039
Sept. 2	1650	1008	22·027	7·425	trace	1·358
5	900	1010	13·231	2·925	·162	·790

At this date an opportunity offered of improving her social position, which removed her from observation, though the paralytic condition was not entirely cured. But there is sufficient evidence in the analysis of the urine on these three days to show that an anæsthetic to the nervous system is not necessarily in all cases an arrester of vital action. The quantity of urine, of solid matter in it, of urea, of chloride of sodium, of sulphuric acid, is steadily augmented. The phosphates truly are remarkably deficient, but that may depend on a pathological state which this is not the place to notice.

On the whole, then, we think we are justified in attributing the temporary arrest of metamorphosis caused by alcohol mainly, if not entirely, to its anæsthetic action on the nervous system.

We have been careful in what has gone before to avoid the use of a short word which, for convenience, is often employed as synonymous with "destructive assimilation" and "metamorphosis." Much as we love Saxon etymologies, there is danger in the double meaning of "waste." It unconsciously suggests the idea that all diminution of excretion is a saving and direct gain. In point of fact, Dr. Donders has gone so far as to call alcohol a savings bank. Now we must not forget that metamorphosis is life, that the arrest which we cause for temporary purposes is an arrest of life, and that it is beneficial only when it enables the body more easily to lay in supplies of nutriment. When it oversteps this point, when the arrest is so continuously prolonged that no reaction has time to take place, a step towards death is taken, disease must and does follow. The use of alcohol is a short stoppage to take in stores which will enable us to go on faster afterwards; the abuse of it is to slacken more and more the pace of vitality, till it stops altogether.

Life and warmth are so closely connected together in scientific as well as popular notions, that perhaps the most striking evidence of diminished vitality is in the lessened power to generate heat. MM. Dumeril and Demarquay* published in 1848 their observation that intoxicated dogs exhibited a great loss of temperature; and Dr. Boecker and Dr. Hammond find the same result from even moderate doses. This accords with and explains the experience of Dr. Rae that

* Recherches experimentales sur les modifications imprimées à la temperature animale par l'éther et le chloroforme. Paris, 1848.

alcoholic drinks give no satisfaction to Arctic voyagers, and of Dr. Hayes (Surgeon to U.S. second Grinnell Expedition), that they actually lessen the power of resisting cold.* The "warming of the stomach" seems to be a mere insensibility to cold, and the flushed face and palm a secondary feverishness.

Alcohol alone has hitherto been the subject of discussion, and it may be taken for granted that it is really the most important among the ingredients of the drinks which contain it. But we must not forget a point to which we particularly called attention in a former article—namely, that "alcoholic drinks are not all alcohol," that sugar, gluten, foozel oil, flavouring ethers, and ferments enter into their composition as well, and very much modify their effects. In an article also on Wines and their Adulterations, attention was drawn to the chemical differences between new and old, good and bad, genuine and false productions of the vat and the still. It was pointed out how highly mankind valued these ethers, what a high price they paid for the rare sorts of the juice of the grape, often in inverse ratio to the quantity of spirit. In short, that as regards wines, the principal use of alcohol is to preserve and produce the ethers. To a certain, but much less extent, this is the case with distilled spirits and beers also. It is universally acknowledged by the market price that there is a great difference, quite irrespective of the alcohol in them, between one kind of drink and another. And that this estimate depends not only on taste, whim, or fashion, is shown by its being shared by medical men, who in their treatment of patients, care but little about the flavour of their prescriptions, yet even in hospital practice prefer the costly Port wine to the cheap gin-and-water. A series of researches by Dr. Edward Smith, previously quoted from the 'Philosophical Transactions,' has lately opened the way to more scientific knowledge on this important subject. He has experimented on the changes in the excretion of carbonic acid from the lungs which follow the ingestion of different members of this class of drinks. Wisely, perhaps, he does not aim at so much as Dr. Boecker, he does not attempt to calculate the whole amount of carbon excreted in the twenty-four hours, but confines himself to the phenomena of a limited period, thus excluding the influences of food and occupation. Dr. Smith found that brandy and gin, in nearly every experiment, lessened the amount of carbonic acid expired, and the latter to the remarkable extent of nearly $1\frac{3}{4}$ grains per minute in one experiment. Whisky generally lessened the carbonic acid, but had a manifest tendency to restore it to, or even to cause it to exceed its original quantity. The aromas of wines inhaled had also an uniform tendency to diminish the respiratory changes. On the other hand, the drinking of pure alcohol diluted with water, almost always increased the excretion in a moderate degree, as did also wine, old ale, and stout. Rum had a still more decided action, for in one experiment there was an increase of two grains per minute upon one of the persons. Sugar also, and gluten, had a great influence in the immediate increase of carbonic acid. From

* American Journal of the Medical Sciences, p. 117. 1859.

experiments occupying only a few minutes, we must not infer that we know the whole of the influence of these substances on the daily evolution of carbonic acid. Some which appear at first to increase may in the end diminish the excretion; and thus the observations of Boecker, who found that pure alcohol lessened the *diurnal*, may be reconciled with those of Dr. Smith, where it augmented the *immediate* amount. It will be very interesting therefore to see this inquiry followed up by observations on the remoter consequences of the substances tested. It is quite true that it becomes a matter of extreme difficulty to eliminate all the varied influences of daily life, but we do not despair of the averages of a long course being a close approximation to the truth, though the accurate valuation of each separate experiment is unattainable. It would be important also to know the individual power of each constituent of such compound articles as fermented and distilled liquors. What, for instance, is the action of foozel oil, very variable quantities of which are present in all commercial spirits, and in different amounts at various stages of ripening? It is certainly a very powerful drug, and as far as a limited experience shows, very deleterious. We once made a trial of it as a cheap substitute for ether in hospital practice, and were much struck by the poisonous, and only poisonous effects of the dilutest doses. Without doing any kind of good in any case, it produced furred tongue, thirst, and headache. What, again, do the delicate ethers of wines, acetic, œnanthic, &c., effect?

In the last century no one ever supposed that fermented and distilled liquors were merely diluted alcohol. Some physiologists, by their illogical introduction of the plural word alcohols* to designate the class, have given rise to what seems likely to prove a fallacy. One might just as well call tea toddy, and broth "waters." For science seems leading us back to the old opinion, that there are differences not only of degree but in essence between one member of the class and another. Few can forget the vivid manner in which the masculine burin of Hogarth has pointed out the physical, moral, and political consequences of these differences in *Beer-street* and *Gin-lane*—the rough popular prosperity depicted in the first, and the degeneration of mind, body, and worldly estate in the latter. And now, again, science would do wisely to investigate further the reasons for these essential differences, and enable us by judicious selection to get the greatest good and the least evil out of those drinks which owe their name and part of their nature to alcohol.

Morbid Results of Alcoholized Drinks.—The length of time which alcohol remains in the system, especially in those nervous tissues on which its effects are first shown, and for which it possesses a peculiar affinity, seems to offer us a means of laying down a definite limit between use and abuse. If the whole of a former dose has not been evacuated, we can easily understand the danger run by adding another, and the geometric progression of the danger from each successive

* Another reason against this use of the word is its employment in technical chemical nomenclature for a certain state of oxidation of theoretical radicals.

quantity. At a City insurance office we are in the habit of substituting for the usual vague question about temperate habits, an inquiry if the proposed life ever takes beer, wine, or spirits in the forenoon; and we have never regretted refusing to insure every one who gives way to this indulgence, and who does not allow a full period of sixteen hours in each twenty-four to pass without alcohol.

That peculiar injury to the health arising from alcohol, the symptoms of which are denoted by the term "chronic alcoholism," seems due to the frequency and not to the quantity of the dose. Injury to the digestive organs, doubtless, but of not a permanent character, is the result of occasional drunkenness. And these brutal excesses work their own cure, only becoming of serious importance when an attempt is made to blunt the stomach to its repentance by "a hair of the dog that bit you." An explanation of this is afforded by the experimental observations we have noticed of the length of time which the actual alcohol remains unaltered in the body; by its peculiar adhesiveness to the nervous system, and by an affinity—which we might almost venture to call chemical—for that tissue. As the poison gradually passes off, the nervous system, like a drowned man recovering consciousness, is racked with pain: if we check this returning vitality, can we be surprised that the half-life becomes a permanent state, and requires a much longer and more serious pathological process for its restoration?

Such an attempt to regain vitality is probably exhibited in acute *delirium tremens.* We cannot now offer our readers any recent additions to the pathology of this disease, and therefore will not detain them longer than to remark the change which has taken place in its treatment during the last few years. Till lately the patients treated with frequent doses of alcohol and large quantities of opium were the rule, those left to rest and purgatives were the exception. Now, the reverse is the rule with many practitioners, with apparently a great diminution in the mortality. What does this show but a growing conviction that the symptoms are the manifestation of a natural progress towards health, and are to be managed rather than antagonized? Yet, undoubtedly, cases are still often found which, standing alone, might seem to prove the old rule right; cases where we cannot fail to observe that our tentative doses of alcohol require to be repeated, and evidently save the sufferer from death. What is the nature of the risk run in these instances, and how does alcohol tide them over it? Our own experience leads us to believe that an explanation is to be found in the state of nutrition of the patients; and that those requiring alcohol are cases of inanition more or less absolute. Hence they are rarely found among the comfortable classes; who, however much they may have degenerated their tissues by drinking, usually have a sufficient reserve fund to stand the strain. But the miserable wretch who stills the cravings of hunger and keeps up his fallacious show of flabby fleshiness with gin, is really just on the threshold of death by starvation. The normal process of restoration is killing him, and we have to stave off its violence, and turn the acute into a slower form of the same disease.

What the physician wisely does from the necessity of prolonging life, is often done unnecessarily and foolishly by the intemperate patient. To the staving off, first of crapula, then of delirium tremens, by spirituous liquors, can always be traced that state called "chronic alcoholism." We have already* reviewed the classical work of Magnus Huss on this subject, and will therefore allude to it no further than to quote his limitation of the term: he applies the name

"Chronic alcoholism to the collective symptoms of a disordered condition of the mental, motor, and sensory functions of the nervous system, these symptoms assuming a chronic form, and without their being immediately connected with any of those modifications of the central or peripheric portions of the nervous system which may be detected during life, or discovered after death by ocular inspection; such symptoms, moreover, affecting individuals who have persisted for a considerable time in the abuse of alcoholic liquors."

It is, in short, chronic delirium tremens.

Since that time Dr. Marcet has paid much attention to the subject, and has enriched the literature of the profession with a modest little volume whose merits are by no means proportioned to its size. He comments graphically on the symptoms of the disease, classing them under the several heads of sleeplessness, headache, giddiness, hallucinations, weakness, difficulty of breathing. This last should, perhaps, more properly be called choking or spasm, and seems closely to resemble *globus hystericus;* like that phenomenon, it is often followed by eructations, and is dependent on spasm of the œsophagus and larynx.

He then discusses the causes predisposing to chronic alcoholism, and first among them points out a fact to which a former part of this paper has drawn attention—namely, that the injurious effect of alcoholic liquors is not to be estimated by the alcohol which they contain. He cites empiric testimony to this, and we trust that he will be led to apply his well-known chemical ability to explain the fact of the innocuousness of even the constant use of some strong drinks, and the poisonous nature of others even when largely diluted. New rum is proverbially fatal in the West Indies, and when Dr. Smith's paper was read at the Society of Arts, there was cited a personal experience of the evil effects of new Irish whisky on the members of the British Association at Cork. In a minor degree the same difference is found in the produce of the grape: "young" wines are universally spoken of with contempt, and "brandied" wines with reproach. Now, in reality, all wines that contain a fine-flavoured body must be supplied during their manufacture with an extra quantity of alcohol beyond that which results from their fermentation; unless this is done they cannot be preserved. But no vine-grower could afford to use what we call brandy; the alcohol is introduced in the form of raw corn spirit, which by keeping is converted into wholesome flavouring ethers, but which in its recent state is most deleterious. Hence the evil nature of the synonymous "young" and "brandied" wines. Hence also the remarkable difference between the British made wine, bought at a shop and manufactured of course with cheap spirit, and the fine whole-

* Vol. ix. p. 389.

some beverage we make of our own gooseberries and French brandy. We suspect the deleterious agent in these and many similar instances to be foozel oil; and now that the Legislature have acknowledged the principle that the physical as well as the moral *salus publica* is in their charge, we would seriously commend it to their consideration whether the presence of that substance in unnecessary quantity in liquors sold for drinking or manufacturing drinks, should not be made an offence.

Dr. Marcet then relates his experience of the very various quantities which induce the disease in different persons, instances of which a very limited acquaintance or practice will supply to all our readers. He quotes from Huss a list of the temperaments in the order of their liability, but we cannot say that this gives much information as to the causes of this remarkable idiosyncrasy. The characteristics of temperaments in the several races of mankind are so discrepant, that observations made upon Scandinavians, and through the filter of a German translation applied to Englishmen, do not carry much authority. Now that a scientific audience consists of several continents, the time is past for expecting the temperaments to represent facts. Much more valuable would have been Dr. Marcet's own notes of any peculiarities of constitution in those who have suffered from small quantities or resisted large quantities of intoxicating liquors. A few observations made without special object induce us to think that these peculiarities may reside in the action of the organs that excrete fluids: individuals who habitually take much liquor without damage, generally urinate copiously, perspire freely, and stink foully of alcohol after and during a debauch; while in some easily affected, the contrary has seemed to be the case. The easiest test of this rapid or sluggish passage through the system is to be found in the facility with which it may be detected in the breath, and we would commend the investigation to the notice of those who can pursue it among hospital out-patients on a large scale.

The infrequency with which women become affected with delirium tremens is ascribed by Huss to their more prudent habits. Dr. Marcet seems inclined to agree with him, but does not give a decided opinion. We feel rather disposed to go along with Roesch, who thinks the disproportion too considerable not to arise from other causes. And for this reason:—if the innate liability were equal in the sexes, the proportion of acute to chronic cases in each sex would be equal also. But such is not the fact. Dr. Marcet's table and our own experience show chronic alcoholism to be entirely unknown among women, though examples of delirium tremens are occasionally met with; whereas in males one form is as common as the other. This must arise from an innate difference of constitution, and not from different habits.

As respects the influence of occupation, Dr. Marcet says that violent bodily labour as well as deficient exercise, exhausting mental efforts as well as sluggishness, dispose to the disease. And in spite of the rarity of drunkenness among the upper classes, he finds them equally with the lower subject to chronic alcoholism. The only two employments which he names as furnishing rare examples are those of butchers and policemen. As regards the former, we must say our experience does not coincide with the author's, especially if slaughterers are to be included; and policemen soon cease to be such if they indulge the habits which induce the disease. Our own impression is that occupation has no influence at all, except so far as it renders impossible or easy the taking frequent small doses of alcohol.

The theory which we have ventured to suggest, as to the symptoms of alcoholism being an attempt of the system to regain vitality, is supported by the remarkable fact of their often first occurring, still more often becoming aggravated, "after the habit of drinking to excess has been given up, and even in many instances after a complete abstinence for some time from alcoholic stimulants." Many instances of this will be found recorded in Dr. Marcet's table. He further remarks:

> "I have been led to observe that the injurious effects of the long-continued abuse of alcoholic stimulants are frequently not developed to any extent until the occurrence of another circumstance which is the immediate cause of the attack. It has not been possible for me to determine satisfactorily whether an attack of chronic alcoholism may supervene long after the individual has given up drinking, and without his having at all suffered from the nervous derangement known to result from frequent excesses; but this much may be safely stated, that in the great majority, if not in every case, the patient's constitution has been so far affected that the slightest cause will be sufficient to startle or frighten him, produce giddiness, headache, and keep him from sleeping at night, yet without preventing him from attending to his occupations, or proving of any material inconvenience; and such patients are very liable to a regular attack of chronic alcoholism from some cause independent of drink."

And he might have said, "acute alcoholism also." An instance of that rare phenomenon, a fatal *delirium tremens* in the female, occurred in our practice to a healthy young woman who occupied the responsible post of chambermaid in a large hotel. She had indulged some time previously in secret dram-drinking, but had concealed both her habit and her leaving it off from her most intimate friends. She remained perfectly well till she was accused (we believe with injustice) of stealing, when she was instantly attacked, with the unhappy result above mentioned.

Among the "Immediate Causes of an Attack of Chronic Alcoholism," Dr. Marcet enumerates several diseases which have produced the effects in conjunction with alcohol, such as bronchitis, gout, rheumatism. As to affections of the stomach, he feels dubious whether they are cause or effect. Here, again, we would suggest whether the connexion between these ailments does not lie in the diminution of excreting power rather than in their weakening the body, to which the author attributes their common influence in chronic alcoholism. Many diseases which weaken the body more—such, for instance, as acute fever—do not act in the same way; and some really seem a direct preservative. Ague, for instance, and malarious neuralgia, certainly are followed by anæmia and debility quite as much as gout and bronchitis; yet instead of making the patient liable to chronic alcoholism, they seem to guard him from it. Persons affected with the aguish diathesis often take habitually quantities of alcohol which in health would affect them most injuriously.

The most important point in Dr. Marcet's volume is the remedy which he proposes for chronic alcoholism. He does not "recommend one remedy for a certain symptom, and another remedy for another symptom, but endeavours to show that there exists a substance possessed of powerful and definite medicinal properties, and having the remarkable property of restoring to health, or at all events of greatly relieving, the disordered nervous system of persons suffering from chronic alcoholism; the medicinal agent in question acting efficaciously in cases where the principal symptom may be either sleeplessness or hallucinations or trembling, or any other; and this substance is OXIDE OF ZINC."

The physiological action of oxide of zinc on the healthy body is not clearly known. One great difficulty in the investigation is its partial solubility, and hence the small, but variable quantities that are taken up into the system. The effects are by no means in proportion to the dose; but, whatever their nature, they are principally manifested in the nervous system. Moderate doses cause slight nausea, giddiness, black specks before the eyes, rumbling in the ears, and fainting. Oxide of zinc also, alone of all metals, is a soporific, producing considerable drowsiness even in the daytime. The absence of very marked effects from large doses, or, in other words, the small doses which are actually absorbed, have made some persons sceptical about its powers, and they have attributed to the imagination of either patient or physician the benefit which has appeared to follow its use. It is seen to do so little harm, that there is a suspicion that it does no good. Dr. Marcet upholds the power of his drug at the expense of its benignity, and shows that it can do mischief sometimes. He also cites an instance of its favourable action on a dog affected with epilepsy and paralysis, where, of course, imagination had no influence. Its beneficial, though not absolutely curative administration in certain cases of chorea and epilepsy appears to be gradually receiving general assent, though, from the nature of these diseases, evidence of the action of remedies upon them must necessarily accumulate with extreme slowness. Convinced that it was a medicine of real, though obscurely manifested powers, Dr. Marcet was led to try it in chronic alcoholism, and he appears to us, from the examples he cites, to have obtained undoubted proof of its efficacy. Let it be observed that the proof is much stronger than it can ever be in the case of remedies for chorea and epilepsy; for the first of these diseases is well known often to run a definite course like an acute fever, and to get well spontaneously; and the second is so little understood, and so irregularly intermittent, that its changes have been made to justify the use of the nastiest and absurdest articles in the pharmacopœias; while chronic alcoholism is a continuous ailment with little or no tendency to spontaneous cure, so that the action of drugs on it can be readily noted. It may be remarked, also, that the observations are made upon outpatients more or less engaged in their usual occupations, so that the fallacies are excluded which arise from the improved hygienic conditions of a hospital ward.

The effects of oxide of zinc in simple cases of chronic alcoholism are improvement in sleeping, diminution of hallucinations; black specks and crawling creatures no longer flit before the eyes, and extraordinary noises are no longer heard; the trembling hands become steadier, and gradually muscular power returns; the appetite gets better, and the mental depression disappears. When organic diseases, especially of the lungs, exist, the power of the drug is interfered with; but even in them, Dr. Marcet says he has found advantage in beginning with the oxide of zinc, in order to alleviate as much as possible the functional derangement of the nervous system, and afterwards to adopt such a course of treatment as may be considered the most suitable to the occasion. The dose in which Dr. Marcet seems generally to have used the drug is that of two grains, gradually increased to ten, twice a-day.

It might have been anticipated *à pricri*, that the diminished vitality which accompanies the use of alcohol should lead to a diathesis of general degeneration. No part of the body seems exempt, but it is of course most notably manifested in those organs which are of the first necessity, such as the liver and the kidneys. We cannot agree with M. Lallemand in explaining this by irritation occasioned by the passage of alcohol through those organs especially; otherwise, how should the lungs escape, through which alcohol in the first instance escapes? Neither does our experience induce us to agree with him in attributing renal degeneration to alcohol as the most usual cause; it is a joint cause doubtless, and an important one, as diminishing general vitality; but, like M. Rayer in France, we have found in London, cold, want, and exposure by far the most potent originators of Bright's disease. It is much more common among those who have been subjected to these influences without being intemperate, than among intemperate persons who have been defended from them.

Earliest probably of all parts of the body this degeneration commences in the blood. Some years ago, Dr. Boecker noticed the alterations undergone by the blood of habitual alcohol drinkers as yet in good health—namely, a partial loss of power to become red by exposure to the air, in consequence of the loss of vitality in a portion of the blood-discs. This loss of vitality manifests itself by the formation of black specks (oil) in the discs, and then by their conversion into the round pale globules which, in all cases of disease (i.e., of diminished vitality), are found in excess in the blood.* To this he attributes the darkening of the blood by alcohol mixed with it, either in or out of the body, which was noticed by MM. Bouchardat and Sandras,† as the immediate result of the reagent, and thinks that it takes place more or less in proportion to the dose whenever alcohol is ingested. If the dose is moderate, reaction quickly occurs and restores the blood by

* Boecker's Beiträge zur Heilkunde, Band i. p. 277 et seq. This is not the place to go into the argument which induces the author to agree with Schultz (Die Verjüngung, pp. 48–306) in viewing the colourless globules as a stage in the destruction of the red discs.

† Annales de Chimie, Oct. 1841.

increased excretion to its normal state; but if it is continuously repeated, the state of diminished vitality is kept up and becomes capable of detection by examination of the fluid. This devitalized condition of the nutritive fluid is probably the first step to the devitalization of the tissues which it feeds. In the liver, instead of glandular substance, the vital energy produces the less complex, less high development of connective tissue, sometimes in large quantities, forming an enlarged liver, sometimes in smaller quantities forming a scarred, contracted, granular liver. And mixed with this hard tissue, it exhibits the still lower development of vitality in the shape of fat.

But this morbid degeneration into fat is a very different thing from "stoutness" and "obesity." Where these are developed in drunkards, they must, if the result of drink at all, be due to the starchy and saccharine substances imbibed, or to the lazy habits of the imbibers.

To recapitulate—we think that the evidence, as far as it has yet gone, shows the action of alcohol upon life to be consistent and uniform in all its phases, and to be always exhibited as *an arrest of vitality.* In a condition of health it acts in some measure immediately on the extremities of the nervous system by direct contact, and is also carried through the universal thoroughfare of the circulation to the brain. To nerve-tissue chiefly it adheres, and testifies its presence by arresting the functions of that tissue for good or for evil. In a condition of health this temporary arrest is rectified by reaction, and the body regains its normal amount of life. But if the reaction be impeded by fresh doses of alcohol, the temporary arrest becomes permanent, and disease is the consequence. The most special exhibition of disease is in the special function of the nervous system, the life of relation, to perform the duties of which the devitalized nerve becomes inadequate. Then the vegetable life suffers; the forms of tissue become of a lower class, of a class which demands less vitality for growth and nourishment —connective fibre takes the place of gland, and oil of connective fibre. The circulation retains indeed its industrious activity, but receives and transmits a less valuable, less living freight, and thus becomes the cause as well as the effect of diminished vitality.

Thus deadly when abused, this powerful agent may be made an instrument of life and happiness when used in accordance with reason, as we have already noticed. In addition to its hygienic employment, a deep interest attaches to the aid it affords to the physician in his treatment of disease; so deep, indeed, as to preclude the discussion of the subject at the end of an article containing other matter. We will merely remark that hitherto the arguments rested upon seem to have been purely empiric and statistical, and that the rational and experimental part of the inquiry has been much overlooked. Moreover, two modes of treatment, assumed without proof to be opposite, are illogically compared with one another; it is taken for granted that patients must be either bled or brandied; whereas no comparison is made with those who are submitted to neither treatment, or to both together. So imperfect is the statistical reasoning, that, with all its shortcomings, we confess we prefer the rational.

REVIEW VII.

On Myalgia: its Nature, Causes, and Treatment. A Treatise on Painful and other Affections of the Muscular System which have been frequently mistaken for various Diseases. By THOMAS INMAN, M.D. Lond., Physician to the Liverpool Infirmary, &c.—*London*, 1860. pp. 307.

THE profession, one and all, should not be slow to thank the man among them who forbids their neglect of the muscle, and compels them to think upon pain. The muscle, by which all is done within and without us that man is permitted to do: pain, its great antagonist, than which nothing more interferes with the muscle at its work. Such are the terms of Dr. Inman's Myalgia; and of the volume to which it is prefixed, this compound neo-classical title is not the least important part. It is forthwith expanded, under the author's definition, into certain muscular disorders which are wanting in the element of pain, and by a further exegesis of mistake, negation, and denial, is made to introduce many local organic diseases that have nothing to do with the muscle. In it the more exact and less pretending titles of two former works (doing duty in the preface as so many previous editions of the present treatise) have been made to merge. Myalgia is at once the key-note and theme of the composition in both its parts. Here, there, everywhere, chapter, introduction, and index, myalgia. It is more than the title of the work; it is the title of a work which is the work of a title. In his dedication the author paraphrases his treatise as an essay. It is a bundle of essays—or rather two bundles of essays—of which myalgia is the binding knot. Moreover, it is a picture-book, with illustrations, highly varied and of a somewhat eccentric character. Opening the volume a few pages on from the title-page, we find ourselves engaged with the anatomist in his dissecting-room. The well-developed muscles of a healthy male subject are laid bare for demonstration. By the frontispiece we are introduced to the sculptor's studio, in presence of a female model the paragon of living grace and unadorned beauty. Advancing in the bookseller's window towards the public gaze, she will assuredly be identified by the street *flaneurs* as the Myalgia of the title-page which she fronts. In such case, her equally fascinating sister of Plate VI., retiring as if by order from the student's notice, cannot escape the more familiar appellation of Neuralgia. In the passionate indulgence of his pathological fancies, has our author become enamoured of his own abstraction and graduated as a nympholept? Pulvermacher's nude electric damsels are scientifically hung in chains. It is as well that Mr. Bagg's myalgic lady-models are anatomically tattooed. There are sundry little anomalies in the arrangement and making up of this volume which we note more to satisfy the enthusiastic author that we have read him through, than from any belief in their importance. In the preface, which is pleasant, and pleasantly short, we were somewhat bewildered among the dates and aliases of former editions, first, second, or third, as they may be. The headings of the chapters in the general table of "contents" differ often and widely from those prefixed to the separate chapters in the body of the work. Thus, in the discharge of our duty, reading steadily on to Chapter XIII., p. 210, we found no warning given in the capitular contents that we were intruding on ground on which, in the spring of 1858, a review by one of our British and Foreign staff had already been held. The last eight chapters, comprising ninety-two pages of the myalgia of 1860, belong to a work on 'Spinal Disorders and other Functional Diseases of the Nervous System,' first published in 1858. There is very instructive reading in the index, redundant as it is with myalgia, myalgic myositis, and muscle pain. Every man in practice would be a gainer from its hints.

It is a queer book, we can't help the word, this of myalgia; but we do not like it the less. It is anything but dull. We have read it literally through with lively scrambling attention, and we invite our readers to the same amusing lesson, assuring them that they will learn much from the myalgia, with all its drawbacks of a style diffuse to garrulity, and a manner which not infrequently trenches on the quality akin, in Hamlet's code, to an excess of familiarity. It is a book of which, though much may be said, it is by no means easy to report. It is without cohesion. It professes muscle, yet wants bone. It is molluscous. It opens with moderate vigour, works out a few principles, supports them with cases, with which it is overloaded to surfeit, and dwindles from time to time into what is little better than gossip. It abounds in mare's nests and recovered discoveries. Written in great part to support a name, and thereupon wordy and disputatious, it becomes in part a treatise on other men's mistakes and misnomers. Professing to tell us of pain in the muscle, the author gives but a few scattered notices of spasm, dismisses cramp in a short unsatisfactory chapter, which is little more than a catalogue of cases, repudiates cholera as a "blood disease," and adopting the "nervous irritation" theory of tetanus, declines all mention of it excepting as the pathological type of myalgia in the dead body. Muscle and nerve are, as usual, unduly confused from neglect of the material and agency which are common and paramount in both. No discussion on the nature of pain as resulting in the muscle from the elementary triple relation of blood, nerve, and fibre, is permitted by our author, who, on such matters, declines all presumption of research and eventual heresy of opinion. There is no getting him into the open. There is nothing to grapple with. It is this shirking of the main question, this dearth of high argument, that starves and may discredit the work. The 'Myalgia' is best recommended by its rules of exception and liberties of denial. Errors to be renounced, things not to be done, are the staple of its text. It is a book specially of negations. Thus Dr. Inman in his questionings of pain, as to its cause and nature, refuses in many cases the inference of deep-seated organic disease which its presence has been supposed to indicate. He has declared to the multitude, what was always patent to the few, that the pathology of pain has of late been too exclusively vital in its structural allotment; that the single viscera of organic life, the lungs, liver, uterus, and kidneys, are often made responsible for this perverted sensation, when the double symmetrical muscles are alone in fault. And he is right to make us listen while he proclaims that pain such as this is for the future to be called myalgia. There is such an inveterate habit among medical men in these ultra-pathological days of dissecting their patients alive, that we require the ring of a name to remind us, while localizing our pains and aches, that between the skin, the nerve trunks, and the deeper viscera, there are structures many and various, which if not always sensitive, may rapidly become so. Every day's experience convinces us of the tendency in ourselves, as in those with whom we consult, to forget that myalgia is a practical and frequent truth. On the other hand, Dr. Inman asserts for the muscle a faculty of "aching, burning, wearing, of soreness, weakness, and sense of heat," of pain in all its grim variety that amounts almost to a monopoly. Myalgia, with him, is the hydra of old, consenting to one head with a new face. Head, back, and chest ache are myalgia. Lumbago is myalgia. Hysteria, pleurisy, and peritonitis, much of neuralgia, and most of chronic rheumatism, are myalgic. These are but few of the usurping brood. Sympathetic pains are discredited. Peripheral neuralgia is not acknowledged. Dr. Inman demands for his myalgia more of sympathy and consideration than he is likely to obtain, but not more, as we believe, than he conceives to be his right. His myalgia is extensive, for his muscle is everywhere. "It must be understood that in using the word muscle, I consider it includes tendon, fibre, or fascia, as the case may be; also the insertions of the muscles into the bones." Again, in p. 42: "We must state that in speaking of muscular pains, we include those which are due to stretching of tendons, fibres, fasciæ, and the like." There is no keeping out of the way of muscle thus defined. Its "stretchability" (see Index, letter S) is without limit. Under this tenure, through every "half-crown space" of the outer skin, myalgia might sink its shaft, and claim all it found by right of soil. For its requirement, in any part of the trunk or limbs, muscle in the wide sense, as Dr. Inman understands it, is unfailingly found. Pain, the other term of the condition, is of more difficult approval. It always rests on the assertion, often with no collateral proof, of the individual patient. Cases in which pain is an essential symptom should be very strictly examined before they are admitted in support of pathological truths. Pain is of all symptoms the one of which the evidence is most frequently confused or perverted. It is falsely or foolishly described. It is unduly obtruded. It is made less than its reality; it is more frequently exaggerated. Sometimes when fiercest, it is denied altogether. Sometimes, in rare cases, it is an illusion; the true belief of a mistaken sense. There is a cheat by spectra and imagined noises of the ear and eye. There may be a false perception in the faculty of pain. In his chapter on Hysteria, pp. 147–8, our author writes thus:

"It has been objected to, that the pains I have referred to the muscles and their fibrous prolongations, or to fibrous elements solely, increase till they become unbearable merely by being thought of and dwelt upon; and that in this consists the explanation of the cutaneous tenderness. It seems to me that the doctrine involved in these remarks is a most dangerous one. It involves the idea that the physician must be always antagonistic to his patient's statements, and that he must refuse to believe in the existence of any pain which he cannot explain; that an hysterical patient, whether male or female, rarely tells the truth; and that a pain described by an individual as unbearable has in reality no existence!"

There is a wise humanity in these remarks. There has been a too frequent disbelief of pain that has been truly felt. On the other hand, there is a daily enormous waste of sympathy and medical speculation on pain that is unmitigated falsehood. The ingenuity, the pertinacity of these self-asserting agonies, which are by no means peculiar to hysterical spinsters, are such as occasionally to betray the judgment of the most experienced physicians. In some of our author's multiplied cases, original and second-hand, we seem to detect an element of this sort that would much interfere with our assent to his favourite conclusions. The one great dictum of Dr. Inman's myalgia is that the muscle, from over use and consequent debility, can and does set up a pain of its own, and that such pain may be various in kind and degree. We knew as much before; but we cannot be too frequently reminded of it. The first faint light of muscular medicine, which has now intensified into the noonday brightness of myalgia, seems to have dawned on our author in 1856, or thereabouts. Does the Liverpool physician really suppose that up to that time the Faculty of England, and of the world at large, had sat and walked in darkness? We can assure him that muscular pains, states, and conditions were terms of familiar use in the consultations of reflecting practitioners for long years before the publication of 'Certain Painful Muscular Affections' in 1856, of 'Spinal Irritation' in 1858, or of their double culmination in the 'Myalgia' of 1860. Apparently forgetful of the blood, and evidently afraid of the nerves, and of all that pertains to the nervous system, Dr. Inman ends as he begins with a local structural symptom; and that symptom plainly rendered, is muscle-ache. The over-partial attempt to make this single symptom of muscle-ache take rank as the disease of myalgia is continually hampering the author throughout the volume, and necessarily betrays him into many logical inconsistencies. Thus, in the title-page, myalgia is distinguished from inflammation. In pages 180–1, we read that the "so-called hysterical pains were myalgic, and due to inflammation." "A myalgia," "pure myalgia," "purely myalgic," are terms for which we have the authority of Dr. Inman. What definite sense he attaches to a "pure myalgia" we are slow to conjecture, unless it be one of pathological inference from the "coursed hare" or the "tetanic man." Yet tetanus, in the living subject, he on principle avoids. And of his many myalgic ladies, no one for the instruction of his readers is hunted like a hare to the death. Pain in the muscle, if not the result of direct local injury, has always reference to some previous or coincident state of the general health, the expression of a prejudice in the composition or functional agencies of the blood.

Three-fifths, or little less, of the work before us, are given up to cases of myalgia in its local variety. This sort of testimony, as it cannot be verified or even called in question, should be more sparingly used. In the narration of some of these cases there is a tone of special pleading which does not conciliate the attention of the impartial reader. Our author's examples of muscle-ache are not the less likely to be remembered, many of them, from being of the homely every-day order. "The soreness, pain, or abdominal tenderness that follows on the next day after a night spent with any irresistibly comic actor;" "boating in a pleasure-barge;" "dancing with spirit (when and where is that now-adays done?) for six consecutive hours;" "sitting up with funny dogs till the small hours of the morning," "enthusiastic travelling," enthusiastic "card-playing," are instanced by our author as so many special provocatives of myalgia. Of the "pubic pain" that waits as a Nemesis on this last-mentioned muscular irregularity, there are, by the way, three separate notices. Is there, indeed, so much and to spare of enthusiasm, with corresponding leisure, at Liverpool, that it must be played out on the card-table? We decline to follow our author through his Chapters IX. and X. Myositis, ovaritis, phlegmasia dolens, and muscular abscess, are inconvenient extensions of myalgia when space is limited, as with us. Saying the very least possible on each of these subjects, we could not dismiss them, as Dr. Inman has done, in less than three pages, exclusive of cases, in long irrelevant detail. Among the self-willed, one-limbed, unsleeping fibres of organic life of Chapter X., we find ourselves on new ground, under new laws of action, new conditions of structure, with a guide who, as it seems to us, is not sufficiently cognisant of the difference. We are not afraid of the nerves, but we have no wish to find ourselves entangled, with a timid companion, in the mazes of their sympathetic ganglionic division. Eleven pages of the 209 that come under our review are all that our author can spare for irritability, cramp, or spasm of the heart, angina pectoris, cardiac symptoms and maxims, myalgia of involuntary muscles, stomachic myalgia, vesical myalgia, vesical myositis, uterine myalgia, and globus hystericus.

In Chapter XII. (the last of those which fall under our review), we come unmistakeably on the mouse of Dr. Inman's myalgia. The smallness of the therapeutical result that creeps out in contrast with the bulk of the parturient dissertation, is such as must win a smile from the author himself. The "short inquiry into the treatment" begins half way down page 207, and is concluded, with room to spare, in page 209, most of the intermediate space being occupied by foot-notes and the details of a single case of infantile convulsions. There is a foregone conclusion for this short allowance of advice to the patient from the very limited character of the pathological principles and inferences in the preceding chapter. Myalgia, the disease into which so many other diseases are made to merge, there resolves itself into the one symptom of muscle-ache, and the attempt of the author to give it a distinct position in good nosological society can establish for it no higher rank than that of "debility with corresponding irritability of fibre." For such condition of head, trunk, and limb, what should be the remedies but rest, generous diet, tonics, and morphia? The torn or congested muscles are to be strapped, shampooed, and supported.

For a "permanent cure" by "strengthening the system," there is no medicine more approved by use than glycerine or cod-oil, with or without iron in full doses. Hysteria, neuralgia, chronic rheumatism, and other countless diseases that are in truth but myalgia under mistaken names, are subjected by plain inference to the same simple rule of treatment. Are all your indications, all your specifics, all your cures triumphant or spurious, shrunk to this little measure? There are incidental notices of treatment scattered through the body of the work, but they all come under the category of rest, anodynes, and tonics, with the exception of one case of the actual cautery actually adopted in the case of Mary C——, a young myalgic maid-of-all-work, "after the failure of every local application that could be thought of." This fiery contravention of a gentler discipline seems to have been an escapade, as it failed to drive out the lesser pain, and is never again mentioned as a remedy in any of the very many cases that are reported. By the way, Mary C——, like some of the other sufferers, seems to have had much more than muscle-ache the matter with her.

The Liverpool physician regards with marked favour the new syrup of fat, euphuistically known as glycerine. Peeping, as we have a right to do, into Part 2 of the Myalgia, we read that "glycerine is as superior to cod-oil as a tonic, as it is in pleasantness." In the same part of the volume, the somewhat vague phrase of "generous diet" expands under directions for "kitchen physic of the most judicious kind," into eggs, soups, fruits, Madeira, champagne, cream, jelly, blancmange, good toasted cheese, tipsy-cake, and anything "light, tasty, and agreeable." Purgatives are never mentioned but with disfavour, and are sometimes denounced as especially mischievous; yet, in the practice of us all, how often muscle-ache pains have been brought to an end by a brisk dose of rhubarb or jalap. Baths and the douche in all their variety, colchicum, and the alkaline alteratives, are entirely ignored. This lame and impotent conclusion of much that is original, clever, and courageous in medical inquiry, is the inevitable result—Dr. Inman will not be slow to see it—of his mistaking an accident for an entity, of his claiming for what is but a symptom of many diseases the consideration that attaches to disease in its context.

Under the section headed "Duration of Myalgia," there is evidence of our author's misgivings as to the therapeutical efficiency of his "weak and irritable fibre" theory in its application to practice. Myalgia, a disease of debility, with rest for its cure. Rest! many of our author's patients enjoyed much more rest than was good for them. They rested for months and years together, and were not the better for it. Among the "myalgias" there are some, not a few, that might be worked off or walked away. There is a muscle-ache of the healthy organ, merely from disuse and want of range in its adjustment; for the living muscle is never in truth relaxed. When most in repose it is continually, by tonic contractility, maintaining a due extension of its fibres in their length between the several points of their attachment. It is always striving against the rigor mortis. Of valetudinarians there are not a few—the over-fed and lazy—who, "lolling in easy-chairs" and saturated with champagne, would consent at our author's bidding to forego the distinction and sympathies of neuralgia, rheumatism, and hysteria, compounding for the present with myalgia and the fashion. That myalgia may keep its name and hold its own with the profession, it will be necessary that wider views should be taken of its nature, character, and causes, with a suitable extension of the remedial agencies that are available for its relief. In urging the necessity of "air and exercise" (always "out of Liverpool"), and of "full and generous diet," for the permanent cure of myalgia, Dr. Inman consents to approach the impoverished muscle through the blood. He has found his clue—let him hold to it. By the blood in mass as an organ, by the blood in direct and intimate relation with the contractile fibre, and, again, by notices and influences from the blood in organic relation with the cerebral and spinous centres transmitted through its appended nerve, the muscle, in health and disease, sentient or without feeling, is continually being influenced in its nutrition and special functions for good or for harm. Pain in the muscle is always a result of something done in its structure, of force expended, of a material consumed; an educt from its atomic reagencies, just as much as the formative cell of its sarcinous or sarcolemmatous tissue. Under certain disturbed conditions of nutrition pain is always being felt, for it is always being generated. Like all other processes of nutrition, pain is incremental. Unlike to many, it is not accumulative. Pain is a thing felt, because it is a thing done; but it is felt in being done, not as having been done. Interrupt the nutritive processes of which the pain increment is one result, and in preventing its further generation you are rid of what has gone before.

Our author, whose volume teems with speculative aspirations which he wants the heart to follow up, suggests, in p. 146, the inquiry "whether it is the nerves alone that feel?" Pain, in its variety, is not a function of the nerve alone, but of the nerve variously organized in the muscle and other structures, from which the nervous filaments proceed, and of which they are so many integral parts. Even in the nerve distinct and separated from the structure to which it specially belongs, pain when felt is an expression, not of what stirs and is done in the nervine alone, but of a function discharged by the nerve as an organ, in organic relation through its supporting tissues with the blood. Pain, we have said, is an incremental educt from the elementary process of nutrition. Variety of pain is the variety of such incremental action. Thus, certain organized structures (the nerves included) which, under ordinary circumstances of growth and nutrition, are not sentient of pain, become acutely sensible when compelled by disease or accident to an altered process of nutrition.

There is a passage in the chapter treating of "causes which predispose to muscular disorders," on which it is impossible not to remark:

"Dr. A. Wood of Edinburgh, in the 'Monthly Journal' for February, 1853, remarked, very judiciously, that there were two elements in the phenomena of convulsions—the nervous and the muscular. Up to that time the medical mind had thought only of the former: he introduced the latter element to their notice, and by his judicious remarks originated a new era in the history of spasmodic disease." Thus, by the accomplished Edinburgh Professor we are fairly invited to consider the strange problems in muscular contraction that we most desire to solve. We find ourselves in presence of spasm and convulsion, not merely as phenomena that can be observed and felt, but we learn to know them as actions resulting in a highly composite structure from impressions and actions antecedent in certain of its component parts. We would gladly discuss these high matters at any risk save that of fatiguing our reader, but we are limited to less than the hundredth part of the space necessary for such encounter; and, moreover, we are warned off, here and *passim*, from all ground that is not "purely myalgic."

Still, now as ever, we must enter our protest against the exclusive conventional duality of nerve and fibre, as tending to derogate in the pathology of the muscle from the triple unity of these living organisms with that of the living blood. It is from the habitual neglect of this third quantity in his muscular calculations that our author's perplexities for the most part ensue. His clue, we repeat it, is in the blood. In the elementary business of the human body there is nothing primordial, nothing proximate but this. It alone is everywhere, and everywhere at once. Its function, like that of the air, of which it is the analogue, is all-containing, ever-present, universal. From the nerve, from the muscle, in no atom, at no instant, can it be excluded. Continuous everywhere, as with the structure so with the air, it is while living, even without its current, in itself a system. Through the blood alone can we consent to acknowledge, in the anatomical central combination of nerves, a true physiological system. They, and the organs of which they are so many subordinate parts, are under no common simultaneous influence but that of the blood. Throughout the volume before us there is a rule of distinction, with a not unfrequent exceptional confusion of the terms "muscular" and "nervous," one with the other. There is no sense of muscle, in its myalgic or any other capacity, by which its nerve-element, intimate or distant, is not of necessity implied. There may be much to affect and disturb the nervous organism in its central anatomical union with which particular muscles discharging their integral composite function of blood, nerve, and fibre, are in no way concerned.

Touching the latter sentence of the paragraph just quoted, we remember, while discoursing of that paragon of muscles, the ever-working, air-weighted, central, double diaphragm, being humorously asked by the late Dr. Pelham Warren, "Don't your fingers sometimes burn and itch to let those fellows, the authors, know that we who don't put all we say into print, are not as far behind them as they seem to imagine, and in the matter of discoveries and original views, are as often as not a long way before them." Assuredly, there is an authority not of authorship. There is a world of thought and opinion with limits wider than have been mapped by ink. Answering merely for ourselves as ordinary practitioners, we utterly refuse the assertion that, up to 1853, in thinking of spasm, we thought only of its nervous element. We deny that we are in the most remote degree indebted to Dr. A. Wood, of Edinburgh, for originally introducing the so-called muscular element to our notice. For long years (say twenty at least) before February, 1853, we strove hard, on all suitable occasions, to impress on the minds of those who came in our way, that in the myology of health and disease, nerve and muscle were not convertible terms. On the formal claim of priority in record, acknowledging for the nonce no fasti but those in type, we venture, as faithful critics, to submit for Dr. Inman's perusal a little volume on 'Spasm and other Disorders termed Nervous,' published in London under date of 1843. It is quite impossible that the author of 'Myalgia' can have read it, or he would have seen in its every page, that for years before its publication, there had been no lack of effort in denouncing the conventional absurdity which confounds in function the integral muscle with its appended nerve. From pp. 2, 3, and 4 of the treatise to which we refer, we will satisfy Drs. A. Wood and Inman that if they are condemned for "physiological heresies" in their muscular religion, they will suffer as Lutheran and Calvinist. There have been Lollards before them.

"There is a disparagement of the entire muscle in the theory which regards its nerve as the sole agent of the influences that determine the contraction of the fleshy fibre, thus rendering a part of the organ paramount in the discharge of the function required from the structure in its completeness and integrity. Under this prevailing error in physiology, the wide, open relations of the muscle with the other parts of the animal frame are continually narrowed and confused; and that which is in truth the most independent of living things, the self-contracting fibre, becomes subordinate in the system to the nervous structures, appended and secondary in their nature, with a function essentially representative of actions that never originate in themselves. Between the blood and the nerve there are at present great and contending claims. In practical medicine, as in physiology, the muscle is the arena in which such questions will be best decided. That much of what disturbs the function of the muscle may be transmitted to its structure by the nerve is of frequent and undoubted proof; in modern pathology, it is not so necessary to be reminded of this principle of nervous influence upon the muscle, as to receive a caution against the too frequent application of it."

In asking room for these passages, few among many from a volume which is not the Myalgia, we do not unduly trench on Dr. Inman's privilege of time and space. Although we quote from another man's book, we feel assured that, from the genuine love of his subject, Dr. Inman will make that book his own. In the mean time, sincere and earnest advocate as he is of much that is true in myology, let him be content in the matter of muscle-ache not to be a discoverer. He is not even the recoverer of a discovery, for discovery there has been none. Time out of mind there has been full recognition in every vocabulary of the ills and pains that flesh is heir to. Cramp, spasm, strain, sprain, stiff neck, ricks, and cricks, have ever been naturalized among us. Their *habitat* is vocally and locally English. Dr. Inman has given them a Greek name. It hampers him at once on the title-page; it cramps him in his exposition of principles, and suggests a spasmodic uneasiness of style throughout the work. By the fair enchantress of his frontispiece our author is paltered with, from chapter to chapter, in the double sense of myalgia a symptom, myalgia a disease, until he is mystified into the belief that his brain has given birth to a goddess. Dr. Inman, it seems to us, is under a glamour of myalgia. He has been writing too much and too fast of late. He is too much in love with the muscles. He will forgive us, for we cherish him as a comrade whom we cannot spare—as a bold, earnest, intelligent inquirer into matters which the conventionalist does not dare to touch. In his chapter on Pleuritic and Pleurodynic Pains, he has shown that he will not condescend to be a mere repeater of phrases. He exemplifies his Dedication in remaining among those who prefer to "think for themselves." To the end of his career he will denounce misnomers and mystifications in the calling which he follows. He is far away from being the founder, but he is a strenuous apostle of true muscular religion. He needs no encouragement, but rather a pull from behind; that in the end he may make the better spring. In his preface we are told that "he has been led to the belief that many of our most cherished notions respecting the nervous system require complete remodelling." He further hints at a complete treatise upon muscular affections. Let him not attempt it. He has not the temperament, he may not have had the training, that suit with the ponderous operations of regular warfare. His energy, intelligence, and independence of thought, will always maintain for him the distinction which, as a partizan, he has already secured. Let him be constant to his myalgia; and for a more hopeful understanding of the nature, causes, and treatment of pain in the muscle, let him for the future approach the structure through the blood. Thus engaged with the muscle in its truth and totality, Dr. Inmán—we are pledged to the result—will not fail to get hold of something worth knowing in the history, differences, and medication of his favourite myalgia. In his fourth forthcoming edition (the one before us, we are told, "is in reality the third"), let him head his essay with his melodious Greek compound if he will. 'Myalgia, a Treatise on certain Painful Muscular Disorders,' as in 1856, would well and sufficiently indicate the range of the author's purpose, without the involutions, contradictions, and inconsistencies of the more expanded programme of 1860. The name myalgia, if not applied to painless states of the muscle, has the stamp of adhesion, and will stick. It is a hit—a telling, selling, palpable hit! That our author may communicate more advantageously with others, it will be better that his volume should be entirely recast; that "Spinal Irritation," as a separate title, should be dismissed, and thus that Parts I. and II. should be laid into one. The nude damsels should retire from the frontispiece and from Plate 6 to the studio from which they have been evoked; otherwise our "facetious patient" of the preface might "dub the Doctor with a more ignominious title than Muscle-man." The dissecting-room sketches of the inner man might be forced into a punning opposition with the undraped figures of the outer woman. The anatomical drawings need revision: the serratus major in Fig. 5 must be induced to indigitate with its corresponding attachments of the external oblique, on the margin and lower surface of the same rib. In the figure before us it overlaps them on different ribs. In Fig. 3 there is much to correct in the outline of the right clavicle at its scapular extremity, as in those of the coracoïd process, where it gives attachment to the smaller pectoral muscle and the short head of the biceps. The oblique articulating processes of the lumbar vertebræ in Fig. 5 have strayed most inconveniently from their corresponding spinal levels. Our author has by this time so much new good stuff under hand for future editions, that he can well dispense with the great Griffin, ten-page, second-hand case in Chapter XVIII. of Part II. Let it disappear, with its cross-examination, special pleading, italics, and foot-notes from the reprint of the current year.

It is well that the draughtsman was not employed on an illustration of the dramatic interview between doctor and patient which enlivens pages 62 and 63 of the 'Myalgia.' Its stage directions, like the tattooed goddess of the frontispiece, are without parallel in a medical work. We read in sooth, "wonderingly," and with eyes "rather" more than "open," of the doctor slowly interrogating "the healthy young woman of twenty-two, of good colour" and correct manners, who complains of a pain in her side, "spreading out her hands and covering the pectorales major as nearly as she could." We marvel not that the lady patient should answer "briskly" and with a stare of "wonderment" to her pertinacious interrogator, who suitably matches his words with action by "touching the sacrum and the origin and insertion of the trapezius with those of other muscles of the upper trunk," and by "pointing in his own person to the insertion of those on a lower level." This little bit of farce-writing is an interpolation in no way needed in Dr. Inman's lively narrative of his medical experiences. We hope to miss it entirely, with all its elaborate exhaustive reasonings and logic of leucorrhœa, when our author again undertakes to teach us our "steps in diagnosis." By the way, we are not quite at ease as to the expediency in many cases of obtaining the myalgic symptoms in the manner suggested by our author. The patient "maps the place;" the doctor "taps the spot." We have read the remarks on this topic in page 148; we respect in a high degree Dr. Inman's earnestness and simplicity of purpose, but we see reason to fear that an over-free and too curious inquiry might sometimes be instituted and more frequently expected from a practice of "tapping" and palpation of the outer surfaces, as from the undue questioning, by manual examination, of the inner structures of the ailing body. Dr. Inman has a true reverence for woman, and a genuine medical sympathy with her peculiarities of suffering. He denounces the wanton frequency with which the mirror is held up to Nature in the exploration of uterine disease. Let him be careful that to his practical treatise, with illustrations from the living model, on the geometry of pain, the motto does not attach of "*veluti in speculum.*" Let him not misinterpret our remarks: they imply no stain on his shield; we would but help him to burnish his arms. He has already done excellent service in his encounter with what is loudest and most rampant in modern false physic—Chronic rheumatism has disavowed its own blazon. He has fairly unhorsed hysteria. In this last discomfiture, as philanthropists, we specially rejoice, for we were bred and we long practised in wards where the realities of organic disease were too often obscured by the shadowy presence of hysteria. During a long dynasty of dogmatic lecturing and prescription, there were those—the many who play at physic as they would at "follow my leader"—who were used to talk familiarly of hysteria, of hysterical dysenteries, and hysterical hæmorrhages, as of hysterical joints and hysterical backbones. "It is only hysteria;" and the woman who was "only hysterical" was dead before the physician who thus set her case aside had reached the end of the ward in which she lay. Many an ill-nourished joint that had striven hard and long to keep its surfaces smooth and elastic, has been compelled to ulceration by the neglect of rest and by the faulty discipline which are implied by this routine phrase of "It is only hysteria." Let us be careful, Dr. Inman, and all of us, that under the fascination of myalgia, the old cry, with its attendant mischief, is not transposed into "it is only muscular." Hysteria and rheumatism, whatever be their conventional designation, must still be recognised as distinct pathological conditions of the system. Whether there be ever a painful affection of the muscle, by cramp, spasm, or otherwise, that is not a consequence of certain conditions of the general health with the blood as its immediate agent, is a question that admits of no answer by proof. Though we do not endorse Dr. Inman's belief in pure myalgia, we thank him in all sincerity for making us think in this difficult practical matter of what we profess to know and undertake to do. It is of the first importance to the physician that he should be able to determine on the nature and local relations of the pain of which his patient complains—whether it be the symptom of inflammation, of obstruction, of internal lesion, or other disturbing causes; whether it be native to the spot where it is felt, or sympathetic from a distant structure; whether it be real, and in what degree hysterical, neuralgic, fanciful or false.

Henceforth, no physician who has read Dr. Inman's essay with the attention it deserves, will proceed against a pain in the shoulder of a consumptive patient as pleuritic and inflammatory, without giving his lungs and system the benefit of a doubt. Many a dull ache or angry wrench hitherto encountered by iodine, arsenic, or the salts of mercury, as remedies specific for rheumatism, will be suffered, through warmth, rest, and wholesome sufficient food, to work out their own complete and quiet cure. Practically, Dr. Inman, by the publication of his 'Myalgia,' has done much towards this wise and salutary end. In the theory and explanation of his principles, he is capable of wider views than are afforded by the few glimpses of light which for the present he consents to borrow from the relations of the blood with the nervous system. Beyond the duality of muscle-element and nerve-element there is another term in triple unity of function and material, which our author, for the better understanding of myalgia, must at every instant consider. Through its life-blood only can the living flesh be known. Hitherto our author has but dallied with Pain and the Muscle. It is somewhat amusing to watch the bold swimmer who should be furthest from the shore still hesitating on the brink of depths into which he should be the first to plunge. He shrinks and shivers, and calls to us in witness of his trouble. He knows he cannot stay where he is. Let him take a "header," and strike out. He will find others—few only—there before him. Better to be tossed and buffeted on the open seas of myalgia in its widest, deepest sense, than to stumble in the shallows, and sink entangled in the superficial profundities of the so-called "Nervous System."

Review VIII.

Lives of Eminent American Physicians and Surgeons of the Nineteenth Century. Edited by Samuel D. Gross, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia.—*Philadelphia* and *London.* 1861. pp. 836.

When the editor of this book acknowledges a "lamentable deficiency" in native medical biography, it can hardly be necessary for us to apologise for an ignorance of American literature of a special kind, as great as and more excusable than his own. Not that we mean to imply that he has really anything in the way of ignorance of which to be excused, for he has availed himself fully of the materials at his command, and epitomized the narratives of many physicians and surgeons who have flourished during the present century. With this period only does he profess to deal. If we seek to retrace our steps farther, we find that the chief work on American medical biography is from the pen of Dr. Thacher, of whom there is a brief and insignificant memoir in the volume now under our notice. This treatise was published in 1828, and was followed by a similar undertaking in 1845, from the pen of Dr. Stephen Williams, of Deerfield, Massachusetts. It is obvious that these works cannot be of a very extended retrospective character, inasmuch as the historical antecedents of the United States are of necessity limited by their political duration. There are many travellers who have spent years in three quarters of the globe, without having the least desire to visit the fourth, because the latter has comparatively no records, and does not present itself to us shrouded in the splendid vestments of immemorial greatness. In treating of even the most comprehensive Western biography, we should find ourselves much in the position of those classic travellers. If we pass the period of the Union now in process of disruption, our horizon would at least be bounded by Bunker's Hill.

Dr. Gross (as we have before hinted) professes only to deal with those who have "flourished" during the present century: that is to say, with those whose lives have extended from the eighteenth into the nineteenth century, or have been entirely a portion of the latter period. To us "Britishers," the most interesting portions of these very unequal memoirs are unquestionably those which connect the subjects of them with the mother-country. We cannot fail, for instance, to read with pleasure how most of those who make up this hydra-headed biography were educated in Edinburgh and London, and initiated their special studies under the guidance of Cullen, the Monros, Ferguson, Gregory, Fothergill, John Hunter, Home, and others of equal celebrity. Of the former physician, Dr. Samuel Bard speaks as "that accurate professor;" of his manner as a lecturer he writes: "I own I think nothing can exceed it; being so entertaining as well as instructive that I could listen to him with pleasure for three hours instead of one." (p. 173.) How different from the stricture which the celebrated Robert Hall was compelled to pass upon the preachers of his own day: "the best passage in many a sermon is the passage from the pulpit to the vestry!" This Dr. Bard, by the way, seems to have been a man of very considerable observation and singularly independent mind.

"New names (he writes with an appositeness which makes the observation a part of later times) are always deceiving; new theories are mostly false or useless; and new remedies for a time are dangerous. This rage for novelty pervades our profession, especially in this country. Hence our extended catalogue of new fevers, and hasty adoption of new remedies; hence the unlimited and unwarranted application of mercury without weight, and brandy without measure, and the lancet without discrimination; and hence, I am afraid I may say, the sacrifice of many lives which might have been preserved, had they been left to water-gruel and good nursing." (pp. 191–2.)

As the editor of the work before us distinctly affirms in his preface that the respective contributors are alone answerable for what they have written, and that he has merely exercised a general supervision, we shall not be guilty of that uncharitableness which has characterized a recent attack by a famous Review upon a theological work of no common interest, and fasten upon Dr. Gross a responsibility which he repudiates at the very outset. In the language of a celebrated French writer: "He does but furnish the thread which binds together the flowers culled by other hands." Three of those flowers, indeed, are from his own garden; and it must be admitted that they have a modesty and a grace which do not belong to many of the flaunty and scentless ones with which they are grouped. To drop our metaphor—Dr. Gross has much too good taste to resort to such language as "the most accomplished editorial villain this country has ever known;" nor does he speak of men "perishing by their malignant ingratitude." (p. 38.) His memoir of Dr. John Syng Dorsey is one of the most interesting in the book. This gentleman was a pupil at St. George's Hospital in London, under Mr. (afterwards Sir Everard) Home, and was treated by his preceptor with great kindness and indulgence. He was taken to Sir Joseph Banks's *conversaziones*, and to Mr. (afterwards Sir Humphrey) Davy's lectures on chemistry at the Royal Institution.

Attending a *soirée* of the famous Quaker, Dr. Lettsom, where five hundred were assembled, and "the strawberries provided for the occasion, but then out of season, cost about ninepence a-piece," he records: "The chattering of the ladies pleased me much better than a lecture of an hour on an eclipse, an Ægyptian mummy, a rusty medal, or the horizontal parallax." (p. 145.) Young Dorsey, however, had not at this time acquired much knowledge of social life; nor did he know the polished manners and habits of the town. He goes to the theatre without his cocked hat, sports nankeen instead of black, and asks the "etiquettical" Mrs. Home for sole instead of turbot. These mistakes were fine sources of amusement to the great surgeon, who "enjoys laughing more than anybody I know." Shall we tell the present accomplished surgical staff of St. George's Hospital what was done by their predecessors in the way of operative surgery?

"This morning Mr. G——, one of the surgeons of St. George's, trepanned a woman at Dr. P——'s request. She had been some time in the hospital, with most obstinate headache. He sawed away till he got through the skull and dura mater; they both came out together, and the chances are ten to one that the woman will die. Is not this licensed murder? But one learns from other people's blunders." (pp. 143-4.)

Another subject of the editor's own pen, Dr. Ephraim McDowell, is distinguished as having been the first surgeon who performed ovariotomy, in 1809. He subsequently achieved this operation thirteen times; but his later were not so successful as his earlier cases.

St. George's Hospital claims likewise as one of her pupils a man of no mean reputation in America. Dr. Philip Syng Physic, "the father of American surgery," was a pupil of John Hunter. If he had been remarkable for nothing else, he would at least have been entitled to standing room among his country's heroes as "having survived (in 1797) a second attack of yellow fever, during which he was bled to the amount of 176 ounces!" (p. 385,) and as having left some very eccentric testamentary directions for the disposal of his body after death. He enjoyed an enormous practice in Philadelphia. "His professional labours," as we are told by Dr. Horner, "sometimes produced twenty thousand dollars a year, and his method in this respect finally yielded more than half a million of dollars." (p. 439.) The memoir of Dr. Physic, by Dr. John Bell of Philadelphia, is the most comprehensive, though not the most modestly executed, biography in this collection. It is not our intention to particularize any other of these sketches, inasmuch as they have rather a local than a general interest. We have no desire to underrate the professional merits or the private worth of the individuals here treated of; but, with few exceptions, their reputations are not European, and the memoirs abound with such a profusion of domestic details, and such an amount of indiscriminate laudation, as to make them quite unsuited to the taste of the generality of English readers. It may, however, interest some of the curious in social economy to know that "Dr. John Warren became engaged to Miss Collins, and soon after his settlement in Boston he went to Newport to claim his bride (p. 97); as also that Dr. Caspar

Wistar was first "united in matrimony to Miss Isabella Marshall," and afterwards married Miss Elizabeth Mifflin. (p. 135.) Others will not fail to appreciate the intelligence that Dr. Amariah Brigham weighed one hundred and thirty pounds, and that "his gait was naturally slow, and by no means graceful, while his voice was soft, low, and quite melodious." (p. 542.) The record, too, must not escape us that Dr. John D. Godman was "taught his alphabet upon the knee of his grandmother" (p. 248), and that, being of the unctuously divine school, a special interposition by "the Great Ruler of events" first prevented his studying medicine; but afterwards he reverted to his first love, by the "friendly interposition of Dr. Davidge." (p. 254.)

But we were wrong in saying that we would not any more particularize. There is a playful and poetical physician who taps us on the shoulder, and claims at our hands a passing tribute. He shall have it. How can we refuse one of whom it is said, "Nature was the altar at which he worshipped," and whose chief delight was to "preside over a gooseberry society?" Dr. Samuel Latham Mitchell had "a mind of vast and multifarious knowledge and of poetic imagery." In his 'Epistles to his Lady Love,' he "gave utterance of his emotions in tuneful numbers, and likened his condition unto that of the dove, with trepidation seeking safety in the ark." (p. 271.) Anything, in fact, says Dr. Mitchell's biographer, "might be eliminated from his mental alembic." He had the key of all mysteries, but condescended, with a humility for which all infantine posterity will be grateful, to rectify the inaccuracies of our nursery rhymes, and reprobate the sacrifice of truth to effect. "You are acquainted," says he, "with the nursery rhymes of 'Four-and-twenty blackbirds?' They abound with errors, and the infantile mind is led astray by the acquisition of such verses. I have thus altered them this morning: 'When the pie was opened, the birds they were songless; was not that a pretty dish to set before the Congress?' I thus correct," added the Doctor, "the error that might be imbibed in infancy of the musical functions of cooked birds; and while I discard the King of Great Britain, with whom we have nothing to do, I give them some knowledge of our general government, by specifying our Congress." We are overpowered by the intensity of this Americanism; but we know not whether most to admire the poetic talent of the biogra*pher* or the biogra*phee*. The former (Dr. Francis) mounts into the highest regions, without the disfiguring obtrusion of such incongruous rhymes as "songless" and "Congress," when he writes: "In the morning he (Dr. Mitchell) might be found composing songs for the nursery; at noon dietetically experimenting and writing on fishes, or unfolding to admiration a new theory on terrene formations; and at evening addressing his fair readers on the healthy influence of the alkalies, and the depurating virtues of whitewashing." (p. 273.) It would not be fair to conceal from our readers the important geographic discoveries made by this illustrious physician, "that the American continent was the Old World, and that the Garden of Eden might have originally been located in Onondaga Hollow." (p. 275.) Such is all the space we can spare for one whose "hymnology was extensive," whose "calm spirit was awakened by the martial strains of Toplady," and who "seems to have practised Christianity without knowing he was a Christian." (p. 287.) Even when this scientific Nestor was buried, the sexton was overpowered by the greatness of the occasion, and when asked by a stranger for whom he had just performed the last mortal rites, replied: "One who knew all things on earth and in the waters of the great deep!" We are astounded to think that America can have so long survived the loss of so great a son.

Beyond the geographical discovery just alluded to in connexion with the "primeval pair," the volume which Dr. Gross has edited contains little or no information of a really scientific character. The contagion of yellow fever is not determined, nor its treatment either. One thinks one thing, and one another. But we are taught (and we are thankful for the instruction) that primitive Christianity is not quite extinct; for Dr. Benjamin Rush "probably followed the advice of St. Paul—was very angry, but sinned not." (p. 42.) Something akin to despair, however, afterwards seizes us when we are informed that Dr. Nathaniel Chapman was a man who,

> "Take him for all in all,
> We shall not look upon his like again."

We should like to transfer the quotation from the man to the book. We are sorry not to hear of another Dr. Chapman; but we shall be equally sorry if it is our lot again to meet with an 'American Medical Biography.'

Review IX.

1. *Die Gesetze der Ernährung des Fleischfressers durch neue Untersuchungen festgestellt.* Von Dr. Th. L. W. Bischoff, Professor der Anatomie und Physiologie, und Dr. Carl Voit, Assistent an dem physiologischen Institut und Privatdocent in München.—*Leipzig* und *Heidelberg.* Wintersche Verlagshandlung 1860. pp. 304.

The Laws of Nutrition in Flesh-feeders, based on New Researches. By Dr. Th. L. W. Bischoff and Dr. Carl Voit, at Munich.

2. *De la Nutrition chez l'Homme et les Animaux.* Par le Dr. T. L. G. Bischoff. ('Archiv. Gén. de Méd.' Août, 1860. pp. 129–148.)

On Nutrition in Man and Animals. By Dr. T. L. G. Bischoff.

The "laws of nutrition," as established by Bischoff and Voit, are based on experiments performed on a single dog. The fact that only one species of flesh-feeders has been submitted to examination, and of this species only a single individual, may, in the opinion of many, detract something from the value of these researches: but the great number of experiments, and the careful and scientific manner in which they have been performed; the long space of time over which they are spread, comprising almost two years; the widely varying condition of the animal during the different series of experiments; the repetition of analogous experiments under varying external influences; and, with all this, the congruence of the results, entitle the authors' inferences to a high degree of confidence, or, at all events, to our most serious consideration.

In the introduction (pp. 1 to 41), the authors explain the relation of the present work to Bischoff's earlier essay* on the 'Urea as a Measure of the Metamorphosis of Tissue,' which may be considered as forming the basis of the researches before us. They further describe their view regarding the conservation and correlation of forces, as also regarding the metamorphosis of tissue. The peculiar arrangement or combination of the molecules of matter forming organisms, is, they maintain, owing to a "peculiar kind of molecular force." There are especially two kinds of substances which concern us at present, the non-nitrogenous—in the animal organisms the fat; and the nitrogenous—in the higher animals principally the muscular tissue. Through and simultaneously with the change of the combination of molecules in these substances, the molecular force to which they owe their combination is set free, and assumes one of those directions, in which we see the forces most active in animal organisms, especially warmth and motion.

"There is no doubt," the authors argue, "that the change of combination of both kinds of substances (non-nitrogenous and nitrogenous) is caused principally by the oxygen of the atmosphere. The change of combination of the non-nitrogenous organic substances, whether they may have become already constituents of the solid parts of the body, or as yet only of the fluid ones, is caused, according to our present experience, by the oxygen of the atmosphere alone; as the very minute division of these substances, and of the oxygen influencing them, perhaps also a peculiar modification of the oxygen (Ozon), combined with the molecular attraction, appears to be sufficient for this purpose. The only manifestation of force connected with this process is *warmth*." (p. 4.)

As fat and the allied substances in the animal organism are not spent in effecting *motion*, this second kind of force, the authors maintain, must be produced by the decomposition of the nitrogenous substances. It is in accordance with this view, that all organs exhibiting motion are formed of nitrogenous substances, and that the measure of the work executed is in proportion to the bulk of the organ. With regard to the manner in which this "change of combination of molecules," or, to use a shorter word, "decomposition" of nitrogenous tissues takes place, the authors assert that it cannot take place under the mere influence of the oxygen within the blood, but that the chemical attractions and repulsions going on within the nitrogenous tissues themselves are necessary to effect it.

As the principal factors in this metamorphosis, we find considered: 1. The organ itself; 2. The blastema of the blood; and 3. The oxygen of the atmosphere. The life or existence of the organ itself is regarded as depending on the constant interchange with the two other factors. The action of the blastema of the blood on the molecules of the organ is compared to the influence of "pressure;" the action of the oxygen

* Der Harnstoff als Maass des Stoffwechsels. Von Dr. T. L. W. Bischoff. Giessen, 1853.

to that of "traction." Neither the oxygen nor the blastema of the blood alone would be sufficient to cause a change in the combination of the molecules, but both together possess this power. The extent of the metamorphosis depends on the size of the organ, and on the quantity of the blastema and of the oxygen; it is the product of these three factors, and it is in direct proportion to them. The ingestion of food, according to the authors, increases the amount of the blastema of the blood, and through the action of this on the organ, the metamorphosis of tissue; the effect of the latter is the development of force, which is spent in the locomotion or circulation of the blastema. We can scarcely understand this proposition quite literally. The force set free by the decomposition of tissue, need surely not be spent merely in the locomotion of the blastema, but part of it appears, at all events, required for the assimilation of the food into blastema, and part of it also for the transmutation or raising of the latter into tissue.

With regard to the authors' view, that the increased quantity of the blastema causes the augmented tissue-change simply through the "pressure" on the organ, we would venture to think that it does so only through the intermediate action of the nervous system. The influence of the latter as a most important factor of the metamorphosis of tissue, and of nutrition in general, appears to us not sufficiently pointed out, although it is alluded to in several places. Thus, the authors say at page 17:

"The nerve is, therefore, another moment, a fourth factor, influencing the metamorphosis of tissue in the nitrogenous organs, in addition to the oxygen, the blastema, and the bulk of the organ itself. But while the action of these latter is probably uninterrupted, that of the nerve is only temporarily submitted to the influence of the will, and manifests itself, so to say, according to circumstances and intention with increased power. But it is evident, that the force set free through this agency is spent only in voluntary movements, and if, therefore, the force necessary for the uninterrupted internal movements is not to be defective, the metamorphosis of the respective constituents of the body—viz., their consumption or that of their food-substitutes must be increased in the same proportion."

It is evident that these remarks relate only to the action of the *voluntary nerves*, but we cannot help assuming that the action of *the nerves of nutrition* is more important to the change of tissue than is expressed in these words, and that it is as *uninterrupted* as that of the authors' three other factors. We are even disposed to think that the hypothesis which all these researches tend to prove—viz., that the albuminous substances are transformed only in the tissues and not in the blood, is materially strengthened by the view that, within the animal organism, it is the agency of the nervous system through which, in the combination of the molecules of the substances in question, that change is rendered possible which precedes their further decomposition. The laws of physics no doubt rule within the living organism, as well as in the inorganic world, yet we must be careful not to explain the most complicated phenomenon as regulated merely by "the quantity of the blastema and of oxygen," and "the bulk of the organ." We are, however, inclined to suppose that the authors, whenever they speak of the "organ," do not regard it as a mere mass of nitrogenous tissue, but as tissue, supplied with nervous elements superintending all its phenomena of nutrition.

We are, further, disposed to think, although the authors do not express it, that terms like "the bulk of the organ," refer not only to the nitrogenous constituents, but also to the liquid, together with the salts pervading the shape-giving substances, as it appears to us beyond any doubt that the inorganic constituents of the tissues play a most important part in their nutrition and metamorphosis.

We will now endeavour to give a short description of the authors' experiments and inferences, the subject of the former having been a young, but adult, strong farm-dog ("Hof-Hund"), whose weight varied between twenty-two and forty-one kilogrammes,* and who was kept in a cage 3′ high, 4′ wide, and 4′ deep. The animal was twice a-day taken out of the cage, and led about, with due precautions to avoid loss of urine and fæces; it was weighed every morning at eight o'clock, and the food, too, was always accurately weighed. The water consumed and the urine passed were measured. The determination of the urea and of the chloride of sodium was performed by Liebig's volumetric method; that of the sulphuric acid by a method slightly modified from the usual one—viz., by adding the solution of the chloride of barium to the warmed urine, an accurate description of this proceeding being given in an appendix to the work.

I. *Metamorphosis of tissue during starvation* (pp. 42–55).—Bischoff and Voit give three series of observations, of several days' duration each, and some of single days; the observations were made in the course of eighteen months, under varying circumstances. In order to show how the authors executed their experiments and calculations, we will subjoin the first series in detail. The weight of the solids is given in grammes, that of the fluids in cubic centimeters.†

"I. *Table on Starvation from October 19th to 25th*, 1857.

Date.	Weight of the animal in grammes.	Food.	Water.	Urine.	Urea.	Fæces.
"October 19	33·310	0	0	202	24·482	0
" 20	32·720	0	0	225	25·560	0
" 21	32·110	0	0	205	22·754	0
" 22	31·620	0	0	203	20·300	0
" 23	31·110	0	63·0	135	13·230	0
" 24	30·750	0	0	160	15·232	0
" 25	30·330	0	63·0	1130	121·558	0

"This table shows that the dog lost, during these six days, 2980 grms. of his weight, of which loss 2917 grms. at least were from its body.

"In order to learn what this loss consisted of, we first calculate from the quantity of urea excreted—121,558 grms.—and from the amount of nitrogen contained in it—56·73 grms. N, the loss which the animal sustained in nitro-

* A kilogramme = 2·6793 pounds troy, or 2·2046 pounds avoirdupois.

† A gramme is equal to 15·4325 grains troy; a cubic centimeter to 16·9 minims, or 15·4 grains troy.

genous tissue. Assuming the animal's flesh to contain 3·4 per cent. of N, the loss amounted to 1668 grms. of flesh. The remainder of the loss, 1312 grms., must have been fat or water, or both.

"It was, however, no doubt, *fat alone*, for if it had been water, the indispensable material for the respiration and calorification would have been wanting. According to the researches of Regnault and Reiset, a dog equal in weight to ours, under a flesh-diet, would consume daily 250 grms. carbon, and 900 grms. oxygen. If our dog had consumed only the carbon of the flesh just calculated, it would have amounted to less than 40 grms. in twenty-four hours, while, if the remainder of the loss were assumed to be fat, 180 grms. carbon would have been at its disposal. In the same manner, the dog, had it put to use only the flesh, would not have consumed more than 100 grms. oxygen per day; while, if the remainder of the loss had been fat, it would have required 644 grms. oxygen. It is, besides, a well-known fact, that starving animals become lean and lose fat, and there is, on the other side, no reason why the dog should have yielded water from his own body, while he could obtain from without as much as he wanted.

"The calculation of the urine, and of the cutaneous and pulmonary exhalation, further shows that the dog did not require more water than was supplied by the flesh consumed, and by the water formed at the expense of the hydrogen of the fat.

"We consider ourselves from all this, perfectly entitled to regard the dog's loss of weight as caused merely by loss of flesh and fat, and are therefore led to the following calculation:—

INGESTION.*

Food.	Water.	Nitrogen.	Carbon.	Hydrogen.	Oxygen.
1668 grms. of flesh from the body	1266·0	56·73	208·8	28·86	85·9
1312 grms. fat tissue with 1128·3 grms. fat	183·7	0	891·3	124·10	112·8
Water	63·0	0	0	0	0
	1512·7	56·73	1100·1	152·96	198·7

EGESTION.

1130 c. cm. urine	1048·0	56·73	24·4	8·20	32·6
Remaining for skin and lungs	464·7	0	1075·7	144·76	166·1

As the 144·76 grms. hydrogen form 1302·8 grms. water, the dog's excretion through skin and lungs amounted to 1767·5 grms. water, and 1075·7 grms. carbon." (p. 45.)

In order to examine the correctness of this manner of analysis, the authors have, in addition, calculated the loss through lungs and skin by the deficit between the loss through the urine and fæces, compared with the consumption of food and substance of body. In the series under consideration :—

"The dog consumed from its body	2980	grms.
Water	63	"
Sum-total	3043	"

* The term "ingestion" (*Einnahme*) is used here and in the other tables of the work for the substance consumed by the animal through the metamorphosis of tissue, including as well the material taken from the body as that received with the food.

It excreted in 1130 c. cm. urine	1186 grms.		
Leaving for skin and lungs	1857	„	
Our preceding calculation yields	1851	„	" (p. 45).

The slight difference between the figures obtained by these different manners of calculation, points to the correctness of the methods.

The loss of the dog during twenty-four hours, estimating its medium weight to be 31·8 kilogrms., was for one kilogrm. 0·30 grm. nitrogen, and 5·67 grms. carbon.

The warmth developed by the animal during twenty-four hours was computed from the carbon and hydrogen of the consumed tissue, a portion of the hydrogen (20·7 grms.) corresponding to the oxygen present in the same tissue (166·1 grms.) having been previously abstracted, as not contributing to the development of warmth. For the remainder of the carbon (1075·7 grms.), and hydrogen (124·06 grms.), the authors have adopted the figures of Favre and Silbermann—viz., for 1 grm. carbon 8086 units of warmth, and for 1 grm. hydrogen 34,462 units of warmth. According to this computation, the animal developed 12,976,466 units of warmth during the six days of the experiment; therefore during twenty-four hours, 2,162,744 units of warmth.

Bischoff and Voit are, however, aware that this method of calculation gives, at the utmost, only an approximative result; that it is doubtful whether the heat-creating power of fat, sugar, and similar substances, depends strictly on their per-centage of carbon and hydrogen, and whether the oxygen contained in combustible substances is to be considered as already combined with part of the hydrogen in the proportion of water. The heat-equivalents for carbon and hydrogen appear, besides, to be not yet ascertained with perfect certainty; those adopted by Liebig, for instance, in his twenty-eighth letter, are for carbon 7881, and for hydrogen 33,808 units; therefore perceptibly smaller than those of Favre and Silbermann.

The results of the authors' various observations on the phenomena of nutrition in starvation, do not materially differ from those of other observers; they show the minimum amount of tissue-metamorphosis under given circumstances; they prove that, the other influences remaining the same, the consumption of tissue is in exact proportion to the condition of the animal; that with the decrease of the weight of the latter, the quantity of tissue undergoing decomposition, decreases likewise; that this law is true, not only with regard to the entire animal, but also with regard to the single constituents of its body; that, for instance, a starving animal, which is relatively rich in fat, consumes a larger proportion of fat, one relatively rich in flesh more flesh. The latter phenomenon the authors explain, by supposing that the bulk of nitrogenous tissue being relatively great, a larger quantity of its substance undergoes a change of combination of molecules; that thus a larger amount of primary products of nitrogenous tissue-change is formed, which in their further oxidation occupy relatively much oxygen, leaving only a small quantity available for the oxidation of fat. The fact, on the other side, that a fat animal, when starving, decomposes relatively little flesh and much fat, is interpreted by the assumption, that the greater bulk of fat appropriates more oxygen, and thus diminishes its action on the flesh.

This interpretation may be correct, but others might have the same claim. Thus, for instance, in an animal comparatively rich in fat and poor in muscular tissue, the phenomena of innervation—and this we consider a point of great importance—may be considered to be less active, the quantity of nutritive fluids may also be conceived to be smaller, and proportionally to this, the amount of activity connected with their locomotion; the nitrogenous tissue-change under such circumstances would be less active, and as a *consequence* of this the consumption of fat would be relatively great; while, according to Bischoff and Voit, the presence of the greater bulk of fat would be the *primary cause* of its greater consumption, and the diminished waste of nitrogenous tissue its consequence. In an animal rich in nitrogenous tissue, on the contrary, the phenomena of innervation might be deemed more active, and also the quantity of nutritive fluids greater, requiring more activity in their locomotion; through all this, and not merely through the large bulk of nitrogenous organs, a comparatively considerable nitrogenous tissue-change may be effected.

The authors' calculations on the production of heat during starvation are important, as their inferences regarding the consumption of tissue in other series of observations are to some degree based on them. According to these calculations, the minimum of heat developed by the dog in question during twenty-four hours amounts to 2,200,000 units.

II. *On the metamorphosis of tissue during flesh-feeding* (pp. 56–97), the authors offer fourteen tables.—The flesh used was "good fresh cow's flesh; fat, bone, cartilage, and other admixtures, were always carefully removed." It seems that only some specimens of the flesh used have been submitted to analysis, and that these contained, at an average, scarcely one per cent. of fat. Considering the very great degree of care bestowed on these experiments, it would have appeared more satisfactory, if specimens of all the flesh used had been analysed, at least with regard to the per-centage of fat, salts, and water; but the manner in which the results of the various experiments agree amongst themselves, permits us to consider them as trustworthy, in spite of the slight deficiency mentioned.

A similar, though unavoidable inaccuracy presents itself with regard to the fæcal excretions. Their analysis has been performed only in an approximative manner; and, besides this, the animal's evacuations having been passed sometimes at long and uncertain intervals, the authors were often obliged to apportion, by calculation, certain quantities to former series of observations.

The fourteen series of observations on pure flesh-feeding exhibit great variety, not only with regard to the quantity of flesh fed, but also with regard to the state of the animal's nutrition at the beginning of each series. Every single series offers points of great interest; we will, however, mention only the results of the eleventh more in detail.

The dog, after having been fed during several days merely on broth and bread, weighed at the commencement of the experiment 32,990 grms. The animal then took, on three successive days, 2000 grms. of flesh daily, therefore 6000 grms. in all. It lost during this period, the fæces having been accounted for, 89 grms. in weight.

"What did this loss consist in? The 6000 grms. flesh contained 204 grms. nitrogen. The dog excreted 380·86 grms. urea, with 177·75 grms. nitrogen; the 94 grms. fæces contained 28·56 grms. solids and 65·44 grms. water, and in the former 1·86 grm. nitrogen. The dog excreted, therefore, in all, 179·61 grms. nitrogen, or 24·39 grms. less than it had received in the flesh. These correspond to 717 grms. flesh, which, therefore, the animal had appropriated. As, however, notwithstanding this, it lost 89 grms., it must have yielded 806 grms. of water or fat." (p. 86.)

"During twenty-four hours the animal excreted:

In the urine.		In the fæces.		Through skin and lungs.
1348·00 water.	...	21·81 water.	...	587·69 water.
59·25 nitrogen.	...	0·62 nitrogen.	...	190·95 carbon.
25·39 carbon.	...	4·13 carbon.		

This gives for 1 kilogrm. dog during twenty-four hours, the average weight being 32·76 kilogrms., 1·83 grms. nitrogen and 6·73 grms. carbon.

"With regard to the production of warmth, 166·85 oxygen require 20·86 hydrogen. The combustion of the remaining 43·33 hydrogen, and 572·86 carbon, yields 6,125,384 units of heat, and for twenty-four hours 2,041,795 units." (p. 87.)

As the minimum of warmth for twenty-four hours amounts to 2,200,000 units, there is for every day a deficit of 158,205 units, corresponding to about 16 grms. fat which the animal consumed from its own body; the dog lived, therefore, during the three days, on 5283 grms. flesh, about 48 grms. fat, and 1238 grms. water (viz., 758 from its body, and 480 grms. taken during the experiment).

This observation proves, according to our authors, that the amount of nitrogenous tissue-change depends, to a great degree, on the condition of the animal, and especially on the bulk of the nitrogenous organs. In the present instance the animal appropriated, during three days, 717 grms. of nitrogenous substance out of 6000 grms. ingested during that period; while in another experiment, 4000 grms. which the animal took within two days, were not sufficient to repair the loss sustained during the same period by the metamorphosis of tissue. In the latter case the animal, just before the experiment, had received 2500 grms. flesh daily, while it had been fed on broth and bread previously to the series under consideration. The nitrogenous tissue of the animal, after the flesh-feeding, may be assumed to have been in good condition, while during the bread and broth diet a part of it had, we may suppose, been wasted and supplanted by water. This supposition is rendered still more probable by the fact, that during the experiment just detailed, the dog lost water and appropriated nitrogenous tissue.

This observation teaches us also, that the weight alone cannot always entitle us to form conclusions regarding the state of an animal's nutrition, for in spite of the considerable gain in nitrogenous substance, and the only small loss of fat, the dog's weight had decreased through the loss of water. It was by basing his calculations on the weight alone, that Bischoff in his former essay, 'On the Urea as a Measure of the Metamorphosis of Tissue,' was led to maintain, that not all the nitrogen is eliminated by the kidneys, but that about one-third of it escapes in some other way than as a component of urea. It is, however, quite clear that a dog fed on two pounds of flesh may exactly keep his weight, not only by transforming these two pounds into tissue and losing two by decomposition of tissue, but also by consuming through the tissue-change a pound and a half of flesh and half a pound of fat, or two pounds and a half of flesh, and retaining half a pound of water. Bischoff himself explains his former error, and believes now, with his joint author, that, by a fundamental law, all the nitrogen in the dog, with insignificant exceptions, is excreted through the kidneys as urea. It will be remembered, that in this respect Bischoff and Voit are in harmony with Bidder and Schmidt, who have established the law in question, with regard to carnivorous animals in general, in their well-known treatise 'On the Digestive Fluids and the Metamorphosis of Tissue.'

Amongst the results from the experiments on flesh-feeding alone, we further remark, *that the dog can be maintained in good condition by flesh alone, without losing either flesh or fat; that this, however, requires an amount of flesh equal to one-twentieth to one-twenty-fifth of the entire weight of body.* This experience is rather opposed to the view of Liebig, who thinks it "exceedingly doubtful whether these substances, considering their properties, would, in the circumstances under which they are presented to oxygen in the organism, produce the necessary temperature of the body, and compensate for the loss of heat, for of all organic compounds, the plastic constituents of food are those which possess in the lowest degree the properties of combustibility and of developing heat by their oxidation."

"Of all the elements of the animal body, nitrogen has the feeblest attraction for oxygen; and what is still more remarkable, it deprives all combustible elements, with which it combines to a greater or less extent, of the power of combining with oxygen—that is, of undergoing combustion."*

It might be said, in support of Liebig's view, that flesh is never altogether free from fat and non-nitrogenous elements, yet, if we may accept the authors' assertion, that the flesh supplied by them to the dog contained rather less than one per cent. of fat, their inference would not be materially restricted owing to the presence of so small a proportion. From the experiments on mixed flesh and fat feeding, it appears, at all events, to result, in opposition to the second remark of Liebig's, that, within the animal body, the oxygen has as great, and even a greater affinity to the primary products of metamorphosis of the nitrogenous tissues (not to the unchanged tissues themselves) than to fat.

Bischoff and Voit further infer, that *if the flesh offered to the dog*

* Liebig's Familiar Letters on Chemistry. Fourth edition, edited by John Blyth, M.D.' p. 394. London, 1859.

is less than one-twentieth to one-twenty-fifth of its entire weight, it consumes in addition a part of the nitrogenous and fat-tissues of its body.

If the dog receives more than is necessary for the repair of its tissue, it appropriates the surplus. As soon as, through this, the animal's weight has become increased, a proportionally larger amount of food is required to repair the necessary tissue consumption, and a still larger one in order to ensure further increase. If the latter is offered in an increasing proportion, the dog arrives at last at a maximum weight; it then ceases to take food, and consequently, the metamorphosis of tissue being uninterrupted, rapidly loses weight, until it again acquires the power of taking food.

The quantity of flesh necessary to repair the loss effected by the tissue-change, or to cause increase of flesh, depends on the amount of nitrogenous tissue in the body, the animal possessed of relatively much flesh requiring more than that poor in flesh.

The amount of nitrogenous tissue-change becomes increased by the ingestion of nitrogenous food, and in proportion to the amount of the latter. If the animal receives only the quantity corresponding to the tissue-change during starvation, it continues to lose weight, because the amount of labour to be performed is augmented, and consequently the decomposition of tissue increased, through the ingestion of food and the larger amount of blastema thus produced. The decomposition of nitrogenous tissue would in this manner, the authors maintain, always go on increasing with the increase of ingestion; the animal would perish from starvation, in spite of the increased amount of food, if not, the quantity of oxygen available being limited, at last the point would be reached when the whole of the oxygen would be occupied by the primary products of the metamorphosis of the nitrogenous tissue for their further oxidation. It is only then that the animal ceases to lose, and that if a further addition is made to the food, it begins to gain weight. By the progressing increase of ingestion the weight of the animal may then be progressively raised unto that limit, when, as already mentioned, and as shown by Table XII., on flesh-feeding, the animal becomes unable to take food for a time, until a certain amount of retrogressive tissue-change has taken place.

These experiences, and the relation of the excretion of urea to the ingestion of various quantities and qualities of food, and to the tissue-change during starvation, lead the authors repeatedly to discuss the question of the source of urea, and the theory of the so-called "*luxus-consumption*,"—that theory which teaches that, whenever more albumen is introduced into the system than is necessary for the repair of the nitrogenous tissue-change, this superfluous albumen is at once oxidized within the blood, without having previously entered into the formation of tissues, and that this immediate decomposition of albuminous substances is the cause of the rapid increase of urea after large meals (Lehmann, Frerichs, Bidder and Schmidt). Bischoff and Voit hold this theory as entirely unfounded, and maintain with Berzelius, Müller, and Liebig, that it is impossible that urea can be formed rom albumen in any other way than by the decomposition of the nitrogenous tissues, and that the quantity of nitrogenous tissue metamorphosed is measurable by the amount of nitrogen in the urine.

It will be remembered that Bischoff already, in his former essay (1853), rejected the theory of "luxus-consumption," supporting Liebig's view, that the albumen in the alkaline blood resists the influence of oxygen, and that, without this resistance, nutrition, through the agency of this principle, would be impossible. The experiments in the work before us tend much to strengthen this view, and to invalidate that of the "luxus-consumption," at all events in carnivorous animals. The facts, that the starving animal forms urea, which can have no other source than that of tissue-change, and that the quantity of urea formed by the starving animal increases or decreases with the greater or smaller bulk of nitrogenous organs; the further circumstance that the animal fed with an insufficient amount of nitrogenous food, yields a quantity of urea, not only corresponding to this, but exceeding it; the additional consideration, that when more nitrogenous food is ingested than is required for the repair of the nitrogenous tissue-change, this surplus food leads to increase of the nitrogenous organs, and is not at once decomposed in the blood and excreted as urea, and that, therefore, less nitrogen is excreted than ingested; and, lastly, the observation that the animal, when fed to such a degree that the oxygen of the blood is not sufficient to oxidize the primary products of nitrogenous tissue-metamorphosis, ceases for the time to take food, and loses weight, until the oxidation of these products has taken place, and oxygen becomes again disposable for further metamorphosis and development of force; all these points render it improbable that albumen is oxidized within the blood, previously to having entered into the formation of nitrogenous tissue.

III. *Metamorphosis of tissue under flesh and fat-diet, and fat alone.* (pp. 97–153.)—We find under this head seventeen observations on fat and flesh, one on flesh alone, and one on fat alone. In the first series, the animal weighing 28,480 grms., received during ten days (from November 22nd to December 2nd, 1857) 150 grms. flesh and 250 grms. fat daily. It lost only 161 grms. of its weight during the whole period; but the calculation from the excretion of nitrogen proved that the loss in nitrogenous tissue amounted to 830·3 grms., while the dog gained about 300 grms. fat and 370 grms. water. The daily nitrogenous tissue-change averaged 233 grms., which is rather more than it would have been in the fasting animal, but less than if 150 grms. flesh had been given without fat, and in addition, fat was appropriated. In another series of 32 days, the animal weighing 28,890 grms. received daily 500 grms. flesh with 250 grms. fat. It gained daily 58 grms. flesh and 76 grms. fat; while in another series,* when the dog had 600 grms. flesh alone, it lost every day 82 grms. flesh and 106·64 grms. fat. These experiments show still more distinctly than the former, that by the addition of fat to nitrogenous food, the amount of nitrogenous tissue-metamorphosis is perceptibly depressed; but that, notwithstand-

* The fifth table on flesh-feeding. The authors at p. 106, when referring to the results of the experiment illustrated by this table (p. 68 and 69), state the *daily* loss of *fat* as 248 grms., while this figure applies to the *fatty tissue* lost during the *two days* of the experiment, the loss for one day being 124 grms. fatty tissue, with 106·64 grms. fat.

ing the addition of fat, an increasing amount of nitrogenous food leads to increased change of nitrogenous tissue.

The author's general inferences regarding the influence of fat as food are: 1. *That the metamorphosis of the nitrogenous constituents of the body, and the consumption of flesh for their restoration, are not impeded by the use of fat.* 2. *The increased metamorphosis of nitrogenous tissues through increased ingestion of flesh, is not prevented by the addition of fat, but is augmented in the same manner as if increased quantities of flesh alone had been taken.* 3. *Fat even increases the metamorphosis of nitrogenous tissues, and does this in proportion to its quantity.* This third inference, although supported by the majority of the authors' observations, seems not to be quite in accordance with Tables XIV., XV., and XVI., on flesh and fat feeding; each table exhibiting a series of three days, the dog having received in the first series 500 grms. flesh, and 100 grms. fat daily; in the second, 500 grms. flesh and 200 grms. fat; in the third, the same amount of flesh with 300 grms. fat. The animal's condition in all these three consecutive series, was almost the same. In the first series the daily metamorphosis of nitrogenous tissue amounted to 463 grms., in the second to 500 grms., and in the third to 456 grms.

The cause of the increased nitrogenous tissue-change, induced by fat, the authors explain by the supposition, that fat is not directly burnt off in the blood, but previously enters into the metamorphosis of tissues. The manner in which the authors suppose this to be effected, is not quite clear to us. An indisputable cause appears to us to be in the fact, that the assimilation and circulation of an increased quantity of fat requires the expenditure of more force than is required for a smaller amount of fat, and that this additional force can only be developed by an increased decomposition of nitrogenous tissue. But there seems to be another cause for nitrogenous tissue-change in the storing up of fat in the body; the deposition of fat, and especially the formation of the surrounding cellular membranes for the fat globules, may be thought to be connected with some consumption or rather conversion of force, and some metamorphosis of nitrogenous tissue which would become increased in proportion to the quantity of fat ingested.

The fourth inference is, *that fat invariably checks the metamorphosis of nitrogenous tissue by a definite amount which exceeds the increased tissue-change noticed under the preceding head. Thus the quantity of flesh, when given simultaneously with fat, may be reduced to one-fourth or one-third of what it ought to be, when given alone, in order to keep the body at its normal weight.*

The authors' view, as far as we can understand it, is that fat does not prevent the tissue-change necessary for the development of the requisite amount of motor force, but that the oxygen available after the oxidation of the products of this necessary tissue-change, is, when fat is present, occupied by this latter, and thus prevented from inducing further decomposition of nitrogenous organs. Fat acts, therefore, by diminishing the influence of one of the factors of the tissue-change, and the fat ingested with the food seems to possess this power in a higher degree than the fat stored up in the body and encased in nitrogenous membranes. If, however, more flesh is given than is necessary for the reconstruction of the tissue used up in the development of the necessary motor force, then the case becomes, with regard to the nitrogenous tissue, the same as if no fat were added to the flesh—viz., the excess of the latter leads to increased tissue-change, and is not stored up; but if the oxygen available after the oxidation of the nitrogenous products of tissue-change is not sufficient to oxidize also the fat taken with the flesh, then a part or the whole of the fat is retained in the body. It is this manner of feeding which Bischoff and Voit regard as real "luxus-consumption," because they see in it an unnecessary waste of nitrogenous substance, the excess of the latter being, so to say, used up merely for its own sake, to produce the necessary force for its assimilation and locomotion within the body. According to this supposition, men accustomed to take the amount of nitrogenous food and fat just necessary, would, by adding more nitrogenous food, not increase the bulk of their nitrogenous tissues, but would rather store up fat; by the addition of fat, too, the waste, consequently the necessary supply of nitrogenous constituents, could not be diminished. The question is of so great importance for the physiologist as well as for the farmer, that repeated experiments on different species of animals, under varied conditions, and also on man, are very desirable.

The researches of others on this subject have been, hitherto, rather scanty when compared with those of our authors. Thus, Botkin* found that a dog weighing 10,099 grms., when fed on a pound of flesh and 200 grms. of water daily, had, during seven days, an average excretion of 31·96 grms. of urea, and decreased in weight to 8721 grms.; while the same dog, during the seven days following, when he received, in addition to the diet mentioned, 81 grms. of fat daily, excreted only 24·302 grms. of urea, and increased in weight to 9583 grms. In this experiment, therefore, the addition of fat to the food had not increased the change of nitrogenous tissue, but had considerably diminished it.

Lang's† and Mettenheimer's‡ observations relate principally to the excretion of fat through the urine. Böcker's§ experiments on four healthy young men who used first a diet without butter, and then with a varying quantity of butter (from 2 to 90 grms.), led to the inference, that addition of fat to the diet exercises no distinct influence on the water, the urea, the uric, phosphoric, and sulphuric acids, the magnesia, the soda, and the potash of the urine. Butter, however, appears to us not the most fit kind of fat for such researches, as well because large quantities of it often derange the digestion, as also because the amount of water, casein, and salts admixed with it, is variable.

* Zur Frage des Stoffwechsels der Fette im thierischen Organismus: Virchow's Archiv, vol. xv. p. 380. 1858.

† De adipe in urina et renibus hominum et animalium. Dorpat, 1852.

‡ Ausscheidung von Fett durch den Urin: Archiv des Vereins f. wissensch. Heilkunde, vol. i. p. 374. 1859.

§ Oesterhen's Zeitschrift f. Hyg., vol. i. 1859.

IV. On *flesh and sugar-feeding combined* the authors have eight experiments, and one on *sugar-feeding alone.* The sugar used was generally grape-sugar, only in one instance milk-sugar. The results obtained with both kinds of sugar are analogous, but the milk-sugar seemed to cause diarrhœa.

Of great interest is a series of five observations, from the 19th of June to the 5th of July, 1859. The following synopsis shows the relation of the consumption to the ingestion:

The dog received (ingestion):

500 grms. flesh alone.
500 grms. flesh and 250 grms. fat.
500 grms. flesh and 100 grms. sugar.
500 grms. flesh and 200 grms. sugar.
500 grms. flesh and 300 grms. sugar.

The dog spent (consumption or egestion):

564 grms. flesh and 161 grms. fat.
557 grms. flesh and 175 grms. fat.
537 grms. flesh, 151 grms. fat, and 100 grms. sugar.
500 grms. flesh, 76 grms. fat, and 200 grms. sugar.
466 grms. flesh, 34 grms. fat, and 300 grms. sugar.—(p. 177.)

As the weight of the animal was almost the same in all these series, it results: 1. That the decomposition of nitrogenous tissues was greatest when 500 grms. flesh alone were taken, and that the addition of fat and sugar depressed the amount of this decomposition. 2. That fat exercised this influence to a less considerable degree than sugar. 3. That this influence of sugar became increased with the increasing quantity of the latter. 4. That the fat in the food diminished the consumption of the fat of the body. And 5. That the sugar had the same effect; but that even the addition of 300 grms. to the food was not sufficient altogether to prevent the consumption of the fat of the body.

"It results from this," the authors reason, "that fat and sugar play opposite parts with regard to the nitrogenous tissue-change and the respiration. Fat is, by virtue of the two and a half times larger proportion of carbon and hydrogen, much more powerful than sugar with regard to the respiratory process, but with regard to the nitrogenous tissue-change the influence of sugar is greater. As both fat and sugar can effect the diminution of the nitrogenous tissue-change only by possessing themselves of the oxygen, and fat ought therefore to act two and a half times more powerfully than sugar, the latter, however, on the contrary, acts more powerfully than the former; it is evident that fat, in addition to its checking influence on the nitrogenous tissue-change, owing to the appropriation of the oxygen, must also, in some way, directly increase this change. Fat has, then, a double action; by means of its carbon and hydrogen it appropriates the oxygen of the blood, and thus lessens the nitrogenous tissue-change; but on the other side, it directly increases the latter, for it evidently in some manner enters the tissue-change before its elements combine with oxygen. The former action is, however, greater than the latter, therefore the general effect is always a depression on the nitrogenous tissue-change. The second effect, the augmentation of the nitrogenous tissue-change, remains, therefore, usually unnoticed; but as we have seen it appearing already on a former occasion, when the quantity of fat in the food was increased, thus it is rendered here, when compared with the action of sugar, still more evident. Sugar does, probably, not at all possess this influence of increasing the nitrogenous tissue-change; it acts by being directly burnt in the blood, merely through its carbon and hydrogen depressing on the nitrogenous tissue-change. Sugar, therefore, when increased in quantity, exercises this influence with increasing intensity." (p. 178.)

We are even after this reasoning not yet quite convinced that fat must enter, in some way, into the nitrogenous tissue-change before it is oxidized; nor can we see why sugar should in no manner whatever increase this tissue-change. The absorption of sugar, its endosmotic influence on the water of the tissues, its locomotion and circulation in the body previously to its being completely oxidized, appear to us to require some motor force, therefore some nitrogenous tissue-change, according to the authors' view, that motor force can only be developed by the nitrogenous tissue-change; but the depression of the latter, through the appropriation of the oxygen, is so great that this increase remains unnoticed. Fat may require more force, already in the process of digestion, and it is, as we know for certain, not so easily oxidized as sugar is; we may therefore suppose that its stay in the body is longer, that its locomotion involves a greater amount of nitrogenous tissue-change, without our being obliged to resort to the theory that fat itself enters into this tissue-change "by forcing molecules of nitrogenous tissue out of their place."

With regard to the other results from the experiments on sugar and flesh-feeding, it is evident that sugar, too, cannot prevent the necessary nitrogenous tissue-change. The conversion of sugar into fat in flesh-feeders, is considered by the authors as improbable. In the experiments before us, the animal certainly never took so much sugar as to leave any part of it unaccounted for after the calculation of the temperature; but this does not prove the impossibility of such a conversion under other circumstances. If it, however, were even proved that pure flesh-feeders never transform sugar into fat, this would not invalidate the experience of Persoz and Boussingault, that in herbivora "the sugar formed in digestion from the starch of grain, potatoes, and leguminous seeds is converted into fat, when sufficient materials are supplied for the formation of cells." Lawes and Gilbert,* too, have proved the transformation of starch into fat, showing that in the fattening of pigs "there were four or five times as much fat stored up in increase as there was of fatty matter in the food."

The investigations of most of the other observers can scarcely be compared with those of Bischoff and Voit, yet we may refer to the interesting researches of F. Hoppe,† who likewise experimented on a dog, and obtained the following results:—"When sugar and flesh were given together, the weight of the animal increased much more rapidly

* Lawes and Gilbert: Experimental Enquiry into the Composition of some of the Animals fed and slaughtered as Human Food: Proceedings of the Royal Society, vol. ix. 1859.

† On the Influence of Cane Sugar on Digestion and Nutrition: Virchow's Archiv, vol. x. 1856.

than when flesh alone was given. . . . When sugar and flesh were consumed, urea was excreted in smaller quantity than when flesh alone was taken. . . . By exclusive sugar diet the excretion of urea was depressed to its lowest amount." Hoppe further holds that "by the presence of much sugar in the blood, the albuminous substances are preserved from oxidation. The albumen thus stored up appears to become decomposed under the development of fat. In this manner sugar produces fattening only, when, at the same time, albuminous substances are liberally supplied." To this kind of influence of sugar on the formation of fat our authors do not at all advert, and without accurate experiments including the calculation of all the ingesta and egesta, and of the temperature, Hoppe's theory cannot be regarded as proved, though well deserving careful consideration. In some of Bischoff and Voit's experiments with excessive flesh, and with flesh and gelatine feeding, the temperature calculated from the nitrogenous substances alone would far exceed the minimum production of heat (2,200,000 units), amounting to more than 2,700,000 units per day. Such instances Hoppe and the supporters of the theory of the formation of fat from albuminous substances might, perhaps, use in their favour, but not without additional and more cogent arguments.

V. The experiments on *flesh and starch-feeding*, and on *starch-feeding alone*, led to similar results as those on sugar-feeding.

VI. On the *influence of bread-feeding* the authors have two series, one of which extends over forty-one days.

The facts that some dogs live almost entirely on bread, and the circumstance that Magendie had found bread an adequate article of food for dogs, increase the interest of these experiments. The quality of bread used was the German rye-bread, of which the animal was allowed to take as much as it liked. The digestion of the bread was imperfect, a considerable portion of the nitrogenous and non-nitrogenous constituents passing through with the fæces. The animal constantly consumed the flesh and fat of its own body, which, however, would perhaps have ceased if the experiment had been proceeded with, after the dog was reduced to a low condition, as then the diminished bulk of the organs would have caused a proportionally diminished tissue-change. Remarkable in these experiments is the trifling loss of weight when compared with the considerable loss sustained in flesh and fat, a fact which, as in another case mentioned before, is explained by the retention of water in the tissues. This retention of water may perhaps have occurred also in Magendie's dog, and may have given rise to the opinion that the animal was in good condition, this judgment having probably been arrived at merely by the consideration of the weight and the external appearance.

VII. The last series of the authors' researches relates to feeding with *gelatine and flesh*, with *gelatine and fat*, and with *gelatine alone.*

In contradiction to the well-known opinion of Magendie and the commission of the French Academy, and also to the view pronounced by Liebig,* and by Bischoff himself in his former work on "Urea,"

* Familiar Letters on Chemistry, edited by Dr. Blyth, p. 443. 1859.

the authors have now arrived at the conclusions, *that gelatine possesses real nutritive value; that it forms to some degree a substitute for other plastic matter; and that, by its addition to the food, the quantity of other nutritive substances may be, without disadvantage, considerably diminished.* The theory, that gelatine plays the part of respiratory food, is now repudiated, on the ground that gelatine being combined already with a large per-centage of oxygen, produces by its oxidation only a small amount of heat, and does not save the consumption of fat.

Table II. shows that the animal, weighing 33,040 grms. at the beginning of the experiment, lost, when fed with 200 grms. flesh and 200 grms. gelatine, 245 grms. flesh and 110 grms. fat; it therefore consumed daily 445 grms. flesh, 200 grms. gelatine, and 110 grms. fat.

In the next following experiment, described in Table III., the animal received 200 grms. flesh and 300 grms. gelatine; the daily loss amounted to 77 grms. flesh and 95 grms. fat; the consumption, therefore, to 277 grms. flesh, 300 grms. gelatine, and 95 grms. fat. By comparing the results of these two experiments, we perceive that the addition of 100 grms. gelatine caused a saving of 168 grms. flesh and 15 grms. fat of the body, a small subtraction being, however, necessary on account of the loss of weight sustained by the animal during the first experiment.

In the sixth experiment (Table VI.), the dog, weighing 37,060 grms., had only 200 grms. gelatine, and water according to desire; the daily loss consisted in 83 grms. flesh and 149 grms. fat, which, compared with the results of the experiments on pure flesh-feeding or starvation, is very small.

The gelatine used was the French gelatine, which, according to the authors, may perhaps contain more chondrin than other kinds of gelatine, and even a small proportion of albumen. This fact, if proved, would rather impair the value of the results. Bischoff and Voit, however, to use their own words regarding this important subject, find it—

"Indisputable, that gelatine may replace the albumen; that it would even not be unreasonable to think it might play its part altogether. But the insurmountable difficulty, rendering gelatine an inadequate aliment, lies in the circumstance that it cannot replace the albumen, except when given in very large quantities. It requires, as we have seen, at least four times as much gelatine as dry flesh, or just as much dry gelatine as undried flesh, to produce the same effect. This quantity, with the necessary admixture of water, no animal can digest (*bewältigen*)." (p. 241.)

The authors, reasoning in this manner, seem not to take into consideration the proportion of the inorganic constituents contained in flesh and in gelatine; and yet these inorganic constituents are all-important in the process of nutrition. The per-centage of these in gelatine is very small; in flesh, on the contrary, very considerable. The urine, therefore, under the influence of gelatine feeding, contains a much smaller amount of salts, in proportion to urea, than under that of either flesh feeding or starvation. Flesh itself, when deprived of its saline constituents, cannot support nutrition, and is soon refused by animals. Gelatine, without the addition of the inorganic constituents of the flesh, would as certainly remain an inadequate aliment, even if animals were to take it in the amount regarded necessary by Bischoff and Voit. We wish, however, by this remark not to invalidate the authors' view, as far as it regards the *admixture* of gelatine to flesh and fat, as through this a saving of both the latter, but especially of flesh, may be effected. We also consider their experiments as corroborative of the general belief, that to the sick, who cannot take solid food, gelatinous articles of diet are of great value, by causing a less rapid consumption of the constituents of the body.

Among their general inferences, the authors advert to *Liebig's division of the aliments into plastic and respiratory*, considering such a division as fully justified by their experiments. They propose, however, for the former the appellation *dynamogenic* or *kinaesogenic;* for the latter, that of *thermogenic* aliments. The authors' view is, that in the animal organism all kinds of motion are, under all circumstances, caused by the decomposition of nitrogenous tissues, never by that of non-nitrogenous substances, the only function of the latter being the development of warmth. They differ from Liebig, by assuming that the primary products of nitrogenous tissue-change are, even in the presence of non-nitrogenous substances, oxidized before the latter, and may, if present in sufficient quantity, yield all the heat required, and thus prevent the oxidation of fat.

With regard to the nomenclature, we are not quite sure whether much is gained by the new names. The expressions "dynamogenic" and "kinaesogenic" appear to apply merely to the display of force as motion; but the action of the mind and senses is, no doubt, likewise intimately connected with nitrogenous tissue-change; and yet, we suppose, the authors would scarcely call this manifestation of force "kinaesis," which word they evidently use as synonymous with "dynamis." Respecting the term "thermogenic," we admit that the principal effect of the decomposition of the non-nitrogenous substances consists in the production of heat; but that this is the only effect, and that this decomposition, and the heat developed through it, *do not* assist in the capillary circulation, and therefore in the motion of the fluids, appears to us not at all proved.

The authors finally discuss the applicability of their inferences as *laws of nutrition.* They seem to feel themselves, to some degree, that their experiments have been as yet rather too limited to permit us to consider their results as general laws. With regard to *man,* they acknowledge that their experiments cannot lead to indisputable conclusions, except when it is proved that all the nitrogen is excreted as urea through the kidneys, or that the quantity excreted in a different manner is likewise measurable. It is, however, well known that in man some nitrogen passes off through the urine as uric acid, as hippuric acid, as pigment, and occasionally also as ammonia and as kreatin; some through the skin as urea, some with the fæces, and some perhaps with the breath as ammonia. It is, therefore, necessary either that the nitrogen passing off in all the excreta be measured, or that a certain proportion be found between the urea of the urine and the nitrogen excreted in other ways.

There will be, besides, other difficulties met with in man, through the much more complicated influence of the nervous system in general, and of the intellectual activity in particular. We know that the differences in this respect, between different persons, and also in the same person, at different times, are very great. As yet we know very little concerning the intensity of tissue-change connected with the work of the mind and of the senses; we possess even not yet an accurate knowledge with regard to the proportion of tissue-change attributable to certain amounts of mechanical labour. The Rev. S. Haughton* has published some investigations, from which it would appear that the metamorphosis of nitrogenous tissue connected with mental labour is very considerable. Thus, he infers that "men employed only in manual and routine bodily labour are sufficiently well fed on vegetable diet, and discharge, on an average, 400 grains of urea per day, of which 300 grains are spent in vital and 100 in mechanical work." As Mr. Haughton takes a man at the average weight of 150 pounds, the vital work for every pound would yield 2 grains of urea during twenty-four hours. He calculates that the force which is equivalent to raising 100 tons one foot high in twenty-four hours requires a decomposition of tissue yielding 38·69 grains of urea; 100 grains of urea would therefore correspond to a force raising 258 tons one foot high. An hour's severe study is connected with the excretion of 43 grains of urea, while an hour's lighter mental work corresponds to the excretion of 27·71 grains. "When the work is of a higher mental order," Mr. Haughton argues, "a better quality of food must be supplied, sufficient to allow a discharge of 533 grains per day of urea, of which 300 grains, as before, are spent in vital work, and 233 grains in mental and the mechanical work necessary to keep the body in health." But Mr. Haughton's experiments do not extend over a sufficient space of time to allow us as yet to admit his conclusions as proved. He farther bases his calculations only on the nitrogen excreted as urea in the urine, which appears to be not quite correct for *man*, although it may be so for *carnivorous animals*.

Another difficulty in the researches on man will probably be found in the influence of dressing and cooking on the various articles of food. The same kind of meat probably influences the tissue-change in a different manner when it is taken raw or underdone, or when taken roasted, or boiled, or baked, or salt. In the salt meat, for instance, as it is generally prepared, a considerable portion of the mineral constituents is lost, and this necessarily must impair its nutritive value; it appears even doubtful whether all the nitrogenous constituents of such meat can be transformed again into muscular tissue. As, according to the author's experiments, gelatine is oxidized within the blood, so, we should be inclined to think, some of the nitrogenous constituents of our food altered through the process of cooking undergo their

* On the Natural Constants of the Urine of Man, by the Rev. S. Haughton, F.R.S.: Dublin Quarterly Journal of Medicine, vol. xxviii. p. 17, and vol. xxx. p. 1.

further decomposition within the body without having previously entered the actual tissue-change. We purposely use the expression "actual tissue-change," as we do not mean to say that such substances are oxidized literally within the walls of the bloodvessels, only that they do not become again part of the nitrogenous tissue of the body, although they may repeatedly, in the course of their circulation through the body and further metamorphosis, leave the bloodvessels and be reabsorbed again.

All these and other difficulties, however, ought not to deter from careful experiments on man; they render them, on the contrary, more desirable; and Bischoff and Voit have certainly the merit of having shown, in many points, the way in which such researches are to be carried out.

In an appendix to their work, the authors offer some valuable observations on some methods of analysis of the urine, and on the composition of the fæces of the dog under the influence of various kinds of food; they give, besides, tables showing the composition of the food and the consumption of the dog in twenty-four hours during the various experiments.

In their observations on the urine, the authors remark that though the *sulphuric acid* of the urine rises and falls in almost the same proportion as the urea, yet the dog's urine does not contain all the sulphur in the shape of sulphuric acid, but a considerable portion of it in some organic combination, which is not precipitated by chloride of barium. The amount of this non-oxidized sulphur increases with the increased nitrogenous tissue-change.

In taking leave of the work before us, we do not hesitate to confess that we think it entitled to rank high among the physiological researches of the present age, and that it does much credit to Bischoff and Voit, not only for the great industry, but also for the truly philosophic spirit exhibited as well in their experiments as in their reasoning.

REVIEW X.

The Physical Examination of the Chest in Pulmonary Consumption and its Intercurrent Diseases. By SOMERVILLE SCOTT ALISON, M.D. Edin., Physician to the Hospital for Consumption and Diseases of the Chest, Brompton.—1861. pp. 447.

ON turning from the great work of Laennec to the treatise the title of which stands at the head of this article, we are at once made aware of the great extension of knowledge as to the phenomena to be elicited by the arts subservient to physical diagnosis, which has taken place during the forty years that separate their publication. The only signs of incipient pulmonary consumption recognised by Laennec are summed up in a few lines; they may be referred to the two heads of *diminished resonance on percussion* and *diffuse broncophony.* Dr. Alison devotes to their consideration more than a hundred pages of a closely-printed octavo volume, and we are not aware that to one of the more salient characters he describes is a place now denied amongst the generally admitted signs of the disease. We do not entirely acquit the author of a fondness for over-refinement, or of occasional reiteration. Some will consider the latter scarcely a fault; in the present case, the former may, we think, be readily excused. The science in which Dr. Alison has rendered himself a proficient is one in which, within certain limits, advance and refinement are synonymous. However it may be desirable for the learner that the varying gradations of sound which arise within diseased tissues should be epitomized under a few comprehensive formulæ, we must believe that an exact recognition of the morbid conditions on which such phenomena depend is not to be obtained by loose generalization. This treatise would be out of place in the hands of the mere student, but by the careful and painstaking physician, whose aim it is to increase his power of detecting disease by every hint which experience can afford, and by every means which ingenuity can suggest, Dr. Alison's labours will be fully appreciated. Although on some points we may be at variance with the author, we would at the outset most willingly accord to his work the praise due to a most complete, comprehensive, and elaborate treatise on the signs of tubercular disease of the lungs.

The question, whether greater weight is to be attributed to physical *signs* or to *symptoms* in forming a judgment as to the existence of early phthisis, is discussed in the first chapter of the work. The author balances the evidence furnished by the short cough, the frothy, scanty sputum, the slight shortness and quickness of breath, the quickened pulse, the debility and languor, and the loss of weight, against that to be derived from the alteration of the percussion-note, the harsh or quasi-tubular, perhaps divided inspiration, with its dry crack and click under the clavicles, the coarse prolonged expiration, with or without its fine crumpling bruit, the defective elevation or expansion of the upper and front part of the thoracic cone, on one or both sides, the strong vocal resonance and fremitus, and the systolic bruit of the pulmonary and subclavian arteries, occurring together with healthy vesicular breathing heard over other parts of the chest, and a normal character of the cardiac sounds, and decides unhesitatingly in favour of the latter. Placing the two sets of phenomena thus in juxtaposition, we cordially agree with him; but the question immediately presents itself: Are there no cases of undoubted phthisis in which in the incipient stage, although marked by a general deterioration of health, one or more—nay, even the whole category of physical signs, have been unrecognisable? The affirmative is virtually ceded by the admission "that in a large number of cases of phthisis, at an early period of the first stage there is much reason to confess that physical signs are little developed. Numerous examples of phthisis at an early period of the first stage present themselves without our being able to detect physical evidence that can be held to be decisive." But it is precisely in these cases that correct diagnosis is most necessary, because treatment is then most effectual. The lung through which miliary tubercles are sparingly scattered will yield no information to the percussion-stroke; difference in the vesicular murmur heard over the healthy and unhealthy apices, may be no greater than that variation which is known to be consistent with a perfectly healthy state; the expiratory sound, in place of being prolonged, may (we state it on the authority of Skoda) be altogether inaudible, whilst the other signs enumerated may be but feebly pronounced or absolutely wanting; and yet the real nature of the case shall be indicated by some loss of strength and flesh, by slight cough and shortness of breath, by a quickened state of the circulation—in fact, by the usual symptoms of approaching decline. If in such an instance the history should show that the attack had not originated with ordinary catarrhal coryza, and if in addition the existence of hereditary predisposition be established, we believe that a diagnosis of incipient phthisis would be better founded than were it made in the case of an individual in ordinary health, in whom, nevertheless, the respiratory murmur failed to accord with a preconceived standard, and even other physical indications of an abnormal variation might be noticed. Neither should we distrust our opinion in the former case, although, in consequence of appropriate regimen and remedial measures, the patient progressed towards recovery, and the auscultator were denied the peculiar (but surely melancholy) "satisfaction" which Dr. Alison describes as being experienced when a doubtful diagnosis is confirmed by the supervention of the moist crepitation of the second stage. The truth is, that in actual practice, little real importance will attach to the question. The aid of the physician is invariably sought, because certain symptoms exist which are considered to be of moment by the patient or his friends. This class of indications, therefore, furnish the ground for further investigation, and are probably never dismissed from the mind in the subsequent examination of the case. As far as they may be present, both sets of phenomena are potentially within the ken of the practitioner; and in a condition confessedly so difficult to be discriminated as early phthisis, due importance must be allowed to the information which each is capable of affording, would he practise with advantage to his patients, or, we may add, to himself.

In the preface to the work two objects are proposed; one is to give a detailed account of the leading physical signs of the chest, &c., in pulmonary consumption; the other, to afford "some practical directions for the examination of the sufferer, and to offer a full account of the instruments employed in exploration, of the principles of their construction, and of their mode of operation." The prominence given by the author to the employment of certain mechanical aids to diagnosis not in general use, induces us to refer to the portion of the work dedicated to the latter object first, as by so doing we shall avoid the necessity of interpolating explanations in our notice of the former and more important section.

We pass over the practical directions for the examination of the chest, not because they possess but little value, but because the more important of them are to be found in most standard works on auscultation. We also shall not recapitulate the various instruments described and commented on with more or less of approval. Taken together, they would go far to constitute an *armamentarium medici* rivalling in proportions the chirurgical collections of the seventeenth century. Of the instruments contrived by the author, two appear to be of really practical value. One is the chest-goniometer for measuring the angles and estimating the curves or flatness of portions of thoracic surface; the other is the differential stethoscope, an invention which may prove a valuable auxiliary in the detection of differences in respiratory sound over different pulmonary areas. The mechanism of this instrument is partly borrowed from the double stethoscope invented by Dr. Camman of New York. The purposes which the two instruments are intended to fulfil are, however, entirely different. Dr. Camman's, like other bin-aural stethoscopes, is only meant to intensify the sound produced in one portion of the thorax by conveying it simultaneously to both the ears of the auscultator. By the differential stethoscope each ear is employed at the same moment in receiving sound from a different region. The latter consists simply of two hearing-tubes or stethoscopes, composed partly of metal, partly of flexible material, each provided at one end with a cup for the collection of sound; at the other, which is curved inwards to meet the ear, with a knob for introduction into the external meatus. The tubes are connected by a jointed bar, which is calculated to prevent the transmission of sound from one tube to the other, and by an india-rubber band, which by its elasticity obviates the necessity for using effort to keep the ear-knobs steadily applied. The instrument so constructed may be used in two different ways, and in each it is capable of affording important information. It may be employed for "the consecutive observation of the sounds of two parts of the chest by the two different ears." This is done by first applying one of the cups over the region to be examined, and, having completed the observation, by removing it and immediately applying the other cup over the area selected for comparison. Variations in force, duration, and quality of sound are thus easily estimated. The other mode of use is by the simultaneous application of the sound-receiving cups over the two areas chosen. We shall best make our readers acquainted with the very remarkable results obtained from this method of examination by transcribing the author's own words :

"The differential stethoscope proves of value not only in taking consecutive observations; it affords, as has been discovered in practice, without, if not contrary to expectation, most valuable information when observations are made upon two different parts of the thorax at the same moment. It has been proven, by this instrument, that slight differences in the intensity of the same sounds conveyed separately to the two ears produce remarkable and very striking results. The same sound conveyed to one ear a little *stronger*, and the same sound conveyed a little *weaker* to the other ear, is, or seems to be, heard through that ear only which has the major sound, and not at all through that ear having the minor sound. It is to be borne in mind that the conveyance of sounds to the two ears must be simultaneous. Not only does the sound appear to be heard in or through the ear favoured with the major sound, and not at all in or through the ear supplied with the minor sound, but the parts from which the sound proceeds, according as they are connected with the ear more or less favoured, are sounding or silent. To be more explicit: a body which sounds in one ear is rendered sensorially silent when the other ear is connected with that body in such a manner or in such a place as to receive rather more sound, the favoured ear seeming to be the only medium or organ through which the sound is perceived, and the place so connected with this ear being the only one which is sounding. Further, lest the idea which we desire to enforce should not be understood, let it be added, that a sound audible in or through one ear is rendered inaudible in or through it the moment the same sound is conveyed in greater force to the opposite ear; sound *quoad* the first ear being taken away, and transferred sensorially, so to speak, to the second." (p. 326.)

Facts illustrative of the principle here enunciated form a principal feature of the work. Some of them will be adduced as we proceed. The only practical objection we have seen to the employment of the differential stethoscope depends, not on any imperfection in the instrument itself, but on the frequency of sensorial defects in those who may use it. It is a well-known fact that in many individuals there is a natural, and in many auscultators, an acquired difference in the auditory power of the two ears. Some of the most skilful investigators are the subjects of this peculiarity. Before, therefore, the practitioner can venture to rely upon the more delicate indications derived from the use of Dr. Alison's instrument, it is absolutely necessary that he should assure himself by repeated comparative experiments that his sensibility to sound on the two sides is equally acute. As the ingenious instrument contrived by the author for measuring the thoracic movements in respiration by the rise and fall of water in a graduated tube (hydrostatic pneumatoscope) is not likely ever to come into extended use as a means of investigating the phthisical condition, we must pass over it with bare mention. We would only notice that the same instrument slightly modified (sphygmoscope) affords a delicate means of estimating the amount of cardiac impulse. By the ingenuity of Dr. Upham, who has connected with the sphygmoscope an electro-magnetic machine with a bell attached to it, that instrument has been converted into an acoustic appliance. An account of this latter invention is to be found in the brochure by the well-known M. Groux, on 'Fissura Sterni,' which, along with the instrument, was described in No. liii. of this Review, p. 276.

The physical signs of the first stage of phthisis which are enumerated under the head of auscultation are those dependent on alterations in the character of the vesicular and expiratory murmurs, different varieties of bronchial breathing termed by the author "bronchnœa sounds," and alterations in the thoracic voice sound. The first modification of the vesicular murmur is generally allowed to be the acquirement of harshness and increased loudness, which soon becomes accompanied by deficiency in duration. A change of an opposite character is, however, not infrequently observed, although Dr. Alison believes that it occurs at a later stage than harsh respiration sound, and that it depends on an increased deposit of tubercle, which annihilates vesicular movement. The inspiratory murmur has become more feeble, and but little audible. The author applies to this modification of the vesicular breath-sound Skoda's favourite epithet "indeterminate." The weak inspiratory murmur of the first stage of phthisis is liable to be confounded, on the one hand, with the scarcely audible respiration sound which is sometimes observed in individuals advanced in life, and in persons of calm temperament and slow respiration and circulation; and on the other hand, with the very deficient inspiration-sound of dry old cavities. In the first class of cases, Dr. Alison would rest his diagnosis on "the detection of an inspiratory bruit, normal in length, although ill-pronounced in pitch and intensity," and on the character of the respiratory sound "being general over both sides of the chest, which is not the case in the feeble respiration of phthisis." It must be admitted, however, that cases may occur in which these grounds of discrimination will prove insufficient; for the duration of the vesicular murmur is not only always diminished in advancing age, but it is liable to variations within the limits of health. Moreover, the character of the inspiratory sound is not always the same over the whole chest; in some instances, it is louder on one side over the supra- and infra- clavicular regions than on the other, and the normal murmur is readily supposed defective when it is compared with a louder or harsher breath-sound heard over another area. It is precisely in such cases that we should not hesitate to prefer the general symptoms present as the basis of diagnosis, to the necessarily uncertain revelations of the stethoscope. The mistake of confounding the feeble respiration of early phthisis with the very deficient sound of dry old caverns, Dr. Alison believes to be a common error. In such cases, the differential stethoscope will generally furnish correct information.

"The respiration of many cavities which are dry, of no great size, which have free bronchial communication, both intra and extra, with little or no mucus at the openings of the bronchi to produce bubbling or clicking, free from distinct cavernous pectoriloquy, approaches the character of feeble vesicular respiration. The approach to feeble, ill-formed vesicular respiration sound is such, that it may be, and often is confounded even by practical stethoscopists with the feeble condition of breathing in the first stage. The differential stethoscope under these circumstances proves of some value. Both sound-collectors being applied, one on the sound side, and the other on the feeble side, the result is such as to convince the hearer that very different conditions of lung are present at the two parts. If the feeble inspiration sound be proceeding from a dry cavity, the contrast is *very great*, when the two parts are listened to in succession; such as is scarcely ever obtained in the case of healthy lung, contrasted with phthisical lung in its first stage of degeneration. If the two cup collectors be simultaneously employed, the ear which is connected with the sound lung is the only one which, to the mind, seems to receive any sonorous impression, which is hardly the case when the two ears are listening, one to a healthy lung, and the other to a lung the seat of tubercle in its crude or nascent state, and while yet in very moderate amount. The absence of hearing through the ear connected with the diseased side always indicates some very considerable amount of deposit, or some form of very serious or advanced disease. When silence is not due to a large amount of crude tubercle, it is generally an old dry vomica that is present, but total hepatization will produce the same result, but this hepatization of the apex is extremely rare." (p. 12.)

The modifications of the vesicular murmur known as wavy and divided respiratory sounds are regarded, and we think justly, by Dr. Alison as corroborative evidence of the existence of tubercle, but as by no means pathognomonic of morbid deposition. The wavy character of the inspiratory murmur is, according to the author's experience, developed after the acquirement of harshness and loudness. It is frequently found over an apex in which tubercle exists in no very considerable quantity and in a dry state. Dr. Alison explains the mechanism of its production by attributing it " to the presence of an obstructing force to the suction of air into the vesicles, consisting in the loss of the dilatability of the lung-structure to a degree sufficient to produce a *retarding* effect without even for an instant absolutely stopping the movement of the air." When, however, the amount of obstruction, instead of merely retarding the movement, is considerable enough absolutely to arrest it, until the obstacle be overcome by the increasing and accumulating force of the inspiratory effort, divided inspiratory murmur is the result. This form of respiration is therefore, when corroborated by the general symptoms and other physical signs, to be held as evidence of a larger amount of deposition than the mere wavy variety. Taken alone, however, it has no such signification, for it may result from any cause which is capable of producing a thickened condition of the coats of the ultimate tubules and a tendency to adhesion of their internal surfaces.

Alteration in the expiratory murmur is to be regarded as a very general as well as an early sign of incipient phthisis. The author believes that whenever tubercle is present in sufficient quantity to sensibly increase the weight of a portion of lung, to diminish its contractile power, and to exert pressure on a few of the tubules of the pulmonary lobules, modification of the expiratory sound will accompany it. The first alteration the expiratory bruit undergoes is an increase in its loudness and elevation. The sound thus intensified also becomes coarser, and soon acquires a prolonged duration, extending in some instances to double the length of inspiration. The mechanism of the production of the coarse and prolonged expiratory sound in phthisis is essentially the same as that which renders the inspiratory murmur harsh and wavy. The pressure of abnormally solid material offers unusual resistance to the expiratory forces which in ordinary respiration are not sufficiently powerful immediately to overcome mechanical impediments.

It is in detecting the alterations in the respiration of early phthisis that the differential stethoscope has, in the inventor's hands, proved of such striking advantage. The phenomena elicited by the simultaneous employment of the two sound collectors are at least exceedingly remarkable. If we suppose a case in which the lung of the right side only is tuberculated, we may have both in inspiration and expiration the sounds on the left side completely eclipsed, or, so to speak, sensorially destroyed. No sound appears to be emitted by the healthy lung; nothing is heard but the harsh loud vesicular murmur and the coarse prolonged expiratory sound of the diseased side. Or, in another instance, where the presence of tubercle is evidenced by weak inspiratory sound and prolonged loud expiratory bruit, and where the healthy lung has taken on increased compensatory action, we shall have inspiration sound only on one, the healthy side, and expiration only on the other, the diseased side. In this way "a sensorial oscillation of the respiratory sounds" from one side of the chest to the other is established. Inspiratory bruit is heard with one ear, expiratory with the other; as one ear receives sound from the healthy, the other from the unhealthy lung. This curious phenomenon has been observed by the author in many hundred cases of phthisis, but as far as he remembers, in no other form of disease.

But an element of deception, to which we have already alluded, is found in the fact that in a certain proportion of persons the vesicular murmur is louder, fuller, and more protracted on one side of the chest than on the other. The following is the result of Dr. Alison's observations upon this very important point:

"In examining about a hundred boys and girls, and young females in health, I have found an excess of loudness of vesicular sound on the left side of the chest in about ten. Nearly the whole of the remaining ninety have had equal respiration on both sides. It is not easy to assign a reason for this excess of loudness on the left side. However, I am disposed to believe it to be dependent upon the greater degree of angularity and curving of the bronchial tubes at the left apex, and to some impediment offered by the presence of the heart on that side." (p. 8.)

And again, at p. 28, he states that for years he has met with this disparity—the vesicular sound being not only fuller and louder, but more protracted on the left side. This conclusion, it will be noticed, does not accord with that arrived at by Louis and Walshe. Louis found, in twenty-two healthy individuals examined by him, that in two only was there difference in the loudness of the inspiratory murmur; in one of these the excess was on the right side, in the other, on the left. Walshe states that no difference is, as a rule, to be detected in the sound of inspiration on the two sides. Both these accomplished observers are agreed that the sound of expiration is more constantly to be heard on the right side than on the left. In Louis's twenty-two cases the expiratory murmur was nearly inaudible under the left clavicle in thirteen; under the right it was inappreciable in five cases only. It is curious, also, that the physician (Dr. Gerhard) who first called attention to the existence of a difference, assigns to the right side a more "blowing" respiration than to the left. We regret that Dr. Alison has not given any explicit information as to the differences in *expiratory* murmur which he may have noticed on the two sides.

Our space will not allow us to follow the author through his admirable chapters on the bronchial breathing, sonorous and sibilant rhonchi, and alterations in the thoracic voice. We can only notice a few of the more prominent points in the chapter which treats of the evidence to be derived from percussion in early phthisis. Dr. Alison agrees with most other practical writers in asserting that no revelations are to be procured by percussion at a period when auscultation is incapable of affording striking and reliable information. It is consequently in the very early stages of no value. He asserts from numerous observations which he has made upon tubercular deposit and its associated percussion sounds, that "it is not until the air has been reduced one-third below its normal amount, that the alteration of the percussion sound in the direction of dulness attains to that degree that may with safety be relied upon." He is of opinion, however, that dulness is by no means the first alteration in the percussion sound which may be detected. The first change which he detects he describes as "a reduction of the air element" in the percussion sound. "The sound emitted is less loud, less continued, less diffused, less vibratory, and it seems to proceed from a body or series of bodies having a smaller extent of area in vibration than is natural." We are disposed to regard this as an example of the author's tendency to over-refinement; for in the next page he tells us that in the perfect manifestation of all these phenomena we have what is called dulness. We therefore are at a loss to understand the distinction between "the reduction of the air element in the percussion sound," and the first recognisable degree of dulness. At least, it appears to us a distinction without a difference. We are, however, quite ready to admit that advantage may accrue from a separate recognition, if it be possible, of the elements which together constitute dulness. But we are inclined to believe that few adepts in the art of percussion are able to draw a distinction between a sound which is shorter and one which is less vibratory, between one that is less loud and one that is less diffused, when at the same time the whole amount of departure from the healthy note is not sufficient to be characterized as appreciable dulness. Dr. Alison insists, and we think with justice, on the greater value of the evidence furnished by auscultation, than of that which percussion alone can afford.

"I have in not a few cases of tubercle present in a crude state, and also in a softened state, found the percussion sound to have more air character than in some persons in perfect health. I am also convinced that I have occasionally found the percussion sound of even the third stage of phthisis to be more of an air sound than that of some few persons in health, and when pneumothorax was not present to explain the occurrence." (p. 77.)

Considerable prominence is given by the author to the alterations in the form of the chest which occur in the progress of phthisis. He finds that in one-half of the patients who are the subjects of the first stage, an appreciable inclination forwards of the thorax and shoulders is to be observed. A progressively increasing curve of the dorsal vertebræ accompanies its progress. Double flattening of the anterior surface would appear to be peculiarly the deviation of the first stage. Of sixty-six examples occurring in phthisis, forty-nine were observed in the first stage, twelve in the second, and five in the third. The comparative rarity of this alteration in the latter stages is due, not to any rising again of the sternum or rounding of the costal cartilages, but to the more rapid progress of the disease on one side, whereby single flattening and undue angularity of the articulations of the sternum and cartilages, and ribs and cartilages, are produced, and replace, so to speak, the general flattening of the earlier phase. Of more importance, as a diagnostic sign, is the flattening of the anterior wall of the thorax on one side only.

"We find that the single flattened chest is generally the accompaniment of serious organic pulmonary disease. A few examples of this alteration of the chest are found without any organic disease, and are dependent upon congenital conformation, or more frequently upon the influence of occupation in trade, or upon lateral curvature of the spine; but the proportion which these cases bear to examples of well-defined flattening on one side dependent upon phthisis is small, and they admit of very ready reference to their proper category. The single flat chest is more rare in the first stage of phthisis than the double flattened chest. While six examples of double flattening were seen in twenty cases of incipient phthisis, only three, or just one half, of single flattening examples were found in the same number of phthisical cases in the first stage. Obvious single flattening is not restricted by any means to the first stage of phthisis, although it is common to it, for it is more frequently met with in the third stage than in the first. Thus I ascertained the presence of this alteration of form was found twenty times in the third, and only thirteen times in the first stage out of forty examples occurring at all periods of phthisis. In the second stage this deviation was found seven times only out of these forty examples." (p. 93.)

Of 75 cases of single flattening observed by the author, 66 were cases of phthisis, 3 of doubtful phthisis, 2 of bronchial affections, 1 of dyspnœa, 2 of heart disease, and 1 of cancer of the lung.

Our limits are so nearly exhausted that we can do little more than cordially recommend the study of the work. There are one or two points, however, to which we would direct the reader's attention. Dr. Alison treats at some length of the alteration of the area of the cardiac sounds in the third stage. When excavation in the upper part of the lung has taken place, and we listen over the second interspace, the heart's sounds will strike the ear with a loudness and nearness as though the organ, uncovered by lung, lay directly under the parietes. The cardiac sounds are, under such circumstances, frequently more loudly heard over the second than over the fifth interspace, and especially, although not exclusively, is this the case when the excavation occurs on the left side. Inspection will also frequently show a waving impulse over the first and second intercostal spaces. The observation of this sign has not infrequently enabled the author to diagnose the existence of cavity.

One of the original features in the work is the elevation of what has been hitherto considered as a mere accidental complication—perforation of the pleura—into the rank of a *fourth stage.* The author's reasons for this innovation are, that perforation is the natural result of the progress of excavation, that from the results of his experience he finds it take place in a large number of cases before death, and that it is probable that in the majority of cases it is overlooked in consequence of its occurrence so near the time of dissolution of the patient, when examination is considered uncalled for. Whether these be sufficient grounds for the introduction of another artificial division in a history whose gradational progress has been already so variously disjoined, we must leave our readers to judge.

Dr. Alison's account of the mechanism of metallic tinkling strikes us as being singularly happy. With other writers he believes that the phenomenon is produced by the explosion of an air bubble, by which the air in the pleural cavity is thrown into vibrations and communicates them to the hard exterior thoracic walls. He differs, however, from other writers as to the more frequent seat of the bubble bursting, which he places in the abnormal aperture in the lung and pleura, instead of on the surface of the fluid at the bottom of the pleural cavity. He has proved by the examination after death of a patient in whom the sign was established, that it is not necessary for its occurrence that there should be fluid in the pleural cavity; and he further argues, that at the mouth of the fistulous opening through which air is drawn at each inspiration, and comes into contact with more or less viscid liquid, we have exactly the conditions essential to the formation of bubbles.

In terminating this brief notice of Dr. Alison's work, we would emphatically express our opinion that it is a book full of the results of diligent, honest, clinical study; that it is eminently the fruit of thought as well as of observation, and that although all may not be disposed to place the extreme degree of confidence evinced by the author in the art of physical diagnosis, few will rise from the attentive perusal of his treatise without being better qualified to test in practice the conclusions which he enunciates.

REVIEW XI.

On the Signs and Diseases of Pregnancy. By T. H. TANNER, M.D., Assistant-Physician for Diseases of Women and Children to King's College Hospital.—*London.* pp. 504.

DURING the several years in which a physician has but little occupation except what he imposes upon himself, Dr. Tanner has evidently read what others had done before him, studied the literature of his profession, and become a careful observer and chronicler of facts which he had himself an opportunity of witnessing. No one can fail, by perseverance in such a course, to contribute to the advancement of medical science. The work now before us, 'On the Signs and Diseases of Pregnancy,' proves industry, freedom of expression, clearness of arrangement, and at least a good theoretical knowledge, on the part of the author; it forms a good text-book, in short, on the subject of which it treats. As a scientific work, however, we cannot speak of it with unqualified praise, there being but little in it that can be said to be original, and a good deal in reference to the "great ones of the past" that might better have been omitted. The first chapter will supply many instances of what we mean, showing that although "they can now only speak to us through their writings," it would be quite as well did they not speak to us at all. No one doubts the "great inventive powers of authors," but to mix up fiction and the marvellous with what is intended to be really practical, seems to us of very doubtful utility. Leaving these fancies of a bygone age, we will make a few remarks on the rest of the work, and with a desire to do justice to its author. The book consists of twelve chapters, divided as follows: 1. General Observations on the State of Pregnancy. 2. The Signs and Symptoms of Pregnancy. 3. The Diseases which Simulate Pregnancy. 4. The Duration of Pregnancy. 5. The Premature Expulsion of the Fœtus. 6. The Examination of Substances expelled from the Uterus, &c. 7. Extra-Uterine Gestation. 8. Superfœtation—Missed Labour. 9. The Diseases which may Co-exist with Pregnancy, and their Reciprocal Influence. 10. The Sympathetic Disorders of Pregnancy. 11. The Diseases of the Urinary and Generative Organs. 12. The Displacements of the Gravid Uterus. We may observe once for all, that the subjects discussed in these several divisions are treated with perspicuity, and in a comprehensive, intelligible manner, so as to afford a very favourable opportunity to students to make themselves acquainted with them historically as well as practically; and although it cannot be said they contain much of novelty to those who have had experience, we doubt not that the book will be referred to with interest by many who may wish to refresh their memory or test their own views of practice in the important branch of the profession of which it treats.

It is well known to us all that difficulties often arise in determining the existence or non-existence of pregnancy; we every now and then meet with cases of such a doubtful nature that nothing but the greatest tact and vigilance can protect us from error; and the consequences of making mistakes may be disastrous to our own reputation, and entail upon others a vast amount of misery and wretchedness. Dr. Tanner has therefore very carefully arranged and described the signs and symptoms of pregnancy, and the various diseases which simulate it, or which may be mistaken for it. He particularly insists upon the value of auscultation, alleging the fourth month as the period at which the uterine souffle can be heard, and the fifth as that in which we may discover the sounds of the fœtal heart. We believe these signs to be invaluable, and well worthy of careful study; but it has several times happened in our experience that when we have distinctly heard the fœtal heart, the capacity for hearing it by others present has been wanting; and we doubt whether, as a general rule, it can be distinguished by many with certainty as early as the beginning of the fifth month. As a sign to be relied upon at an earlier period of pregnancy, our author insists very much upon the presence of mammary secretion; and lays it down as a law, that when a woman is gravid for the first time, and has missed two monthly periods, a drop or more of fluid may be expressed from the breasts, which, on minute examination, will be found to present all the characters of true milk; while, from the presence of even this small quantity, in the great majority of cases, we may alone successfully predicate the existence of pregnancy.

With regard to the necessity on the part of the practitioner for always exercising great caution and circumspection in the diagnosis of the diseases which simulate pregnancy, we cannot do better than quote our author's own words.

"In some cases the most experienced physician will have misgivings; hence, none need be so foolhardy as to descend to guesswork. He, however, who really desires to prove accurate will generally be so; since he will take care not to come to a positive conclusion without a careful review and calm consideration of all the circumstances of the case. He will weigh well all the data on which reliance can be placed; for, otherwise, though his judgment may be founded on what is apparently strong evidence, yet, from not considering, or from being ignorant of, one or two small facts, the deduction is not unlikely to be wrong."

In the chapter on Premature Expulsion of the Fœtus, some remarks are made on what is termed, in medico-legal phraseology, "live-birth." In contested lawsuits relating to the inheritance of, or succession to, property, and on some other occasions, it is sometimes a matter of considerable importance to determine whether a child was born alive, even though it may have perished shortly afterwards; and this raises the question as towhat constitutes a live birth. Formerly, we believe, the act of *respiration* or crying was necessary as evidence of a child being born alive; but, owing to some recent cases in civil jurisprudence, it seems that the mere muscular movements of the limbs or the features, independent of any signs of respiration, are sufficient evidence, and held now to be ample enough proof, of "live-birth." The latest decision which has been given in such a case is in that of Brock *v.* Killock, before Vice-Chancellor Sir J. Stuart, on

April 29th, of the present year. The question raised was whether a child was born alive or not; and turned on the evidence of Dr. Freeman, the medical man who delivered the child's mother, and that of the other medical men who had given opinions *pro* and *con* upon Dr. Freeman's statements. The case is reported in 'The Times' of May 1st, 1861. Four of the medical men swore to the *absence of respiration.* Dr. Freeman said the child was born alive, in consequence of the pulsation of the umbilical cord. The Vice-Chancellor considered the proof of the performance of *one clear vital function* was enough to show the existence of life, and he thought that pulsation was enough. Dr. Tyler Smith, in his affidavit, said "it would be much better to take as a proof of live-birth a single vital act, such as the beating of the heart after separation from the mother, which can hardly be mistaken, than an undefinable amount of respiratory action, which must always be doubtful and difficult of proof in cases where the child dies shortly after delivery." The Vice-Chancellor said that, upon the whole, he was of opinion that it had been satisfactorily proved that the child had been born alive.

One of the most fatal diseases of fœtal life, without any doubt, is syphilis; and it is scarcely possible to imagine any more important subject for strict investigation and study than the transmission of the syphilitic poison by parents to their offspring. It is the greatest scourge, we hesitate not to say, that is inflicted upon mankind; the greatest evil that exists in the world; the chief source of nine-tenths of the illness and misery that flesh is heir to; a true exemplification of the certainty of ample punishment for sin, even in this present world. We have neither time nor space to dilate upon this melancholy statement, although we would fain believe that much of the evil we have spoken of would be prevented, were the circumstances which lead to it fully known and appreciated by the public; that many, indeed, were they aware of the possibility of their transmitting syphilis to their offspring, would rather lose their own lives than contract marriages likely to be followed by such frightful consequences. We have been induced to make these few observations, not only for the purpose of leading the mind to the contemplation of the possibility and frequency of the evils spoken of, but also of inserting our author's description of the chief morbid appearances found after birth in syphilitic fœtal diseases—viz.,

> "A copper-coloured cutaneous eruption, abscesses in the lungs, or indurations of portions of the pulmonary tissue; diffused or circumscribed suppuration of the thymus gland; the infiltration of an albuminous fluid into the parenchyma of the liver, disconnecting and deranging its proper secreting structure; and lastly, the presence of numerous pemphigoid vesicles, having a blue or violet tinge, and being seated on the palmar or plantar regions, is stated to be a veritable indication of syphilis, though one of its most rare phenomena."

We must now conclude our brief notice of Dr. Tanner's book, by recommending it as a useful practical work. We have read it, upon the whole, with satisfaction, and consider it a good epitome of the signs and diseases of pregnancy.

Review XII.

Norsk Magazin for Lægevidenskaben. Udgivet af det Medicinske Selskab i Christiania. Anden Række. Redigeret af A. W. Münster, Voss, Faye, W. Boeck, Lund. Bind XIII., XIV., & XV. Hefte 12.—*Christiania*, 1859–60–61.

Norwegian Magazine for Medical Literature. Published by the Medical Society in Christiania. Second Series. Edited by A. W. Munster, Voss, Faye, W. Boeck, Lund. Vols. XIII., XIV., and XV., comprising twelve numbers. — *Christiania*, 1859–60–61.

Norwegian medical literature, and the proceedings of the profession in that remote corner of Europe, being but imperfectly known to practitioners throughout Great Britain, we are induced to notice briefly the publication whose title heads this article, especially as the work possesses considerable reputation in the country where it appears, and further, really deserves notice from foreigners.

Like similar periodicals, in England as elsewhere, for communicating knowledge, the one under review contains, first, original communications, some being both interesting and valuable; next, reviews, reports, and extracts from other journals; afterwards, notices concerning scientific medicine, the treatment of diseases as they occur in Norway, with the effects of remedies; and ultimately, medical news, or items of general information.

Among the original communications in this Norwegian periodical, an interesting account of a journey recently made in the United States of North America, by Professor Voss, may be mentioned, although perhaps esteemed of greater value in the opinion of the writer's countrymen, than it would prove to Englishmen, on account of the information thus imparted respecting a Republic and its medical institutions, with which the latter are much better acquainted. As statements of an entirely local character, however, possess more value among British readers, we now subjoin a brief statistical report of the diseases which came under treatment in the hospital during March, 1859, seeing that such account indicates the type of maladies generally prevalent at that season. For example, 53 cases of pneumonia were there admitted, of whom 5 died; influenza comprised 227 patients, 4 ending fatally; diarrhœa and cholorine 101, but no deaths; typhus 19, among whom 1 died; croup 5, death following in 3; puerperal fever 3, whereof 1 died; and lastly, 18 cases of scarlatina, 8 of diphtheria, 7 of varicella, and 14 of pertussis, all of which 47 inmates, however, ultimately recovered.

In subsequent numbers, Professor Boeck contributes elaborate observations on the question of Syphilization, which inquiry thus seems to occupy as much attention among Norwegian surgeons, as it does at present both in France and England. Professor Faye likewise publishes an extended and valuable paper on obstetric pathology, showing that department of medical practice is also there zealously cultivated. Subsequently, Dr. Holst communicates a minute report of the different skin diseases treated during two years ending in June, 1858, at the national hospital for these complaints, whereby it appears that syphilis and gonorrhœa were common affections. Thus, among 484 male patients, 149 laboured under the former malady, while 219 out of 410 female patients were similarly affected. On the other hand, 117 males had gonorrhœa, but not one woman is stated to have been admitted under that category, although it ought to be added 41 females had leucorrhœa, and 69 urethritis; no males are classed in the latter column. These facts may perhaps explain the above anomaly, as also that 77 men were enumerated as suffering from "epididymitis." Finally, scabies comprised 88 cases, of whom 60 were male and 28 female patients. Various other original articles of interest might be likewise alluded to on the present occasion, which have appeared in the periodical now designated; we can only now add that, for those who may be able to read the Danish language, the 'Norwegian Magazin' will amply repay perusal, as well on account of the matter it contains, as from being a good exponent of medical literature, and of the professional or scientific questions which now occupy our brother practitioners in that part of Europe.

One of the features characterizing the publication which has been thus briefly brought under notice must not be overlooked—namely, the reports therein given of the proceedings of the Christiania Medical Society, as also the remarks made during its discussions by various members—showing that here, as elsewhere, there exists a friendly interchange of opinions and experience among the profession of this northern metropolis, alike beneficial to each associate as to the community at large.

This Society meets fortnightly, and in most respects resembles similar Associations for the promotion of knowledge elsewhere established; while those foreigners who desire to become more intimately acquainted with what their medical associates are doing in the far North, may peruse advantageously that portion of the journal now indicated. Had present limits permitted, we should have certainly made some more special allusions to this department, but to do so now being beyond the narrow scope originally proposed, when the task was undertaken to bring briefly before English readers the subject of Norwegian medicine, it only remains to refer any student who feels anxious to obtain further information respecting such matters to the work itself, which deserves examination—supposing, of course, that inquirers understand the Norsk language.

Believing a few cursory remarks respecting the present condition of medicine in Scandinavia may be interesting to most readers, especially to those who perhaps have not paid much attention to occurrences among their professional friends in so northern a portion of the Continent, we may mention that in Christiania there is an university of some repute, having usually about eight hundred students, and comprising a medical faculty, with professors whose fame is not confined to their own country. It possesses an admirable library of nearly

200,000 volumes, and has museums of various descriptions, where one of the professors often attends, particularly on public days, to explain the several collections to all persons anxious for information regarding the different objects there exhibited. This arrangement proves highly conducive to the diffusion of knowledge, and ought, therefore, to be adopted at every similar institution, wherever located. Of late years, several works of importance on medical subjects have made their appearance in Norway, which are creditable to native authors. Thus may be specified, as examples, the recent work of Dr. Danielssen, Chief Physician to the Leper Hospital at Bergen, "On Diseases of the Skin," illustrated by splendid plates; as likewise the joint production of Drs. Danielssen and Boeck, On "Spedalsked"—Elephantiasis Græcorum, or Leprosy; while the malady is accurately displayed in drawings highly creditable both to artists and publishers. Other Norwegian medical publications might be alluded to if necessary, although the above seem sufficient to show that our northern *confrères* are active in promoting the practical study of medicine and the collateral sciences.

Analogous opinions may be enunciated with equal justice regarding a neighbouring kingdom, to which Norway was long attached under the same sovereign, and whose language the Norwegian educated classes still speak—namely, Denmark. In this island country, both medicine and surgery are zealously cultivated by able labourers in the field of science; while numerous works have issued from its press, of which other nations would not be ashamed. Further, the University of Copenhagen is considered one of the best throughout Northern Europe, and usually comprises about twelve hundred students in the four faculties. It has an excellent library, containing upwards of one hundred thousand volumes; and there may be seen besides, in the Danish metropolis, numerous magnificent museums well adapted for the cultivation of science, which would do honour to any capital whatever. Indeed, the richness of Copenhagen in many varied collections intended for advancing knowledge in nearly every department, fully entitles this city to the designation it has popularly received, of being the "Athens" of Northern Europe. Among its different museums, the Zootomical, in the University, may be specified, as being the richest of that description throughout the Continent. The Royal Zoological Museum—occupying one of the King's palaces, forms also a splendid collection; and as periodical instructive demonstrations are frequently given by curators, to all spectators who may choose to listen, as also by professors in respective departments, the valuable objects there accumulated become utilized in a way highly commendable; this plan might be often advantageously imitated in other countries, whose inhabitants sometimes vainly deem themselves superior in some features to the more modest Danes, with whom they are too little acquainted, particularly in reference to their educational and scientific establishments.

Prior to taking leave of Scandinavia, and the actual state of medicine in that distant part of Europe, which at one time, however, formed an united empire, Stockholm, now the capital of one of its integral kingdoms, and usually designated the Northern Paris, must not be omitted from our present brief sketch, more especially as the subject is truly deserving of notice by the profession in England. Unlike almost every other European metropolis, the city just named has no university: Upsala being still the chief Alma Mater in North Sweden. However, in lieu of such an institution, with reference to medical studies, there is the Carolinish Institute, where ample appliances exist for obtaining professional knowledge, quite equal to any university. This establishment has an extensive library, numerous teachers in the usual branches required by students, and several museums containing whatever may be wanted for their instruction, as also a maternity institution and large hospital adjacent, named the "Seraphim," where varied clinical teaching constitute prominent features. It should, however, be added, that to obtain medical degrees, students must still resort to Upsala, although great efforts are now being made to transfer that privilege to the metropolitan institution. Nevertheless here, as elsewhere, such an innovation is resisted by those in possession of ancient rights, against the attempts of other parties equally desirous of wresting the power of making M.D.'s from the hands of existing monopolists.

In no part of Europe does greater anxiety prevail to become acquainted with what may be passing in other countries, whether as regards science or medicine, than among professional men in Sweden, who, during late years, have not only often published important works in reference to medicine and on surgical questions, but also in reference to the collateral branches of knowledge. Moreover, Swedish practitioners are generally well-informed respecting foreign medical literature, of which two instructive examples, derived from our personal observation when recently visiting Stockholm, will suffice as apt illustrations. At the Carolinish Institution already mentioned, besides French, German, and the standard publications of other nations, there were noticed on its reading-room tables, not only indigenous periodicals, but those issued from various foreign capitals, including Dublin, Edinburgh, and London, among which we may enumerate This Journal, whose pages must have been often perused, if one might judge from appearances. Again, at the chief medical society somewhat analogous to the Imperial Academy of Paris, in addition to many continental journals on professional subjects, the members also frequently received important works published in Great Britain; this is very different to what usually prevails in France, as for instance, at the celebrated French medical corporation just mentioned. At that learned establishment, enjoying a high reputation everywhere, when recently visiting, we could not find any British medical periodical on its library table, excepting the London weekly return of births and deaths, forwarded gratuitously by the English Registrar-General. Such statements speak unmistakeably regarding the marked existing difference betwixt our Gallic and Scandinavian neighbours. This discrepancy may, however, be explained by the advanced position which France has virtually attained in the comparative scale of science and learning, if contrasted with residents in the more boreal regions, who feel ever anxious to obtain knowledge from any source, whether foreign or domestic. The former are, to quote the saying of an ill-natured critical foreigner, oftener teachers than scholars of other nations, according to their own estimation; and usually believe themselves best entitled to lead the van, instead of following in the wake of modern civilization.

Review XIII.

A System of Surgery, Theoretical and Practical; in Treatises by various Authors. Edited by T. Holmes, M.A. Cantab., Assistant-Surgeon to the Hospital for Sick Children. In Four Volumes. Vol. I., pp. 850.—*London*, 1860.

"A tendency to greater exactness of thought in questions of cause and effect," it is remarked by one of the writers in the volume before us, "has been part of the general scientific development of our time —belonging not exclusively to medicine, but perhaps rather coming to medicine by contagion from physical and chemical studies. Fortunately, however, it has come to us." The appearance of a new work, long wanted, which is to comprise the whole range of surgical practice as understood in the present day in England, naturally leads us to inquire how it happens that such a work is new, and how it is that the want of it is only now to be supplied. It had long been felt that however high the standard of surgery had been raised in this country, it was not to the credit of the professors and practitioners of the science, that there was no modern work sufficiently comprehensive in its pages to compare with the productions of other countries, which at once gave a view of the whole range of the science and practice of surgery, and was at the same time the production of individual research and experience, rather than a compilation from the labours of other men. Contented to take their stand on ground they knew they could hold, it seemed as if the leaders and the teachers in the English schools of surgery were to be known by their well-earned title to safe and successful practical knowledge—the pride, as it is thought, of the practical Englishman; rather than by any claims they could put forward to higher honours gained by toil in the more laborious fields of original investigation and research. At all events, there was no single work, as a written record of the results of such labours, by which their high claim to pre-eminence could be tested, or from which it could be proved. Where the possession of such a title was not known, it could safely be called in question; and the high claims of the English schools could be challenged by others who, perhaps, looked around them from a lower standing on the slopes of the surgical Olympus.

The projectors of the work, of which one volume is now before the profession, have undertaken to supply the acknowledged deficiency; and for this purpose they have formed into a "system" a collection of essays or treatises by different writers on the numerous subjects comprised in the general term of "Surgery." The difficulty of obtaining qualified contributors to unite in the general plan of a work which should be a complete expression of the opinions and experience of many men; and, still more, to act in concert in the execution of it, and to perform with punctuality their engagements with their common editor and the professional public, has been one chief obstacle in the way of the completion of former undertakings of a similar comprehensive nature, though the hope of it has often been conceived by active minds.

The essays in the present volume, which are professedly limited to subjects of general pathology, are for the most part introductory to the fuller treatises which are to appear in subsequent volumes.

The opening essay is by Mr. Simon, who takes for his subject the pathology and treatment of *Inflammation* generally; a subject, he says, "of large scope and of equally large importance; the study of which seems to branch almost throughout the whole subject matter of surgery:" and he adds, "assuredly it is no exaggeration to say that rational surgery depends more upon a knowledge of the inflammatory process, than upon all other pathological knowledge put together." The inflammatory process, in its most characteristic type, is illustrated by the history of the rise of a carbuncle, and its progress, through the separation of the slough, and the healing, with the formation of a sound cicatrix. Mr. Simon, we see, calls it a slough, and minutely describes the microscopic characters which entitle him to declare it to be so. Hyperæmia invariably attends inflammation, and is essential to the inflammatory process, whether that be a destructive or a productive process; and it is seen in all forms of increased nutritive activity. It can go on for months without producing textural change, without causing a single pus-cell to grow, or a single texture-germ to die. A part is not inflamed because it contains more blood; it receives more blood because it is inflamed.

Our knowledge, such as it is, of the state of the capillary circulation in inflamed parts, is summed up by Mr. Simon in these conclusions:

"That within the area of stasis, the blood has lost the fluid in which its corpuscles should float; that the circulation of the corpuscles is delayed; that they collect against the wall of the capillary, as though by mutual cohesiveness, though they do not, when removed, seem more cohesive than in blood of healthy parts; that there is increased infiltration of the contiguous textures, and that stasis occurs with more or less facility in proportion as the liquor sanguinis is more or less transudable, and it does not result from alteration in calibre in the vessels of the part; that the cause of its production is an influence excited on the blood by textures within the area of stasis; that this influence is, mechanically speaking, of a suctional kind, which differs but in degree from that which the textures naturally exercise on the blood as it passes amid them."

The symptoms and causes of inflammation, as taught and described by former writers, much of whose doctrines may still be accepted as true, are traced with the closeness of investigation and clearness and comprehensiveness of arrangement which have distinguished all Mr.

Simon's former writings. The phenomena of inflammation are modified phenomena of textural life; the power which produces them is the power which produces the ordinary phenomena of textural life, and fully to explain inflammation would, in fact, be to explain life. Mr. Simon closes a valuable and instructive essay with a brief history of the literature of the subject; "the special study of which," he writes, "without undue partiality, an Englishman may be glad to say dates from the labours of John Hunter."

Mr. Coote contributes two short essays—one of them on *Abscess.* He gives in clear language, and in very few sentences, the most approved modern doctrine of the origin of pus: in behalf of which it must be said, as of much other learning that we labour to accumulate, that it seems a weak part of the doctrine that a writer is forced to confess that some of what is most necessary to the support of it "has yet to be proved." The possibility of the absorption of pus, though now known to be of frequent occurrence, rather than an occasional and very rare exception to a pathological law, is not yet universally admitted, if we may judge by the practice, still followed by some surgeons, of incising every fluctuating swelling. Where there is room for a doubt, there will generally be found some ground to support it; and of a soft and fluctuating swelling, which has been allowed to disappear without active surgical interference, it will many times be a question after all, which can never be solved, whether it was indeed pus.

The rules laid down for opening abscesses are liable, we think, to some difference in opinion. The bleeding lancet, if made, as lancets are now usually made for the pocket-case, sufficiently strong in the blade not to bend, and strong enough in the point not to break, we have generally found the most satisfactory instrument for opening the abscesses under the common integument; and these are the abscesses for which an opening is most frequently needed, or, perhaps we should say, is most frequently made. It is a less formidable instrument to produce in the sick-room, and we think it may be commended as doing the work sufficiently well. For chronic suppuration, for the abscess under the deep fascia, "a thin, yet broad-shouldered, sharp-cutting, double-edged knife, or scalpel," recommended by Mr. Coote, will be found more serviceable—greatly superior, according to our observation, to the curved cutting instrument, the bistoury, or the old-fashioned "abscess lancet." The rapid division of inflamed tissues, such as those generally lying over an abscess, of the integument especially, is attended with severe pain; and Mr. Coote's "rapid and bold plunge" of the instrument will often be found to give unnecessary pain in the *beginning* of an operation, at the end of which he justly condemns all probing and squeezing, as "a proceeding extremely painful" to the patient. The poultice, after the first application, may well be dispensed with; for it rather tends to keep up the suppuration, and by weakening the vessels, so far interferes with the recommendation to "leave the remainder of the cure to Nature." In connexion with several cases well related, Mr. Coote describes the different regions of the body in which abscesses are found sufficiently often to become subjects of surgical interference, or, when of rare occurrence, to give rise to difficulty in diagnosis.

Mr. Paget is one of the largest contributors to the present volume; four separate articles are from his pen. After describing the structure and anatomical characters of *Fistula* and *Sinuses*, as commonly found in practice, Mr. Paget proceeds to consider their different modes of formation, with their general pathology. The many different methods of treatment, constitutional, local, and operative, are passed in review, with concise and practical remarks on their value as applicable to the different diseases, and as affording means of cure when stationary under the unaided efforts of Nature: for these, unhappily for the patient, are not among the diseases in which Nature alone is wont to show the most brilliant success.

Under the head of *Gangrene*, Mr. Coote includes the different terms which have been applied to mortification, or "the doing to death," of different textures of the body, internal or external; "but these different terms," he adds, "have been used loosely, and it would lead to no practical result to attempt any very strict definition." Ulceration and mortification are often mingled; the ulcerative and sloughing processes may go on, as it were, hand in hand. Still, surgically speaking, there is a wide difference between ulceration and gangrene, beyond the extent of the part involved in the spreading destructive disease, and the more intimate sympathy of the constitution with one than in the other; however difficult it may be to frame a definition which shall separate them by a broad line. The causes of gangrene are numerous; the attempts to interpret them aright have been conflicting, and the interpretation not always satisfactory. The death of an inflamed part, it has been remarked, is a very complex matter. The rules for the treatment of mortification, Mr. Coote says, are much simpler than in times gone by; but the means we possess of controlling it are few in number.

Mr. Coote remarks that surgeons do not often witness death from the unchecked progress of traumatic gangrene, in a part destroyed by direct local violence. The fearful amount of local injury, of which it is thought that gangrene will be the certain termination, is too clear an indication for the immediate removal of the injured member, and it is seldom indeed—very seldom, we should say—that the patient is not himself sensible of the urgent necessity for an operation, or that he refuses, when it is really required, to be guided by the judgment of the surgeon when he advises immediate amputation. Yet we do find now and then that he refuses; and we do find sometimes, and when we have hardly expected it, that gangrene in such almost hopeless circumstances will stop short of the extinction of life; as we also find that the amputation of a limb, even at a distance from the injury, and far out of reach, as one would think, of all contaminating influences, does not always save life. The cases in which, in the absence of constitutional causes, predisposing or exciting, the gangrene is most frequently seen to run its course with rapidity, are those where the injury has been to a large nerve or primary bloodvessel, and for which, upon its occurrence, the indication for amputation was not sufficiently distinct. The secondary mortification following injury less severe in its character is less rapid in progress, and more frequently stops, or performs a spontaneous amputation.

Mr. Coote gives a full and faithful description of hospital gangrene, the "good example" of the disease which forms the subject of his essay. Constitutional symptoms, he correctly says, may be generally observed; and where the disease attacks large wounds, such as stumps after amputation, it will often seem as if they were really the precursors of the local manifestation of the disease; though we suspect a more careful observation of the wound and its secretion would have shown early enough that *there*, at least, all was not right. As might be inferred in a state sometimes resulting from atmospheric contagion, or inoculation, the local condition often precedes the sympathy of the system days—aye, weeks. In the wards of the hospital where Mr. Coote has gained his chief experience of the disease, constitutional treatment, most carefully followed out, failed forty years ago to cure a single case of the disease, every one of which in its utmost fury was instantly and permanently checked by local treatment. Pure nitric acid was "generally preferred," then as now, for the reason that no known caustic has such an affinity for animal matter. If applied in its full strength, it is found to be less painful than when diluted. To some extent this is the case with other caustics, and applied in other cases. Indeed, we have heard it suggested that a strong caustic might not inaptly be called an *anodyne*.

The subject of *Ulcers* is treated by Mr. Paget—limiting his essay to the consideration of those forms of ulcer which appear in the integuments—as being of chief interest from the surgeon's point of view. It simplifies a work to have some mode of arrangement and classification for purposes of description; and Mr. Paget describes each of the chief varieties of ulcers, of which sufficiently numerous instances are seen to allow of such arrangement, according to their origin and character; and when not of specific origin, they are named from some prominent or distinctive characteristic. Many of these ulcers, so indicated as inflammatory, eczematous, &c., are also capable of classification as simple ulcers, without constitutional vice, to which accident has added some complication—as local inflammation, eczema, &c. Among the patients presenting themselves at public institutions, such complications are often epidemic, if we may judge from the number of patients in whom they are seen. "The characters of ulcers," Mr. Paget remarks, "if more fully studied, both in well-marked and in modified and complicate examples, would be found as various, and severally, at least, as well-defined, as are those of cutaneous eruptions; and with the better diagnosis that would be thus attained, there might follow a much better discrimination of the means of treatment appropriate for each."

An essay upon *Erysipelas* is the contribution by Mr. De Morgan. "The term," he observes, "has been applied to a variety of forms of inflammation which probably have no true connexion with one another." Of the two chief varieties now universally distinguished, simple and phlegmonous, Mr. De Morgan thinks that there is no real boundary between them. The various modifications of these he regards but as phases of one disease, which has its seat not merely in the part, but in the system: "The form which the disease may take is determined by the state of the system, the temperament, habits, &c., of the individual, or the external causes which produce it." There are, we think, some prominent points of distinction to be noted between the two more commonly observed forms, which tend to make a boundary between them less open to question. If we may believe the practitioners beyond the limits of this great city, in the purer air of the country, or in less crowded towns, and in the county hospitals, phlegmonous erysipelas is rare, while simple erysipelas is of frequent occurrence. The phlegmonous form, if contagious, must be slightly so in comparison with the simple. The phlegmonous species is not often seen at hospitals in its early stages, at the period when local depletions or a milder treatment than free incision is likely to be beneficial and to arrest its progress. We think it may be open to question whether the phlegmonous form is not in reality an inflammation of the areolar and adipose tissues, of a specific character, spreading rapidly with gangrene, inwards to the fascia and muscles, and outwardly to the common integument, rather than a disease of common origin with the simple form—a disease "not of itself a formidable complaint."

Mr. De Morgan has condensed all that is valuable and practical in the literature of the subject; and in reviewing with him the opinions and practice of former writers, we are pleased to receive from his own pen the theory and practice of a surgeon so well able to give the result of his own large experience. Putting the fullest confidence in constitutional treatment, he gives in detail the many local applications recommended to relieve or cut short the attack; but he does not set a high value on them, for he has not found them realize fully the merits claimed for them. Indeed, it almost seems that the more nearly these approach to *harmlessness*, the greater is their merit as medicinal agents.

Mr. Callender contributes an essay on *Pyæmia*, restricting his remarks to those cases in which the mischief begins in a diseased or injured part. The *materies morbi* he considers to be a modification of the blood, resulting from various predisposing causes: "Constituents are probably left in it, unused or unremoved, from defective secretion, or embarrassed excretion, some of which are ready, on the least disturbance, to undergo decomposition. Chemistry fails to give us conclusive evidence of these changes." The systemic infection is caused by some animal poison; and, influenced by predisposing causes, it becomes a general disease, more or less rapid in its progress, with symptoms allied to those of typhoïd fever, or it may be limited to a local action more or less diffused. Mr. Callender has noted with care the pathological changes, as deduced from examinations, for which the numerous fatal cases give abundant opportunities for observation; and he gives a selection from the views taken by such previous writers of authority as have received the greatest amount of credit, and he relates the arguments by which they have been supported. Although some cases have been recorded in which patients have recovered, Mr. Callender remarks that "pyæmia is dependent upon a state with which life can scarcely be prolonged;" and he urges most strongly, as a thing of the greatest importance, where therapeutics fail so signally, the paramount necessity of attending to prophylactic measures.

Mr. Poland, in treating the subject of *Tetanus*, remarks that the degree of the local injury bears no relation to the severity of the symptoms; yet the relative proportions which the occurrence of the disease bears to the several surgical lesions show a great preponderance of cases among the more severe varieties of injury and accident, such as compound fractures, burns, and injuries to the fingers and toes. With the pathology "still involved in great obscurity," and in the absence of "an appropriate means of cure," Mr. Poland abstains from offering any remarks on the numerous medicines which have been administered and proposed as remedies. Woorara, one of the most recent, first used by an Italian surgeon, "has at present not fulfilled its purpose;" no real success, we believe, followed the use of it upon the wounded after Solferino.

A short essay on *Delirium Tremens*, contributed by Dr. Barclay, contains a good medical description by a physician of the special characters of the disease in its most striking form. "The information derived from anatomical research," he remarks, "is worthy of study, because, as it tells how death is actually brought about, it offers suggestions as to how the fatal event may be prevented or warded off;" and he urges the importance of clinical study, as it alone furnishes data on which we can rely in forming any notion of the true nature of the disease, where anatomy often fails as a guide. "The moral management of the patient" is recommended as "the point of most importance in practice," coercive measures being justifiable only under unavoidable necessity. This necessity unfortunately too often exists in the circumstances in which the unhappy patient is placed. Where *quiet* can be secured, and complete separation from all continuance of excitement, as in the lunatic asylums (where cases often happen), personal restraint is not found to be necessary; the patient, if violent, is put into a small ward by himself, from which the light can be excluded, with padded floor and padded walls, where he cannot hurt himself; and it is found that he obtains rest, and the cure follows.

Mr. Savory contributes an essay on *Scrofula*, a word "in one respect at least unfortunate, because very different significations are attached to it;" he proposes to understand by it "a certain disease or defect of the constitution, in which there is a tendency to the production and deposit of tubercle (a substance which possesses no inherent power of growth) in various tissues and organs." In fact, he uses the term as generally identical with tuberculosis. He describes the minute structure of tubercle and the elements proper to it as recognisable under the microscope, and he traces the history of its transition through the various changes it undergoes. The symptoms of scrofula are the symptoms of debility; it is a disease of early life, common before the middle period, rare after it—the predisposition undoubtedly hereditary. "Can the diathesis, if not hereditary, be acquired?" is a question to which Mr. Savory thinks it is, from the nature of the subject, almost impossible to give an unequivocal answer.

A shorter essay follows, by the same hand, on *Hysteria*—a disease equally a disease of debility, seldom seen in healthy, or, at all events, not in strong constitutions—generally associated with some form of debility of the system or defect in the general health. Mr. Savory describes the symptoms of hysteria, whether pure or complicated, and the diseases which are mimicked by the patients under the influence of this obscure affection, and he points out that the symptoms are exaggerated, and the difficulty of diagnosis is incalculably increased when hysteria supervenes upon real disease,—a combination which, however, is comparatively rare.

Mr. H. Lee contributes a treatise on *Syphilis*. His experience of venereal diseases has been gained by long observation of the natural progress of the diseases occupying the parts concerned in sexual congress. He is candid in his acknowledgments that much yet remains to be done for the complete elucidation of those cases which give rise to such great contrariety of opinions and practice. In the introductory chapter he canvasses many of the opinions which have been of late years published by foreign practitioners upon the subject of inoculation with venereal virus, and the conclusions which have been drawn from them. Much of this experimental inoculation has been made with matter taken from various sores, not all probably of the same specific character, or in the same stage of progress. It is also an objection that the experiments have been for the most part, if not always, made upon the diseased subject. In order to show the natural progress of the original disease, if a constitutional affection, another subject should be taken, in good health—an expedient which it is not likely will ever be adopted extensively,—at least, in this country. Moreover, we think these experiments, even on the person of the patient, are not so free from danger as is generally supposed. We have known the ulcer produced by inoculation continue to spread, and proceed unchecked, till the death of the unfortunate patient put a period to farther observation. The scaly form of disease can perhaps be always introduced by inoculation from the secretion of the primary sore, and it may proceed on its regular course, through known stages, little (if at all) modified by peculiarity of constitution. Not so the phagedenic; this takes root less readily, unless when engrafted on a fit stock. At all events, we seldom see it, either hereditary or acquired, in its full characteristic virulence, except in what may be called rotten constitutions, or if in young and (what might be thought) healthy subjects, young rakes, fair people with thin skins and thin bloodvessels. As Mr. Lee says, "pustular eruptions do occur; but they are rare in unimpaired constitutions." It need not be a matter of surprise that some forms of known constitutional disease, or cachexia, cannot be engrafted by inoculation. Cancer, for instance, has never been; and experiments have been abundant on the persons of surgeons engaged in operations. A professional friend once told us that he and others of his acquaintance had tried in vain to inoculate measles.

Mr. Lee's investigations confirm the observation of some former writers of the great numerical frequency of sores followed by irritation of the absorbents, and extensive suppurating buboes, without constitutional infection, or "secondary" symptoms; and also that there are constitutional affections, slow in progress, and not in general preceded by visible contamination of glands—the irritation and enlargement of the lymphatics often escaping the patient's observation, as, we think, the original sore of the scaly form will sometimes. His investigations also go to support the opinion long and publicly maintained, that there *is* a steadily progressive venereal disease, though of (pathologically speaking) a mild character, not tending to ulceration—a disease which it has been too much a fashion in the present day to doubt and even to deny. We think the distinction proposed between a "suppurating sore" and an "infecting chancre" is a good and practical one; and we shall be glad to see Mr. Lee distinguish further between the secondary affections. The "suppurating sores" require a further separation into those which are harmless and those which are followed by phagedenic or other secondary affections.

The proposal to cut short the progress of venereal diseases by excision of the primary sore does not find favour with Mr. Lee: "the cut surface will in a few days take on a specific action." Mr. Evans pointed out, many years ago, that the diseased action of the sore, "which," he says, "is more frequently met with than all other ulcerations on the parts of generation put together," extends in many instances a considerable distance beyond the seat of the sore, and that, if leeches be applied to the groin for the reduction of bubo, the wounds made by them often become sores of the same form. But, in truth, the patient generally applies too late for excision to give a fair chance of removal of the disease: the poison has taken effect by the time he discovers the sore which gives rise to his anxiety. We are not aware that the vaccine vesicle has ever been excised. Dr. Walker's experiment was the nearest approach to this, and he considered that the constitution was sufficiently infected, notwithstanding the strange proceeding.

Mr. Paget, acknowledging the great difficulty felt by most preceding writers of a correct definition of *Innocent Tumours*, arranges these irregular growths—these "lumps" that ought not to be—according to their "chief and most nearly constant distinctive characters," thus forming a classification which shall include all known forms of visible swellings in the body, or all that are of interest in practice, or (we may add, perhaps) that are of alarm to a patient; a "tumour," wherever situated, being generally a source of anxiety if its nature is doubtful. The modes of development and onward growth, with their minute anatomical and pathological structure, are given; the difficulties in many of them, met with in practice, and interfering with what have been thought established rules for diagnosis, are carefully considered; and many valuable remarks are made upon the surgical treatment of the different kinds of tumours, to which the reader may refer again and again with advantage.

Mr. Moore contributes a long and carefully drawn essay on *Cancer*. "The subject," he remarks, "is one in which there is the strangest disproportion between the amount and the practical value of our knowledge." The material of the essay, we may suppose, is the result of his own experience, confirmed by that of his colleagues, and gathered among the ample records of the Middlesex Hospital. The various forms in which cancer presents itself to the notice of the surgeon are arranged by Mr. Moore as Scirrhous, Medullary, Melanotic, Epithelial, and Osteoïd. Of these shades and shapes of malignant disease, scirrhus is the most frequent form in this country, and "in early stages of the disease it often possesses every character of a tumour but that of swelling." Mr. Moore calls attention to the very early period at which infection takes place in the absorbent glands which are anatomically associated with the site of the disease. He has observed them to be decidedly tender before they were in any degree enlarged or indurated. Often, indeed, if not commonly, the first change is rather a diminishing than an increase of their bulk, and a rounding of their form with bullety hardening. "I have met with very few instances in which the glands were not already affected, however recent the primary disease, and with still fewer in which they were not involved at the time of death." Upon the subject of treatment, Mr. Moore considers that the greatest benefit at present obtainable from medical treatment is that which is likely to arise from improving the general health; "as that fails, the disease advances; with its improvement the cancer-growth is stayed." Removal of the primary tumour by surgical operation lengthens life in some cases which were hastening to a fatal issue, and can rarely be accused of really shortening it. But, he remarks, "the majority of the cases selected for operation are such as present the least probability of early death; these give too favourable a view of the operation."

A short essay on *Contusions*, and a longer one on *Wounds*, conclude Mr. Paget's contributions to the volume. Mr. Paget describes the changes which take place in the parts contused, which may be studied in their proper textures, and in their bloodvessels and blood; and he also describes the mode in which reparation is effected after such injury. Upon the subject of incised wounds, Mr. Paget gives some good rules for their treatment, in which a large amount of practical observation is brought to bear on a subject on which writers have not often bestowed so much care. "There are comparatively few," he says, "in which the healing by immediate union is not desirable, or worth attempting."

The subject of *Poisoned Wounds*, or rather those received from poisons derived from the animal kingdom, is treated by Mr. Poland. The wounds so frequently received in dissection, as generally carried on, are not ordinarily attended with any peculiar or specific disease, or, we may add, with any characters which should separate them from other incised or punctured wounds. It is otherwise with the wounds —the "pricks" or "scratches"—received in the examination of recently dead bodies. More alarming symptoms follow these; in rare cases, it is true; but still they do happen with sufficient frequency to receive, as they require, a large amount of attention, and the more so, as they do also sometimes lead to a fatal result. These alarming symptoms, we think, arise more frequently in cases of inflamed serous membranes, especially the peritoneum.

After treating of these, Mr. Poland proceeds to consider separately the wounds received from healthy animals, the stings and bites of "venomous beasts," and those from diseased animals, of which hydrophobia and glanders are the best known and most frequent, and, we suppose we may add, with the general consent of our readers, the most uniformly fatal in their results in the human subject, as well as in the brutes from which they are derived.

In an essay by Mr. Moore, upon the general subject of *Wounds of Bloodvessels*, excluding those of the larger arteries and veins, which are to form the subject of other parts of the present series, he gives a good criticism upon the acknowledged modes of treatment in such cases. When giving in detail the rules applicable to ordinary hæmorrhage, Mr. Moore urges the importance, when bleeding continues, of clearing away the clots which have collected, and exposing the bleeding surface freely to the air. The effect of such treatment is sometimes exemplified in a remarkable degree when it becomes necessary to lay open the integument, and even the more deeply-seated parts. It seems as if the blood, being injected into the areolar tissue, hinders the vessels from retracting efficiently within their sheaths. Many years ago, a man was brought into St. Bartholomew's Hospital, shortly after receiving a kick in the groin from a horse. A large and rapidly-increasing swelling arose at the part. This was laid open freely by Mr. Lawrence, the clots were cleared out, the hæmorrhage ceased, and no vessel required ligature. Free suppuration followed; and the wound eventually healed well.

The readiness with which arteries "escape threatened injury" is generally attributed to their great elasticity, or to the firmness of their outer coats. We think that some share of this freedom from danger may not unjustly be attributed to the nervous twigs from the sympathetic system which so abundantly surround the tubes. The large arteries have been known to escape injury when a bullet, or a piece of shell, has passed through a limb close to their natural course. A pitchfork has often been thrust through the neck immediately in front of the vertebræ without any bad consequence. In the riots in Paris in 1848, a cavalry soldier thrust his sword through a man's neck in this part, inflicting an injury from which the man recovered after a few days' rest in hospital.

Collapse, and the general effects of shock upon the system, are treated by Mr. Savory. To estimate the real effects, it seems to us that we must take "shock" in a vastly different number of cases. The effect will be different in the shock from an operation and in that from an injury; for one the patient is prepared, not so for the other. The effects will also be aggravated by loss of blood. Much obscurity still attends the cases of what are called "Death from Shock." Mr. Savory attributes the death from such a cause—the "purest example" of which is in a blow on the epigastrium—to a sudden and violent impression on some portion of the nervous system, acting at once through a nerve-centre upon the heart, and destroying its action," often producing death "without any detectable lesion." In these, as in some other cases, something more is required to be known beyond the facts, or absence of facts, discernible on a pathological examination, to enable us to say what was the real cause, physiologically speaking, of the fatal termination. The symptoms during life should be known—the "history of the case," as we are in the habit of saying. The influence of respiration on disease, especially in its effects upon a severe surgical accident, has not been sufficiently noted. In the case of death from shock, what is the state of the lungs at the time of the injury? If slightly expanded, or, in common language, "empty," the little air is suddenly expelled by muscular contraction, and the patient dies; if full, emphysema follows. A carriage shall run away, and four ladies be thrown out as it is overturned; each will have her power of resistance to injury, in proportion as she has breath, or is without it.

Cases of death from "sudden and powerful emotions of the mind" are now and then reported in the medical journals. What happens when the patient does not die? Is recovery, as it is called, complete? Are no permanent traces left upon the system? A serjeant in a *crack* regiment of dragoons, on retiring from service, was appointed superintendent of a railway station. One day he attempted to pass his own child into the arms of the stoker of an engine which was passing the station; the child fell, and the truck following the engine went over it, causing immediate death. The father, up to that time a strong man, became so forgetful and inattentive as to be unfit for his duties. He was discharged and removed to a lunatic asylum; in a few months he died, from the general paralysis of the insane. The apparent cause of death was shown to be emphysema of the lungs, with congestion of the brain.

Mr. Holmes, the editor, contributes an essay on *Burns and Scalds,* and another upon the general pathology of dislocations. He follows Dupuytren's classification of burns and scalds, as being the most widely accepted, and the most complete and practical; observing that the *extent* of the burnt *surface,* as well as the depth to which the injury reaches, are important considerations, which, however eluding attempts at formal classification, should be taken into account in attempting a general arrangement, or one which shall embrace all the elements necessary for prognosis and treatment. "The extent of a scald often compensates, as it were, for the superficiality." In setting the pathology of these injuries before his readers, Mr. Holmes gives a valuable analysis, compiled from the case-books of St. George's Hospital, containing a full account of the history and post-mortem examinations of a large number of fatal cases. He thinks the causes and modes of death require to be more accurately determined, as they should serve for indications for the constitutional treatment of the patients. A short account is given of accidents from lightning—rare in this country, and of which few of us have seen many examples.

The general pathology and treatment of *Fractures* form the subject of the essay contributed by Mr. Hornidge, in which he has brought together within a short compass whatever is valuable in modern authors upon the subject he is treating.

The volume closes with a short essay by Mr. Holmes upon *Dislocations,* in which he discusses the general principles deducible from the pathology applicable to the treatment of traumatic dislocations; such displacements at least of one articular surface of bone from another as are caused by violence.

The general plan of the work, as far as we may be allowed to judge from the single volume before us, is well conceived, and the task has been executed with industry. The contributors have been well selected, and their united labour has been well expended. Mr. Holmes has done well in enlisting the services of the younger, though comparatively untried, writers, and writers from different schools. Without too much of what is merely historical, they have managed to place before the reader, in a small compass, and in clear essays, what is valuable in modern knowledge, and trustworthy as a guide in practice. The descriptions of diseases for the most part are good, and well drawn; and the symptoms well described. The frequent allusions in different essays to subjects not yet treated, show that the present volume is intended to be introductory to the fuller treatises which are to appear hereafter. From the ability displayed in the more excursive views appropriate as preliminary to a more extended subject-matter, we shall look with well-grounded interest to the judicious concentration of practical details on special points. This we consider to constitute the essential core of a systematic abridgment of the present state of professional knowledge, furnishing at once easy access to condensed material, and the assistance of a discriminating experience.

PART SECOND.

Bibliographical Record.

Art. I. — *Ophthalmic Hospital Reports and Journal of the Royal London Ophthalmic Hospital.* Edited by J. F. Streatfeild.— *London.* Nos. IV. to XII. inclusive.

It is now three and a half years since the above-named periodical—the only one in England devoted exclusively to ophthalmic considerations—was undertaken; and we are glad to find that it has fully attained that high position which in our notice of the first three numbers (see the Review for October, 1857) we ventured to predict would be accorded to it, provided the scientific and practical character with which it started were maintained. The chief structure of the material brought together in its pages has been the same throughout all the numbers, but in the fourth one a modification as regards the quarterly reports of operations at the Ophthalmic Hospital was made, rendering them less formal and conspicuous, and consequently less tending to confer importance on the practice of a single institution; and this arrangement has been adhered to ever since. In the first number existed some expression of doubt as to the co-operation of the profession in contributing to its pages. We now have to congratulate Mr. Streatfeild and the other authorities of the Ophthalmic Hospital that much good literary support has been forthcoming from so many quarters. The monographs which appear are of the most varied character. To select from them for special mention would be an invidious task when so much excellence characterizes all, and to attempt a description of the original communications which would be of any real value would require more space than we have at our disposal. We shall therefore content ourselves with giving a simple enumeration of the various papers presented in the last nine numbers of the 'Reports.'

Commencing in No. IV. with a notice on the Operations for Cataract among the natives of India, by Mr. W. Martin, we pass on to the continuations of Mr. Poland's paper on Protrusion of the Eyeball, and of Mr. Hulke's on the Growth, &c., of the Crystalline Lens; and these are followed by a dissertation, by Mr. Hutchinson, on Hereditary Syphilitic Disease of the Eye. In No. V., Mr. Hutchinson's paper is continued, and we have one by Mr. V. Solomon on Reclination of Cataract with Two Needles, one by Mr. Critchett on Iriddesis, and another by Dr. Bader on the Ophthalmoscopic Appearances of Secondary Syphilis. No. VI. contains Dr. A. S. Taylor's contribution on Ophthalmia from Arsenical Wall-papers, Mr. France's contribution on Cataract and Diabetes, Mr. Dixon's on the Extraction of Foreign Bodies from the Vitreous Chamber, and two papers by Mr. Hulke, one being on Dacryops and one on Injuries causing Rupture of the Eyeball. In No. VII. will be found a paper by Dr. Letheby on the Iris, and one by Mr. France upon the Forceps in Extraction of Cataract. In No. VIII. we have the continuation of Mr. Hutchinson's paper on Syphilis, &c., one by Dr. Bryson on Night Blindness in connexion with Scurvy, as also the first part of Dr. John Wells's paper on Paralytic Affections of the Muscles of the Eye, which is continued in all the succeeding numbers. No. IX. furnishes Further Remarks on the Formation of Artificial Pupil by Mr. Critchett, and a paper by Mr. Bowman on Conical Cornea and its Treatment by Operation. In No. X. Mr. White Cooper writes on Foreign Bodies in the Eyeball, and Mr. Poland continues his observations on Protrusion thereof. No. XI., more bulky than its associates, comprehends a short paper on Glaucoma and the Optic Papilla by Dr. Mackenzie of Glasgow, a continuation of Mr. Poland and Mr. Hutchinson's above-mentioned observations, and a lengthy paper by Mr. Streatfeild, the editor, on Corēlysis, or pupil-freeing (κόρη and λύσις).

The last and twelfth number, forming the first one of Volume III., embraces the conclusion of Dr. Wells's paper before mentioned, and also a paper by Mr. Hancock, On the Division of the Ciliary Muscle in Glaucoma.

Of the above original communications it may be observed that well-nigh all are practical, and altogether in other respects of such a nature as to elevate the specialty of British ophthalmic medicine with which they are connected.

Besides original matter, the Reports contain a vast assemblage of facts, observations, and selected cases by numerous observers; and we observe in No. XII. that a novelty exists in the shape of a "Periscope of Foreign Ophthalmology." This, we are particularly glad to see, is promised as a permanent addition to the 'Reports;' and the more so, as in our own quarterly chronicles we have so little room for supplying English readers with foreign science of this kind. We heartily hope the energetic and able editor of this ophthalmological quarterly may find such additional support, both literary and pecuniary, that he may be the better enabled to render the volume committed to his charge a full exponent of the particular department of knowledge to which it is devoted.

Art. II.—*A Practical Treatise on Mechanical Dentistry.* By Joseph Richardson, D.D.S., M.D., Professor of Mechanical Dentistry in the Ohio College of Dental Surgery. With 110 Illustrations.—*London,* 1860. pp. 427.

Dr. Richardson's work is a desirable addition to those we have on this subject. It contains a valuable amount of information, which cannot fail to command the attention of all those who are interested in this branch of the profession. It has been said, "That any one who has had great experience in his art, and is sufficiently generous to give a candid result of his opportunities of study to his fellow practitioners, is entitled to the thanks of the whole profession." No one will be found more worthy in this respect than Dr. Richardson.

We are well aware that the mechanical branch of dentistry can only be acquired by practical manipulation; still, it is of the first importance to the student, and a great boon to the young practitioner, to have at command the experience of one who has devoted so much study to elucidating the difficulties that occur in practice as Dr. Richardson has done. Only those who have had to overcome such difficulties without such a help can fully appreciate the benefits conferred by such a work.

The first part of the book is devoted to "Metals employed in Dental Laboratory Operations, with preliminary Observations on the different Modes of applying Heat." The author differs from the majority of dentists as to the quality of the gold that should be used as a base for artificial teeth. The standard he recommends is higher than that generally used. The advantage of which, however, we conceive to be somewhat counteracted by the larger proportion of copper recommended in the alloy.

The second part of the work is devoted to "Artificial Dentures," and consists of no less than eighteen chapters. In the chapter on "Treatment of the Mouth preparatory to the Insertion of Artificial Dentures," the author says:

"It rarely occurs that all the structures of the mouth are in such condition as will render it proper to insert an artifical appliance without some preparatory treatment. This important requirement cannot, in any material respect, be disregarded by the practitioner without endangering the utility and permanence of the substitute, and inflicting upon the patient a train of consequences alike distressing and pernicious. These consequences cannot be wholly averted by the most skilful manipulation, but they may be greatly magnified by a defective execution of the work, or by a faulty adaptation of the appliance to the parts in the mouth." (p. 110.)

In the article on Pivot Teeth, the destruction of the tooth pulp is thus described:

"Its extirpation may generally be readily effected with the use of a three or four-sided, barbed, untempered broach; which, being small enough to penetrate freely to the apex of the fang, is thrust quickly to the bottom of the canal, rotated, and withdrawn: when, if the entire nerve does not come away adherent to the broach, the operation may be repeated, with comparatively little pain, until all portions of it are removed." (p. 181.)

This is an operation more ingenious, we fear, than pleasant.

In the chapter on "Defects of the Palatal Organs," we find

"The object of the labor and skill bestowed in making and adapting an artificial velum is not attained when the instrument is completed, be it performed never so skilfully, for the speech remains almost unchanged; nor is there much involuntary improvement, but at first only a sufflated tone, like that of a person with a cold. There remains a course of *vocal practice* to be entered upon and patiently persevered in, before any great improvement in speech is attained." (p. 407.)

The two articles on "Holding Pieces in the Mouth by Atmospheric Pressure," and "Vulcanite Base," are fully and most ably treated; but to discuss these subjects would occupy more space than we have at our command. The scope of the whole work is thoroughly carried out; and to any one desiring a theoretical knowledge of dental mechanics, Dr. Richardson's book will be found a most efficient guide.

ART. III.—*The Scale of Medicines with which Merchant Vessels are to be furnished by command of the Privy Council for Trade; with Observations on the Means of Preserving the Health of Seamen, &c. &c.* By T. SPENCER WELLS, F.R.C.S. Second Edition.—*London.* pp. 212.

THIS edition has been most carefully revised, and is superior to the previous one of 1851, inasmuch as it contains notice of all recent information bearing on the matters with which it is concerned. Well known and highly valued as was the earlier edition, the present one is rendered still more serviceable, as it embraces the latest alterations made in the scale of medicines, as also the clauses of the "Registration Act," the "Merchants' Shipping Act, 1854," and the "Passengers' Act, 1855."

ART. IV.—1. *Um Typhussóttina, eda Taugaveikina, er menn kalla hjer á landi, orsakir hennar og medferd. Stutt leidbeining fyrir almenning, er eigi nær til læknishjálpar, af landlækni* Dr. J. HJALTALIN.—*Reykjavík*, Einar Thórdarson. 1860. Large 8vo, pp. 16.

On Typhus, or Weakness of Fibre, as it is here popularly termed, its Causes and Treatment. A short guide for those of the public who are not within reach of medical advice. By Dr. J. HJALTALIN, District Physician.

2. *Islenzka Homöopathian og Nordlenzku Prestarnir, ritad af* Dr. J. HJALTALIN.—*Reykjavík*, E. Thórdarson. 1858. 8vo, pp. 48.

Icelandic Homœopathy and Northern Parsons. Written by Dr. HJALTALIN.

3. *Vísindin, reynslan og Homöopatharnir.*" Af Dr. J. HJALTALIN.—*Reykjavík*, 24th Nov., 1856. 8vo, pp. 16.

Philosophy, Experiment, and the Homœopaths.

THREE *brochures*, by the same author, and reaching us from a district which so rarely seeks our editorial notice as Iceland does, may well, though treating of very different subjects, be classed together. In a short preface to the first work in the above list, the writer informs us that typhus has long been endemic in Iceland, and he states his belief that there is no other region in Europe where it prevails to an equal extent, having fully kept pace with the increase of population. Hence it is often mentioned in the annals and year-books of the island. He appears to hold the doctrine of the unity of typhus and typhoïd fever, for he says that there are various species of the disease, two of which are the most common—viz., "typhus contagiosus" and "typhus abdominalis," and that the latter may run into the former chiefly in proportion as the debility increases. The author treats at length of the symptoms of typhus fever in its several stages, of which he enumerates five—viz., the stage of incubation, the incipient stage, the stage of eruption, the nervous stage, and the stage of crisis. Of the course of typhoïd fevers he makes a similar division. The third section of the work is devoted to the etiology, and the fourth to the treatment of typhus and typhoïd diseases. The title of the book indicates that the *brochure* is intended chiefly for the use of non-professional readers.

Of the works on homœopathy it will suffice to record the titles. Such essays may, perhaps, still be required in Iceland, but to revert to this delusion in our pages would be pure and simple loss of time.

ART. V.—1. *Operative Surgery adapted to the Living and Dead Subject.* By C. F. MAUNDER, &c. Part II. 1861. pp. 174.

2. *A Manual of Minor Surgery and Bandaging for the use of House Surgeons, Dressers, and Junior Practitioners.* By CHRISTOPHER HEATH, F.R.C.S., Surgeon to the West of London Hospital.—1861. pp. 200.

BOTH these works have the considerable merit of telling their readers what they want to know in intelligible terms, and in very short compass. We have already* had the pleasure of speaking in commendation of the first part of Mr. Maunder's book on Operative Surgery, and have examined this second part with considerable attention. It appears to us to be as good as any of the manuals on the subject yet published, while its very low price will no doubt recommend it to the patronage of medical students. Mr. Maunder has been almost prodigally liberal in illustrating his book. Many of the cuts are both useless and, to say the truth, hideous also. Witness the frightful cut of amputation at the hip-joint on page 65, in which the man's figure is absurdly out of drawing, the knife far too short, and the whole gives no exact idea of the place at which the knife should be entered and brought out, and therefore might have been omitted without detriment to the book; witness also the horrid drawings on page 137, to illustrate the operation of extraction of the lens, where the knife appears to us out of all proportion, and the staring head of the patient does not seem to be of any use except to give fastidious readers "the horrors." A few of these and such-like cuts may be usefully expunged from the next edition, and perhaps Mr. Maunder may think it right to confine his book more exclusively to *operative* surgery, excluding the short and very insufficient references to the after-management of cases and other general topics which the present work contains. The book,

* See British and Foreign Medico-Chirurgical Review, Oct. 1860, p. 476.

however, is a very good and useful one, and one to which we heartily wish the success which we believe it merits.

Mr. Heath's is another of the numerous works by which at the present day all the difficulties of the art of surgery are removed as far as books can do it. This work is addressed especially to house surgeons, and appears to contain plain directions what to do in each of the emergencies which are likely to occur in the practice of a large hospital. No one could be better qualified than Mr. Heath to write a book of this sort. He is already favourably known in the hospital where he was himself house surgeon (King's College) for some ingenious improvements in the after-treatment of cases of excision of the knee; and his book is practical, modest, and sensible. The danger of such books is lest the persons chiefly addressed in them should trust to their manuals rather than to a careful previous study of the cases brought under their notice during their student career. Books, however good, are very poor substitutes for the coolness, dexterity, and readiness which constant practice alone will give. Still, Mr. Heath's book will be found a very useful companion to those capable of profiting by his ample experience.

ART. VI.—*Ale, Wines, Spirits, and Tobacco. A Lecture delivered before the Leicester Literary and Philosophical Society.* January 7th, 1861. By JOHN BARCLAY, M.D., F.R.C.P., &c.

"To use but not *ab*-use" might be appropriately selected as the motto of this pamphlet, which we have read with attention and with no little interest. It does not profess to contain original scientific views, but what is more to the purpose with which it was delivered, it explains and reduces to the comprehension of the audience some of the most scientific and most recent discoveries connected with the physiology of the subject. It is, moreover, perfectly and easily intelligible, amusingly written, and bears the character of honest common sense.

As indications of the tone which pervades this little work, we would instance the two following passages. Speaking of temperance as to fermented liquors, the author observes:

"The definition of moderation is like the definition of health, like the definition of sanity,—one is more, another less excitable, one is more, another less mad, and no one is absolutely perfect in his organization either of body or of mind. As a rule, all drinking, except at meals, all frequent and occasional drinking, and all use of raw spirits, unless under most exceptional circumstances, cannot be classed under moderation.

"But each of us has had a gauge, a correct and infallible gauge, given him by the Almighty. Our own consciences will ever and always tell us when we have contented ourselves with moderation, and still more faithfully will warn us against the earliest approach to indulgence or excess." (p. 62.)

Again, as regards the cant so frequent on this matter:

"Do not taboo the honest publican, or you make him forget and overlook his dignity as a respectable tradesman and townsman. Let us never forget, as we sit in our comfortable drawing-rooms bemoaning the ignorance and self-indulgences of the poor, that they have no comfortable homes, and that the attraction of the warm, well-lighted, and sociable public-house will often tempt a man from his slatternly home, who has no desire or intention of drinking to excess. Let us therefore apply ourselves to the bettering the condition of the working class, and improving their education; let us encourage reading-rooms, mechanics' institutes, free libraries, and museums, coffee-houses, lectures, exhibitions, public parks, recreation grounds for games (and not sell our cricket-ground for building purposes); above all, popular concerts and music, always attractive." (p. 66.)

Failing more space for quotation, we will say in conclusion, that we heartily wish that science and political economy were always so judiciously popularized as we find them in this pamphlet.

ART. VII.—*A System of Instruction in Quantitative Chemical Analysis.* By Dr. C. R. FRESENIUS. Third Edition. Edited by L. LLOYD BULLOCK, Fellow of the Chemical Society.—*London,* 1860. pp. 687.

THE value of this work is very considerably enhanced by much fresh matter and new considerations. We shall notice its characteristics in an article upon chemical text-books and methods of chemical teaching, which we hope to insert in an early number of the Review.

ART. VIII.—1. *Gheel, ou une Colonie d'Aliénés, vivant en famille et en liberté.* Par JULES DUVAL.—*Paris,* 1860. pp. 214.

Gheel, or a Colony of Insane, living together and at large. By M. JULES DUVAL.

2. *De la Réforme des Asiles d'Aliénés.* Par le Docteur J. PARIGOT. —*Bruxelles,* 1860. pp. 32.

On the Reform of Lunatic Asylums. By Dr. J. PARIGOT.

3. *The Cottage System and Gheel; an Asylum Tract.* By JOHN SIBBALD, M.D. Reprinted from the 'Journal of Mental Science,' 1861.—pp. 31.

ONE of the great medical and social questions of the day is, "What shall be done with our lunatics?" for, to all appearance, the prevailing plan of aggregating them in asylums is a failure. New asylum accommodation is perpetually being provided, and yet the demands for it are not met, and this inequality between supply and demand threatens to continue, if the present opinions of the requirements of the insane, and the prevailing system of supplying those requirements, continue to be maintained. But it is not for us, in this place, to attempt to discuss the question, what classes of insane patients do, and what classes do not require the special arrangements and management of asylums constructed and organized on the plan usual in this country. Suffice it to say, that the opinion is gaining ground that many so-called lunatics may be advantageously provided for elsewhere than in such expensive establishments; but how this view may be best carried out is still a moot point.

The *brochures* whose titles head this notice are written in support and in illustration of the plan of distributing lunatics in isolated and limited communities of the sane, and so of according to them a mode of life as little removed from that of ordinary domestic life as possible, and also an amount of personal and social liberty unattainable in asylums. The model of this proposed arrangement is presented in the history and constitution of the ancient colony of insane in the little town of Gheel in Belgium, respecting which M. Duval has in his work given full particulars. M. Parigot was formerly the chief physician of Gheel, and his name is intimately associated with it, and with the advocacy of the system of treatment it represents. The pamphlet by him under notice is written chiefly as a reply to objections and criticisms which several visitors to Gheel have expressed. Among others, Dr. Stevens, who wrote an account of a visit to the colony in the 'Asylum Journal,' 1858, has come in for some sharp rebuke from Dr. Parigot for some statements he put forth. For, according to the Belgian physician, Dr. Stevens, to say the least, completely misunderstood the observations of Dr. Bulckens, the chief physician of Gheel, and therefore misstated them in his paper; a circumstance which M. Parigot attributes to his imperfect knowledge of French, and his "misapprehension, both of what was stated to him, said, and indeed of what he saw." (p. 28.) Moreover, Dr. Sibbald, in his judicious and carefully-considered pamphlet on the organization of Gheel, evidently cannot coincide with Dr. Stevens's adverse statements respecting that remarkable colony. On the contrary, Dr. Sibbald bears important testimony in favour of Gheel, after a careful examination of its condition and management, extended over several days. But what is of still more interest, he enters into the inquiry how far a similar plan may be applicable in this country, and to what classes of patients it promises to be most useful. In doing this, he takes the population of the Edinburgh Asylum, and divides it into groups, according to the mental condition of its members, and their suitableness for treatment in the asylum, in detached buildings, and in cottages. "From these data it appears that about 8 or 9 per cent. of the inmates might be boarded in cottages, and about 19 or 20 per cent. in the detached buildings."

Throughout his pamphlet Dr. Sibbald endeavours to mete out full justice to the advantages presented by the organization of the establishment at Gheel for certain classes of insane; and also to discuss impartially its defects. It is, in our opinion, one of the fairest accounts of Gheel, and of the system of providing for the insane that Gheel typifies. And if this "*système Belge*," as it is now called, has for many past years commanded the attention of physicians and of philanthropists generally, it deserves, at the present time, renewed and careful study, for it is very evident that the prevailing system of treating lunatics in aggregated communities in huge asylums must ere long be replaced, at least in part, by another. We are therefore glad to call the attention of our readers to the treatises quoted above on the colony of Gheel, and will take the opportunity to refer those who would inquire farther into its history and condition, to the notes by Dr. Webster in the 'Journal of Psychological Medicine,' 1857, and to the larger treatise by Dr. Parigot, entitled, 'L'Air Libre et la Vie de Famille dans la Commune de Gheel," 8vo, *Brussels*, 1852.

ART. IX.—*Die Krankheiten des Nervensystems.* Von Dr. RUD. LEUBUSCHER.—*Leipzig*, 1860. pp. 320.

On the Diseases of the Nervous System. By Dr. LEUBUSCHER, &c.

THIS volume before us is one of a series of treatises on clinical medicine now in course of publication in Germany. The author is already favourably known by his able book on 'Diseases of the Brain,' published some six years ago, as well as by several less considerable contributions to medical science.

The present work may be regarded as a manual of diseases of the nervous system, including both the brain and spinal cord. It is also not only a pathological, but also a therapeutical treatise, and thus supplies a useful introduction to the diseases of the nervous system, confined within convenient limits for the student.

These characters of the work just referred to are, however, those which would render it more difficult to review systematically; but the position of the author as a principal teacher in one of the larger university medical schools of Germany, and as a well-known pathologist, furnish a guarantee of its value and soundness. At some future time its contents may come under notice in a critical examination of the present state of the pathology of the nervous system.

ART. X.—*Contributions to the Anatomy of the Spinal Cord.* By J. B. TRASK.—*San Francisco*, 1860. pp. 16.

THIS paper treats of the anatomy of one portion only of the cord—viz.: that included between the upper surface of the second cervical and the lower surface of the fourth cervical vertebræ, in the sheep. Dr. Trask prefaces his remarks by a brief historical sketch of previous researches, and pays a just tribute to the labours of Mr. J. Lockhart Clarke, to whom we owe the means of obtaining "such superior definition of these minute forms of structure," and whose plan has been adopted by Schröder van der Kolk with successful result. Dr. Trask, however, states that by "the use of neutral chromate of potash, and subsequent to this a very dilute solution of caustic alkali, neutralizing this with acetic acid, and washing with water," he has obtained sections ready for mounting in seventeen hours. We have not succeeded so well by these means as by following the directions of Mr. Clarke.

A large portion of Dr. Trask's pamphlet is occupied in combating Dr. Todd's denial of the existence of a central canal in the spinal cord. Those of our readers who have made themselves acquainted with the literature of the subject, or who have examined any good sections of the cord, will know that Dr. Todd's opinion was attributable to the mode employed in its examination. Dr. Todd dried the cord on slides; the inevitable result was that the structure became torn and distorted.

In moist transparent sections the canal is patent enough under moderate powers of the microscope.

Dr. Trask gives a careful description of the cells contained in the cornua, but does not throw additional light beyond what we are already in possession of, with reference to the organic connexions or physiological endowments of the cells.

ART. XI.—*Le Non-Restraint, ou de l'Abolition des Moyens Coercitifs dans le Traitement de la Folie.* Par M. le Docteur MOREL.—*Paris*, 1860. pp. 107.

On the Abolition of Restraint in the Treatment of Insanity, &c. By Dr. MOREL.

THIS small treatise will prove especially interesting to those concerned in the management of asylums in this country, and indeed to every philanthropist who has at heart the welfare of a class of afflicted beings, necessarily placed under disabilities, and unable to plead their own cause or secure their own well-being.

The treatise owes its origin to the commendable resolution of the Commission of the Asylum of St. Yon, at Rouen, to send the chief physician, Dr. Morel (so well-known for his original researches on the causes of degeneration of the human race), to England, to examine into the management of our asylums, particularly in reference to the abolition of mechanical restraint, and to report his observations. The result of M. Morel's inquiry is very flattering to English physicians. He is one of the first foreigners who seems to have been able to lay aside the prejudices prevalent among his countrymen on the question of restraint, and to appreciate the true nature of the system of "non-restraint." Although his visit to England was very short, he made good use of his time; stayed in asylums, and watched the details of management from day to day; questioning both their physicians and attendants with a view to elicit information. His knowledge of the language and of English works on insanity greatly facilitated his undertaking.

In the course of his remarks he reviews very fairly the objections, mostly of an *à priori* character, which have been urged against the non-restraint principle, and replies to them very satisfactorily. He presents a brief sketch of the organization of our asylums, and before concluding his work makes a gratifying revelation of the progress which Anglican opinion respecting the treatment of the insane is making in France. Lastly, were any additional arguments required against mechanical coercion and in favour of non-restraint needed, some very weighty ones might be drawn from M. Morel's statements, particularly those referring to the evils he has himself witnessed as a consequence of the use of mechanical restraint.

We are indeed very happy to reckon M. Morel among the converts to the modern system of treating the insane, and trust he may be a successful promoter of it in his own country.

PART THIRD.

Original Communications

Art. I.

On the Nature and Cause of the Respiratory Murmur. By Hyde Salter, M.D., F.R.S., F.R.C.P., Assistant-Physician to the Charing Cross Hospital.

(*Concluded from our last.*)

In a paper on the Nature and Seat of the Respiratory Murmur, which I published in the 'British and Foreign Medico-Chirurgical Review' for April last, I endeavoured to show—

That the immediate seat of the normal sound of breathing, from which it is conducted direct to the ear, is a thin sub-pleural stratum of lung-structure, consisting of air-cells and the minutest air-tubes:

That in health one hears no breathing-sounds going on in any part of the lung other than this superficial portion, on account of the feeble conducting power of lung-tissue:

That clinical observation tells us that breathing-sounds of a peculiar character are always present in every part of the bronchial system:

That this bronchial breath-sound furnishes a portion, probably the chief part, of the raw material of the breath-sound as it is rendered at the surface of the lung and thence conducted to the ear:

That there are many circumstances both in the physiology and physics of respiration that adequately explain the identity, and yet the difference, of these two sounds, and show how the one is modified into the other:

That therefore although the immediate seat of the breath-sound is in the sub-pleural air-cells and minutest bronchules, the seat of its primary generation is the entire bronchial system.

I also endeavoured to show the reason of the greater loudness of inspiration in vesicular breathing and of expiration in bronchial, and many other interesting points in connexion with the breath-sounds.

This inquiry into the nature and cause of the respiratory murmur would hardly be complete were I not to notice a little more at length those theories of its nature and production which I have already mentioned, and demonstrate their insufficiency and untenability. I have preferred stating and supporting my own thesis first, because it will enable me so much the more concisely to point out wherein I believe these different theories are erroneous; inasmuch as the arguments I have adduced in support of my own views will be, if considered conclusive, so many refutations of any views of a different nature; and I shall thus merely have to refer to them instead of re-stating them.

Dr. Sanderson is of opinion that the only seat of the respiratory murmur is the point at which the terminal bronchial tubes end in the air-cells. His views, as far as I understand them, may be thus stated. He holds,

That the lung consists of two kinds of structures, the expansible and the non-expansible; the expansible the air-cells, the non-expansible the entire bronchial system:

That the respiratory murmur must depend upon either air and tube-wall friction, or on the movement of the air upon itself; that the latter view is not tenable, and that the former must therefore be the cause:

That the seat of the respiratory murmur will be the point where this friction is greatest; in other words, where there is the largest amount of air-movement:

That this must be at the point where the non-expansible portion of the lung-structures joins the expansible; in other words, where the air-tubes end in the air-cells:

That if the air were an inelastic fluid, the greatest movement would be in the larger bronchi and windpipe, in consequence of the superficies of these tubes being less than the aggregate superficies of the bronchules in which they result; but the air being an elastic fluid, the law of "increased rapidity of stream with diminished capacity of channel" does not apply.

In an abstract of his opinions on this subject with which Dr. Sanderson has kindly furnished me, he thus explains the principles on which he arrives at his conclusions:

"At the end of the pause the air is motionless throughout the respiratory system, and its tension is zero. In inspiration the following changes take place:

"*a.* As regards tension.—An instantaneous diminution of tension is produced in the expansible portion (vesicles) of the respiratory system by the act of expansion. To restore equilibrium the tension is diminished in the inexpansible portion (bronchial tubes), but in a slighter degree, because in natural breathing, with open mouth or nostrils, the communication with the external atmosphere is so free that equilibrium is constantly maintained.

"*b.* As regards movement.—The first movement of air is from the inexpansible towards the expansible portion of the respiratory system, i.e., from the ultimate bronchial tubes into the air-cells. This movement, although more rapid at the first moment of the inspiratory act, is active during the whole of it. It is the commencement of a consecutive movement propagated in a direction from the smallest bronchial tubes *towards* the external orifices, i.e., in a direction opposite to that in which the movement itself takes place. Although *not* more rapid, it is larger than the movement that takes place in any other part of the respiratory system; that is, *more air passes from the bronchial tubes into the vesicles than enters by the mouth and nostrils.*

"The vesicular murmur is produced by this movement. Whenever air passes from a confined tube or cavity into a space of which the tension is less, a similar sound is produced.

"This movement may be called the *expansion current.* Its velocity varies—

"*Inversely*, as the quantity of air contained in the expansible portion of the lung—viz., the aggregate capacity of the vesicles;

"*Directly*, as the quantity of air inspired at each act of breathing."

While agreeing with Dr. Sanderson that the respiratory murmur depends on air- and wall-friction; that the seat of the loudest sound must be where this friction is greatest; that the air in the bronchial tubes is at one time in a state of *plus* tension, in another of *minus* tension; and that the elasticity of the air *does* modify its movement in the large and small tubes respectively, though not in the way he suggests; I object to his views, and the conclusions he draws from them, on the following grounds:

1st. That it is not correct to divide the lung into expansible and inexpansible structures—the air-cells and the air-tubes respectively; the air-tubes, especially the smaller ones, being remarkably extensible and elastic.

2ndly. That the elasticity of the air does not affect the law of the relation of rapidity of movement to calibre of tubes.

3rdly. That, in point of fact, the movement of air in the air-tubes is greatest at the windpipe, and diminishes continuously till it reaches its minimum in the ultimate bronchial tubes.

4thly. That air- and wall-friction does generate sound throughout the bronchial system, and that it is impossible but that this sound shall be conducted to the surface by those bronchial tubes that have a superficial distribution.

I admit at starting that there are some points in the physics of respiration, about which we have as yet no certain knowledge, that very materially affect this question.

One is the relative expansibility of the tubular and cellular portions of the lungs. The more the tubes expand at inspiration, the less exclusively is the twenty cubic inches of inspired air spent in the dilatation of the air cells; and the less considerable the dilatation and change of volume of these last, the less will be the movement of air in them and the terminal bronchules.

Another is the average volume of the ultimate groups of air-cells and the relation of that volume, and its change in respiration, to the calibre of the terminal bronchules. This points to the same as the last proposition—the rapidity of the movement of air at the point which Dr. Sanderson believes to be the exclusive seat of the respiratory murmur.

Another is the relative aggregate volume of the air in the air-tubes and the air in the air-cells at any one given time.

Another is the distance down the bronchial system that the twenty cubic inches of inspired air reaches.

Another is the law, with regard to increase of collective calibre, of bronchial branching—the ratio of increase. This would answer the last proposition: for, starting from the windpipe as a fixed point, we should soon find, if we knew the law of branching, at what point, at tubes of what calibre, the twenty cubic inches of inspired air would be used up.

Still, though these points are so uncertain, there is quite enough demonstrable and certain, in my opinion, to show that the limitation of the point of generation of the respiratory sound on which Dr. Sanderson insists, is not possible.

It seems to me that, granting the fact that the respiratory murmur depends upon the friction of air on tube or chamber wall, the questions to be answered are—"What is the seat of the greatest friction of air against the walls it rushes over?" and, "Does air-friction produce a sound in the entire bronchial system; and if so, what is to prevent its conduction to the superficies of the lung by the air in the bronchial ramifications?"

Dr. Sanderson admits that if the air were an inelastic fluid the whole of the traction exercised by the expansion of all the air-cells upon the air contained in all the ultimate bronchules would be transmitted undiminished to the windpipe, the single tube of supply, and produce there a much greater movement of air than that in any single ultimate bronchial tube—a movement greater by as much as the aggregate calibre of all the ultimate bronchules taken together is greater than the calibre of the windpipe. But, he says, the air is *not* an inelastic fluid, it is highly elastic, and the expansion of the air-cells is spent in diminishing the tension of the air in the peripheral portions of the bronchial system, so that the movement of the air in the ultimate bronchules is not transmitted to the larger bronchi and windpipe, or in a very diminished way, and thus that there is not that rapid current of air in the larger air-passages that there is stated to be, and that there otherwise would be. Now, if this were the case, if the movement of the air in the recesses of the bronchial system were thus lost by air-expansion from diminished tension, there would be no correspondence between the movement of air in the windpipe and that at the thoracic parietes. Whereas there is an almost exact correspondence. The expiratory and inspiratory sounds of tracheal breathing are just appreciably behind the movements of the thoracic parietes, and only just, and are slightly modified in the way and by the means I have already mentioned in my former communication. But if we listen carefully over the windpipe we find that the inspiratory tracheal sound commences almost at the moment the thoracic parietes rise, is coincident with their movement, and ceases with it: the same at expiration; and between expiration and the next inspiration there is perfect quietness in the trachea, coinciding with the quiescence of the thoracic parietes. The same thing may be shown by the movement of air at the mouth or nostrils. It is manifest that the chink of the glottis being permanently and naturally open, and the air in the chest in communication with the external atmosphere, the air in any part of the air-tubes or at the orifices could not arrive at a state of rest until an equilibrium of tension is established: how soon such an equilibrium of tension *is* established is shown by the instantaneous rest of the air at windpipe, glottis, or nostril, that accompanies a sudden cessation of respiratory movement.

No doubt the first effect of the movement of the boundaries of the chest is to produce a state of minus tension of the intra-thoracic air at inspiration, and of plus tension at expiration; it is by this varying tension that the air in the upper parts of the bronchial system and at the orifices is moved; but the propagation is so instantaneous that the question of time may be ignored and the air regarded as if it were inelastic. The same amount of air passes in and out of the windpipe at each inspiration and expiration as if it were absolutely inelastic. I can, in whistling, repeat a note six times in a second by six short equidistant expiratory jerks of the parietes of my chest. How could I do this if the movement of the air at the orifice of my mouth were not exactly regulable by the thoracic parietes—if the movement of the two did not coincide all but absolutely? How but for this could we ever have heard Bosio's execution of Rossini's running passages—each note isolated from its neighbour by a momentary cessation of respiratory movement?

If, then, the movement imparted to the air in the recesses of the lungs is so immediately transmitted to that at the orifices, Dr. Sanderson's theory, in principle strictly true, as far as the question of varying tension goes, in no way interferes with the law that would multiply the rapidity of the movement of the air in the windpipe as compared with that in the ultimate bronchiæ, by as much as the aggregate calibre of all these tubules exceeds that of the windpipe. The other part of his views—viz., *that more air passes from the bronchial tubes into the vesicles than enters by the mouth and nostrils*, must necessarily be erroneous, as the quantities must be exactly the same.

We might arrive at the same conclusion another way. The quantity of air inspired at each inspiration is twenty cubic inches or thereabouts; I have ascertained that the cubic contents of the windpipe are *six* fluid drachms, or one and a half cubic inches; at each inspiration and expiration the air in the windpipe is therefore renewed fourteen times. The windpipe is three inches and a half long, therefore the air moves in the windpipe at the rate of fourteen times three and a half inches—i.e., forty-nine inches, each expiration and inspiration; inspiration occupying about a second and a half, and expiration less than a second. The air moves, therefore, in the windpipe at expiration at the rate of forty-nine inches—i.e., four feet—in a second. Can it be for a moment supposed that the movement of the air at expiration in the ultimate bronchules, the movement with which it starts from a state of rest in the air-cells by the gentle collapse of the lung, is four feet a second? Such a rate of movement would carry the air from the air-cells to the orifice of the windpipe before expiration was half complete.

I think, from what I have said, that it is abundantly clear that the termination of the bronchiæ in the air-cells is not the seat of the greatest movement of air, and therefore, according to Dr. Sanderson's own postulate, not the especial seat of the respiratory murmur.

I now come to the second question, "Does air-friction produce a sound in the entire bronchial system; and if so, what is to prevent its conduction to the superficies by the bronchial ramifications?" and for the answer to this question I must refer the reader to its full discussion in the preceding communication already referred to. Clinical evidence shows us that the bronchial tubes are everywhere the seat of sound, and we know that the vibrations of the voice, cough, or glottic breathing, are audible at the thoracic parietes: to suppose, then, that the bronchial sound should not be audible there, would involve a suspension of physical laws.

Dr. Sanderson's theory is in part built on the supposition that the lung may be divided into an extensible and an inextensible portion; that the air-cells are elastic and the air-tubes not. Now I am sure this is not true: I do not mean to say that the air-cells are not greatly more extensible than the tubes, though how much we do not know; but it is impossible to examine the smaller tubes minutely without seeing that they are in a high degree elastic and extensible. And remembering their constitution, we should antecedently conclude that they would be so, for their walls are largely endowed with yellow elastic fibres; indeed, the elastic coat is, with the exception of the epithelial, the only one that is continued down to the very extremities of the bronchial ramifications; so that the smallest tubes may be said to consist entirely of elastic tissue with a lining of a single layer of epithelium, and surrounded with the smallest possible quantity of connective.

Again, it is impossible to imagine the air-cells dilating without the bronchial tubes dilating too. What is it that surrounds the bronchial tubes? The parenchyma of the lung—air-cells. If we make a section of a small bronchial tube, we shall find it imbedded among air-cells, which are in contact, but not communicating, with its outer wall, just as the branches of the portal vein course their way among the lobules of the liver. Now, is it possible to imagine that these air-cells can dilate without the channel that they surround dilating also? Let A (Fig. 4) be a small bronchial tube seen in section, and B, B, B, air-cells in contact with its outer wall; the expansion of these air-cells is manifestly impossible without increasing the calibre of the cylindrical channel which they bound, unless we suppose them to expand in one direction only—that is, in a radiating direction, and not laterally, which would be gratuitous and unreasonable.

Or perhaps this may be more clear thus. Let *a* (Fig. 5) be a circle formed by twelve circles, *b b b*, each of them three-quarters of an inch

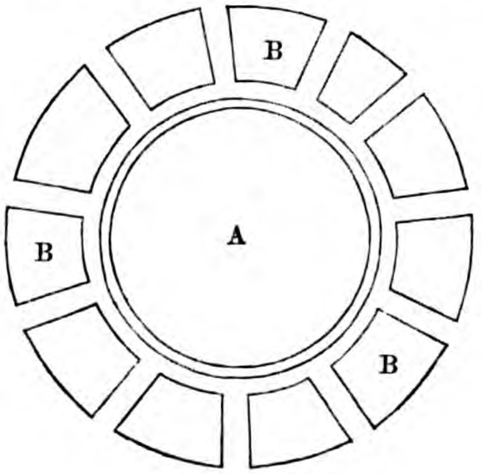

Fig. 4.

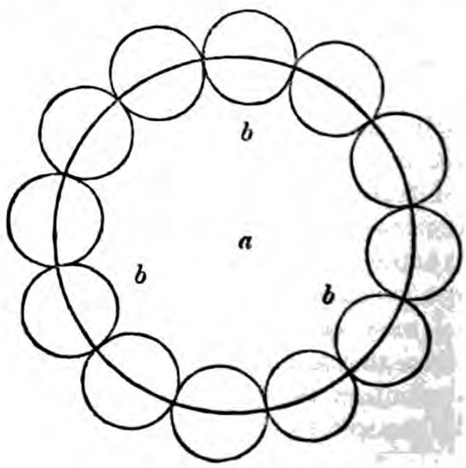

Fig. 5.

in diameter. The circumference of the circle will be nine inches. But if the smaller circles undergo an expansion of one-third of their diameter, so that instead of three-quarters of an inch, the diameter of each is an inch, the diameter of the whole circle which they form will have increased from nine inches to twelve. So that it is impossible for the air-cells to expand without the tubes which are everywhere surrounded with them expanding too, unless we suppose a vacuum to be formed; for the thin areolar web surrounding each tube, although capable of extension in any one direction, is incapable of occupying any additional space. It seems to me impossible to help believing that the air-tubes *do* in this passive manner expand, the smaller ones considerably, at each inspiration.

But above all, in proof that the calibre of the bronchial tubes is increased at inspiration and diminished at expiration, we have the capital fact—a fact that I have adverted to in my work on asthma—that one constantly finds dry musical sounds at expiration which are inaudible at inspiration; in fact, that dry sounds are expiration sounds, and that it is the common thing to find them confined to expiration. Now, how it is possible to explain this, except by supposing that the air-tubes undergo a diminution of calibre at each expiration, I cannot imagine. On this supposition it is quite intelligible. For suppose, which is often the case, the source of the sound is a plug of tenacious mucus sticking to the side of the tube. At inspiration, when the tube is expanded in common with the other tissues of the lung, the plug of mucus does not sufficiently obstruct the air to throw it into musical vibrations; but at expiration the elastic tube, no longer expanded, is left to its own natural disposition to contract, and the plug of mucus which in the patulous state of the tube was not equal to the production of any considerable narrowing, is now sufficient to produce a very narrow strait, perhaps almost a complete occlusion, and musical sounds are immediately generated. The mucus plug is a constant, it cannot vary at each inspiration and expiration, and yet the conditions for the generation of the sound *do* vary, for the sound varies; and this variation can only be produced, as I conceive, by variations in the calibre of the tube—contraction at expiration—so that what was a relatively small becomes a relatively great narrowing. If my explanation is not correct I should be glad to hear some other offered; of the fact there can be no doubt. If my explanation *is* correct, it is a proof that the tubes *do* undergo expansion and contraction at each respiration, like the cellular structure of the lungs.

There is another fact that in my opinion tends strongly to show that the bronchial sound furnishes a good deal of the material of the respiratory murmur—it is the inaudibleness of the murmur at expiration. If Dr. Sanderson's view were correct, the respiratory murmur ought to be as loud at expiration as inspiration: the same amount of air rushing out of the same orifice, close to the surface, in the same time, or even less time, ought surely to make as much sound as when it rushed in. If, however, we imagine the staple of the sound to be furnished from a distance, convexion comes into operation, and its audibleness will depend on the "set" of the air-current. If a bell is rung close to the ear, it makes no difference which way the wind blows, but if it is rung at a distance, we shall hear it plainly if the wind blows towards us, while when the wind blows away it may be quite inaudible. So, the fact that the direction the air is taking in the bronchial tubes determines the audibleness or inaudibleness of the respiratory murmur, shows that the sound is influencible by convexion —in other words, has a distant source, and is not generated at the superficies.

I cannot therefore subscribe to Dr. Sanderson's views:

Because the air-rush, which Dr. Sanderson admits to be the cause of the sound, is at its minimum at the very part which he claims as the seat of the respiratory murmur:

Because it is impossible to conceive but that the sound generated in the entire system of air-passages must be conducted to the surface by the air in the bronchial ramifications:

Because the alternate audibleness and inaudibleness of the respiratory murmur shows it to have such a seat that its conveyance to the ear is influenced by the direction the air is taking in the bronchial tubes.

I pass now to the consideration of another set of views—those of Spittal and Beau—who transfer the seat of the respiratory murmur to the very opposite end of the air-passages, M. Beau believing that it depends on the rush of air through the pharynx, and Dr. Spittal that the chink of the glottis is its seat.

Now, with regard to the pharyngeal and glottic breath sounds, I believe that any one may, in his own person, satisfy himself of two things:

1st. That there really are such sounds.

2ndly. That they have nothing to do with the production of the ordinary respiratory murmur.

With regard to the so-called pharyngeal sound, I must premise that I do not believe it is produced in the pharynx at all, but at the isthmus faucium, by dropping the soft palate upon the root of the tongue, and so producing a narrow horizontal chink; through this the air rushes and generates the sound in question; it is of a thick, faucial, guttural character, and is the sound that marks the first heavy breathing of sleep before snoring comes on. The glottic sound is produced by the voluntary partial closure of the glottis, so narrowing the chink that the air rushes through it; it is the laryngeal element of whispering, and we hear it well in the breathing of chronic laryngitis. Each of these sounds may be produced at pleasure; we know when we are making them and when we are not; and if we listen to the chest of any one who is thus voluntarily making and unmaking them, we can tell in a moment, as I have already stated, when they are being produced and when remitted. With regard to the faucial sound, I may remark that it can never be made with the mouth shut —the glottic sound can; the opening or closure of the mouth makes no difference to it.

How, then, can these sounds have anything to do with the respiratory murmur? We cannot make and unmake it; we cannot vary it at will; we cannot affect it by anything except by the length and strength of the respiratory acts.

Dr. Walshe and Dr. Herbert Davies have attributed the excess of the inspiratory over the expiratory sound to a greater resistance offered to the air by the lung tissues in inspiration than expiration. I must confess I am quite at a loss to comprehend the possibility of such an increased resistance. It is in consequence of the increased capacity of the air chambers and channels of the lung that the tension of the intra-thoracic air is diminished, and the inspiratory current generated. The atmospheric pressure, balanced during a state of respiratory rest by the resistance of the walls of the air-tubes and cells, is no longer balanced; the pressure of the walls is temporarily taken off the air by their dilatation, and a state of *minus* tension is produced. It seems to me, then, that there must not only be not more, but less resistance to the air at inspiration than at expiration, and that this lessened resistance is the very cause of inspiration.

Dr. Herbert Davies specifies that it is the elastic contractility of the lung that offers greater resistance to the inspired air than the expired. This seems to me to imply the idea that the contractility of the lung is overcome by the distending force of the inspired air, instead of the air being drawn in in consequence of the contractile tendency of the lung being overcome by the expanding parieties of the chest. If the contractility of the lung were not, for the time, overcome, and more than overcome, no air could be drawn in. We may regard, then, the contractility of the lung at inspiration as potentially *nil*. It is to the parieties of the thoracic cavity, and to them alone, that the elasticity of the lung offers any antagonism during inspiration; and it is at expiration only, when the muscular distension of the chest ceases, that the elastic resiliency of the lung is allowed to reassert itself, and the elastic lung-tissues react upon the intra-thoracic air. The resistance, therefore, offered to the air by the elasticity of the lung is an expiration and not an inspiration resistance.

In the opinion of Dr. Herbert Davies the inequalities of the internal surface, produced by the cartilaginous rings of the windpipe and bronchi, tend to throw the air into vibrations, and contribute to the respiratory murmur. I think it has yet to be shown that the rings of the windpipe and bronchi do produce any inequalities of surface. For my part I have never been able to see it; it has always appeared to me that the cylindrical smoothness of the internal surface is not in the slightest degree broken or interfered with by them. Indeed, to suppose that it would be, would be to suppose that Nature had been clumsy, and that she had thrown unnecessary obstruction in the way of that free flow of air into and out of the lungs, which is the essential condition of healthy breathing, and which all the other arrangements of respiration conspire to favour.

Art. II.

A Statistical Account of Four Hundred Cases of Acute Rheumatism admitted into the Wards of the Middlesex Hospital during the Years 1853–59. By George William Fleetwood Bury, Esq., Fellow of the Royal College of Surgeons, late Registrar to the Hospital.

Four hundred and seventy-six cases of acute fibrous rheumatism were admitted into the wards of the Middlesex Hospital during the six years commencing from 1853.

The following table exhibits the number of cases admitted, and the nature of the complication which occurred among them:

Disease.	Sex.	No. of cases.	Total.
Acute rheumatism, uncomplicated	Males ...	140	223
" " "	Females ...	83	
" " with endocarditis	Males ...	56	138
" " " "	Females ...	82	
" " with pericarditis	Males ...	13	35
" " " "	Females ...	22	
" " with endo-pericarditis ...	Males ...	42	71
" " " " ...	Females ...	29	
" " with endo-pericarditis and pleurisy	Males ...	5	9
" " " " " ...	Females ...	4	
Total			476

Or, briefly,—

	Males.	Females.	Total.
Acute rheumatism, uncomplicated	140 ...	83 ...	223
" " with heart affection ...	116 ...	137 ...	253
Total	256	220	476

It is here necessary to state that under the different heads of heart complication are included only those instances of cardiac mischief which were considered either to have originated with the present rheumatic attack, or those in which, during the present attack, new cardiac disease had been grafted on old structural and rheumatic changes. No example, therefore, of simply old heart disease—the result of previous rheumatic attacks—has been entered as a complication.

From the tables we find that more than one half, or about 53·7 per cent. of those attacked with the disease suffered from heart complication of one kind or another, and that women suffered in a much greater proportion than men. In the latter, the per-centage was 45·3; in the former, 62·2.

Endocarditis has been by far the most common form of heart complication, and its combination with pericarditis stands next in order of frequency. Pericarditis without endocarditis was present in only thirty-five cases, whilst nine cases alone are recorded of endo- or pericarditis with lung affection. The relative frequency of occurrence was nearly as follows:—Pericarditis four times as frequent as endo- or pericarditis with pleurisy; endo-pericarditis twice as often as pericarditis, and endocarditis twice as frequent as endo-pericarditis;—endocarditis, therefore, being four times as frequent as pericarditis.

Hereditary Tendency.—The transmission from parent to child of the tendency to rheumatism is a matter so generally regarded as an established fact, that it is interesting to inquire how far statistics corroborate this general belief.

Of the 476 cases under observation, we find that 67, or rather more than fourteen per cent., had relatives subject to rheumatism, and in the following rate of frequency:

	In 28	instances	the	father.
	„ 17	„	„	mother.
	„ 2	„	„	father and mother.
	„ 3	„	„	grandfather.
	„ 9	„	„	brother.
	„ 7	„	„	sister.
	„ 1	„	„	relative not stated.
Total . .	67			

It is worthy of note that the predisposition seems to be much more frequently inherited from the father than from the mother, and its probable derivation from both is of comparatively rare occurrence.

The relative frequency of Acute Rheumatism, and also of its Heart Complications, at various ages.

Age.	Number of cases.	Per-centage for total number of people living at same age in England.	No. in which heart was affected.	Per-centage.
Under 10 years	9	1·89	6	66· 6
Between 10 and 15 years	51	10·71	31	60· 8
„ 15 and 20 „	124	26·05	85	68· 5
„ 20 and 25 „	103	21·06	59	57·28
„ 25 and 30 „	76	15·90	31	40· 8
„ 30 and 35 „	36	7·56	19	52· 8
„ 35 and 40 „	29	6·13	8	27·58
„ 40 and 45 „	23	4·83	7	30·43
„ 45 and 50 „	15	3·16	5	33· 3
„ 50 and 55 „	6	1·26	2	33· 3
„ 55 and 60 „	3	·63	...	...
„ 60 and 65 „	1	·21	...	...
Total . . .	476	...	253	

Joints affected.—It would seem that not only are the distal joints of a limb those most liable to be first attacked, but that there exists a tendency, so great as almost to amount to a rule, for the disease to commence in the lower extremity and travel upwards with a certain regularity. From the foot—the part first selected—the disease extends to the ankle, and then severally in order to the knees, hips, hands, wrists, elbows, and shoulders; the hips, however, not infrequently escaping. Whence this preference for its commencement in the remote joints it is hard to conceive, though a partial explanation of the fact may, perhaps, be afforded in the circumstance of the greater exposure of those parts to the exciting causes generally supposed to be productive of the malady.

In order to simplify the tables as much as possible, we have not particularized the frequency with which each joint has been affected, but have massed our facts under the heads of "Joints of the upper," "lower," and "upper and lower extremities." Curiously enough, the joints of the lower extremities alone have been the seat of the disease exactly twice as often as have those of the upper extremities alone. Again, the number of instances in which the joints of both extremities have been attacked is more than double the united number of the cases in which the disease appeared in the articulations of either the upper or lower limbs singly. This latter fact speaks strongly for the erratic and diffusive tendency of the disease.

The following is the table :—

Joints.	Males.	Females.	Total.
Upper extremities ...	26 ...	25 ...	51
Lower extremities ...	53 ...	49 ...	102
Both extremities ...	177 ...	146 ...	323
Total ..	256	220	476

Duration of attack.—Though our records, from obvious reasons, do not afford us any precise information on this point—one, it is imagined, of the least importance in the consideration of the disease—yet it is believed that the average length of time patients attacked with acute rheumatism remained in the hospital comes to much the same thing. For while, on the one hand, must be allowed the period of illness which elapsed previous to the patient's admission; still, on the other, must be deducted the time the patient remained after all rheumatic symptoms had disappeared, and during which he only lingered to regain sufficient strength to justify his leaving.

These two periods, it is believed, would mutually compensate, and the average length of stay—forty-nine days—of rheumatic patients, may be considered as fairly representing the duration of an attack of acute rheumatism.

Period of occurrence of heart complications.—The following is a table embodying the results of our inquiries on this point. It is almost superfluous to state that the first day on which any distinctly abnormal sound was heard has been noted as the advent of the complication :—

Day.	No. of cases.	Day.	No. of cases.
1st	... ...	Brought forward	74
2nd	... 3	15th	... 3
3rd	... 1	16th	... 1
4th	... 4	17th	... 5
5th	... 10	18th	... 2
6th	... 8	19th	... 2
7th	... 7	20th	... 5
8th	... 12	21st	... 2
9th	... 7	22nd	... 3
10th	... 2	23rd	... 1
11th	... 9	24th	... ...
12th	... 3	25th	... 4
13th	... 7	26th	... ...
14th	... 1	27th	... 1
Carried over	74	Total	103

Owing to the fact that the greater proportion of the patients attacked with heart symptoms—and this is an additional proof of their early occurrence—were admitted subsequently to the supervention of the cardiac mischief, the exact date of their appearance in those instances is unknown. From the remaining number (103), from the details of whose cases the table has been compiled, it is ascertained that cardiac complications may occur as early as the second, and as late as the twenty-seventh day of the disease, and also that during the first thirteen days the liability to them is at a maximum. After that period till the twenty-seventh (the latest day on which cardiac symptoms have been noted as commencing), there is a decided decline. The fifth and the eighth are the days on which the complications showed themselves most frequently though not by any great numerical excess.

In regard to the relative period of occurrence of endocarditis and pericarditis, there seems to be no fixed rule. Some conclusions may be drawn, perhaps, from the respective dates of their appearance when existing conjointly; and in endo-pericarditis it is found that endocarditis generally preceded pericarditis by a day or two at the least.

Previous Attacks.—Of the 476 cases of acute rheumatism here collected, no less than 207 (or nearly one-half) had suffered from previous attacks, and of these, 136 had experienced but one.

The following table exhibits in detail the results of the inquiries on this head. Under the term "several attacks" have been included all the cases of patients who were unable to state precisely the number of their former seizures:

Number of attacks.	Males.	Females.	Total.
One	75	... 61	... 136
Two	16	... 9	... 25
Three	15	... 2	... 17
Four	6	... 2	... 8
Five	1	... ...	... 1
Six	1	... ...	... 1
Nine	1	... ...	... 1
Ten	1	... ...	... 1
Several	12	... 5	... 17
	128	... 79	... 207

Period of the Year during which Acute Rheumatism is most prevalent.—Owing to the difficulty that exists of ascertaining, in numerous instances, the exact date of the commencement of the attack, we have, in the table appended, employed the month of the patient's admission into the hospital. As this period doubtless coincides, in at least half the cases, with the one of attack, and as the plan mentioned above has been invariably followed from the first, the results, it is imagined, will in the main be correct.

It will be observed that during the last six months of the years from 1853 to 1858 inclusive, the admissions have exceeded those of the first six by sixty-four. This excess, however, is not sufficiently great to warrant our drawing the inference that acute rheumatism is a disease of the latter half of the year.

Again, the anomalies presented by the months prevent our making any more satisfactory deductions from them. For instance, it is hard to conceive why June, a month of comparative dryness and equable temperature, and apparently so unfavourable to the production of the disease, should be signalized by a majority of cases; the more especially as it presents so few points of analogy to October, during which the admissions have been numerically next in order. The admissions have been as under:

January	32
February	23
March	31
April	28
May	33
June	65
July	37
August	37
September	43
October	56
November	44
December	47
Total	476

Art. III.

On Sexual Limitation in Hereditary Disease. By William Sedgwick.

(Concluded from our last.)

From hereditary diseases of the organ of vision, the transition is easy to those affecting the organ of hearing, for there are some defects which these organs seem, as it were, to share in common. This connexion has been already referred to by some writers, amongst whom Mr. White Cooper* states that imperfection of the two senses (of sight and hearing) not infrequently co-exist, especially in the curious class of cases we have just been considering, where the inability to distinguish colours is often associated with a corresponding inability to distinguish musical sounds. Dr. Earle relates, in his case of colour-blindness, that "the whole family, of which the chart has been exhibited, is

* Cyclopædia of Anatomy and Physiology, art. "Vision," p. 1453.

probably no less generally characterized by a defective musical ear than an imperfect appreciation of colours. Several of the individuals comprised in it are utterly incapable of distinguishing one tune from another."*

In some cases the difficulty of appreciating sounds is more limited in its range than this; for example, Dr. Wollaston, in an interesting paper, "On Sounds Inaudible by certain Ears,"† refers to the case of a friend, "whose sense of hearing terminated at a note four octaves above the middle E of the pianoforte." And just in the same way as in colour-blindness, the limitation of the defect may occur in either sex; so in cases of cricket-deafness, as it has been termed, a corresponding limitation may prevail; for whilst in Dr. Earle's case, referred to above, the defect was restricted to the male sex, in one of the cases related by Dr. Wollaston, "a female relative of his own was unable to distinguish the chirping of a field cricket, and a sister of the last person had the [same] peculiarity."

It has been suggested by Dr. Wollaston that human hearing "has probably, in every instance, some definite limit, at no great distance beyond the sounds ordinarily heard;" and most observers are aware that many individuals are deaf to the sounds emitted by some of the lower animals—such, for instance, as the sparrow, the cricket, the bat, &c. But as the range of human hearing includes more than nine octaves, there are difficulties of observation with regard to such defects in the sense of hearing which prevent any fulness of detail in the history of these cases, especially when only one ear is affected, as occurs in Sir David Brewster,‡ who suffers from cricket-deafness of the left ear; and I must therefore pass on to the consideration of those defects which are more easily recognised, among which congenital deafness, as occurs in the case of the deaf and dumb, will be first noticed.

I have at the present time under observation the family of a respectable carpenter in Paddington, who is the father of six children, three boys and three girls; the three boys have no imperfection, either in speech or hearing, whilst all the three girls are deaf and dumb. One of these daughters, who is now an intelligent young woman of seventeen years, is apprenticed to a dressmaker in my neighbourhood, and shows great quickness in learning the business. Neither the parents nor any of the immediate relations, so far as can be ascertained, have any defect of hearing.

In one of the cases recorded by Mr. Wilde,§ a corresponding limitation of the defect to the female sex prevailed. "In a family of thirteen, in the county of Sligo, mute twins occurred twice, being the seventh and eighth births: in the former, both children were mute females; in the latter, a male and female, the boy not mute. Of the

* American Journal of Medical Science, vol. xxxv. p. 347. 1845.

† Philosophical Transactions, Part 2, pp. 306–314, 1820; and Tillock's Philosophical Magazine, vol. lvii. pp. 187–192. 1821.

‡ Tillock's Philosophical Magazine, vol. xxv. p. 136. 1806.

§ The Census in Ireland for the year 1851, part 3, Report on the Status of Disease, p. 14. Dublin, 1854.

entire thirteen births in that family, five were males, none of whom presented any defect; and eight females, of whom seven were deaf and dumb; the order of the births of the mutes being the third, fourth, fifth, eighth, ninth, and eleventh."

These cases possess additional interest from the fact, that if there is any sexual preference in this congenital defect, it is for males rather than females; for, according to Mr. Wilde,* the proportion of sexes of the deaf and dumb in England and Wales, where 1 in 1738 of the inhabitants are affected, is 100 males to 82·9 females; in Scotland, where 1 in 1340 are affected, 100 males to 80·0 females; and in Ireland, where 1 in 1380 are affected, 100 males to 74·5 females. And this agrees with M. Boudin's statistics of the deaf and dumb in Prussia,† where 1 in 1360 of the inhabitants are affected, the proportion of sexes being 100 males to 78 females.

It is well known that where one member of a family is born deaf, several subsequent members of the same family are often similarly affected. Thus, Sir Henry Holland‡ relates that five children in a family of eight, and four children in a family of seven, were born deaf and dumb, without any corresponding defect in the parents; and sometimes the births of such children occur in a regular series of alternations with those of children who can hear. A physician at Marseilles relates a case,§ in which deafness from birth occurred in three children alternately in a family of six; the parents were not affected. M. Saissy‖ refers to a family "living at Aix, in Savoy, composed of seven children; the eldest is deaf and dumb, the second hears perfectly, the third is deaf and dumb, the fourth enjoys the same advantage as the second, the fifth, sixth, and last are completely deaf. The last but one (the sixth) in this case being an idiot. There is no defect in either of the parents." A similar case¶ occurs in the commune of Bessenay, department of the Rhone; in a family composed of eight children, four are deaf and dumb, and alternate with four who enjoy the sense of hearing. And M. Saissy states that these observations might be still more multiplied.**

Numerous instances might also be referred to where not only several members of the same family are born deaf, but where cousins have the same defect. Mr. Wilde, in his valuable report on the deaf and dumb in Ireland, informs us that—

"In cases of single congenital mutism, where the relations were also deaf and dumb, there were, by the father's side . . . . in one instance, six cousins affected; in three cases there were four cousins, in eleven instances three cousins, and in

* Op. cit., p. 9. † Annales d'Hygiène Publique, tom. xlix. p. 129. 1853.

‡ Op. cit., p. 33.

§ Dictionnaire des Sciences Médicales, art. "Oreille" (Saissy), tom. xxxviii. pp. 38–9, 1819; et Essai sur les Maladies de l'Oreille interne, par J. A. Saissy, pp. 20–1. Paris, 1827.

‖ Op. cit., p. 21, note. ¶ Ibid.

** The same phenomenon has been observed in many other diseases. It occurred in a case of sexual limitation of hereditary cerebral disease in my own practice, to which I shall again refer; but perhaps the most curious instance on record is the celebrated case of dwarfs related by Clauder (Les Ephémérides des curieux de la Nature, Dec. 2, an 8, p. 543, 1689, Nanorum Generatio), of a woman who gave birth to eight children of one and the other sex, the first, third, fifth, and seventh of whom attained the ordinary size, whilst the other four were dwarfs.

nine cases two cousins, all deaf mutes. Where two of the family were affected with congenital deaf dumbness, in two instances four cousins were in a similar condition; in two cases three cousins were deaf and dumb; and in four instances two cousins were thus afflicted. Where three cases of congenital deaf-dumbness occurred in the same family . . . . in five cases two cousins were in a similar condition. Where the relationship came by the mother's side, there were in cases of single congenital mutism eight cases where three cousins were deaf and dumb, and fourteen instances were two cousins were thus affected. In the case of two mutes in the same family, we find that . . . . in one case three cousins, and in four cases two cousins laboured under the like defect; and . . . . where three mutes occurred in the same family . . . . two cases presented of two cousins also deaf and dumb." (pp. 18, 19.)

Whilst such is the case with regard to cousins, it might be expected that other relations would be in many cases similarly affected, and this has occasionally been noticed. Several cases might be cited where an uncle or aunt, a grand-uncle or grand-aunt, nephews, nieces, or other collateral relations, have also been deaf mutes. But it is rare to find instances of the immediate parents being deaf and dumb, for even with the aid of "breeding in," this defect fails as a rule to show itself in the direct line of descent; and among the numerous examples on record of intermarriage between deaf mutes, a very small percentage of the resulting offspring share their parents' defect. All statistical reports on the deaf and dumb agree in confirming this remarkable fact. For instance: "Of one hundred and forty-eight scholars at one time on the foundation of the Deaf and Dumb Institution in London, one was of a family in which there were five deaf and dumb; one in which there were four; eleven in which there were three; and nineteen in which there were two . . . . and none of them were the children of deaf and dumb parents." The subsequent history of the scholars showed "that some of them were married and had children, all of whom were perfect in the organ of hearing. One instance occurred in which both parents had been born deaf, yet the children possessed the faculty of hearing."*

"At the School for the Deaf and Dumb in Manchester, in 1837, there were forty-eight children taken from seventeen families . . . . an average of nearly three such cases in each family. Out of these instances there appears but one in which the defect was known to exist in either parent."†

Mr. Wilde, whose observations included the whole of the deaf and dumb population in Ireland, states that "ninety-eight deaf and dumb persons—sixty males and thirty-eight females . . . . were married. In eighty-six instances—fifty-four males and thirty-two females—only one patient was deaf and dumb; from the marriage of these, two hundred and three children resulted, among whom there was but one instance of mutism, a male, in the county of Limerick. Six instances have been recorded of the intermarriage of deaf and dumb persons;

* A Treatise on the supposed Hereditary Properties of Disease, by Joseph Adams, M.D., p. 86; quoted by Dr. Joseph Brown: Cyclopædia of Medicine, art. "Hereditary Transmission of Disease."

† Medical Notes and Reflections, by Sir Henry Holland, third edition, p. 24. 1855.

their offspring amounted to thirteen, of whom only one, a female, in the City of Dublin, was deaf and dumb.*

Lastly, in the Thirty-fifth Annual Report of the Asylum in Hertford (U.S.), we find that one hundred and three of the deaf and dumb had been, or are now, married. In forty-one of these marriages both parties were deaf and dumb; in twenty-three, one could hear or speak. Of these one hundred and three, thirty-one had not become parents, but the remaining seventy-two were the parents of one hundred and two children, of whom ninety-eight could hear and speak, and four only were deaf and dumb. One of the four was the only child of his parents, both of whom were congenitally deaf. Besides the parents, the paternal grandfather, a sister of the father, and two sons of this sister, were deaf and dumb. In the other family, that of the three children, the father lost his hearing by disease at two years of age, and had no known relative deaf and dumb. The mother was born deaf, and had a deaf and dumb brother." †

As the deafness in the father of this family of three deaf mutes did not occur till the age of two years, the above case is a doubtful instance of the direct inheritance of congenital mutism, for deafness sometimes appears to be more readily inherited when it occurs at a period subsequent to birth. Thus Dr. Fosbroke‡ relates that "a lady, a friend of the late Colonel James Smith, of Cheltenham, fell out of a window at Norwich, in a state of pregnancy, and instantly became deaf in one ear. The child produced by this pregnancy was born deaf in the corresponding ear;" and M. Saissy§ records the case of a man who became deaf at the age of forty years. "His paternal grandfather, his father, and two of his brothers, have at the same age experienced the same defect."

There are, however, occasional instances in which congenital deafness has been directly transmitted from parent to offspring, for, besides the cases referred to above, I may cite the well known case of Mr. Bass, of Peterborough,‖ where congenital deafness occurred in three generations out of four. But such cases are decidedly exceptional, and we are quite justified in assuming that deaf-dumbness is not hereditary in the common acceptation of the term, but only in that wider sense in which we apply it, in consequence of several members of the same family, their cousins, or other collateral branches, being similarly affected. The cause of this exceptional phenomenon in the deaf and dumb cannot be satisfactorily accounted for, and Mr. Wilde, in referring to it, merely states that it is owing to "some unexplained family peculiarity."

In connexion with this fact it must be remembered that in some cases of congenital mutism the defect may be referred to the evil consequences resulting from intermarriage between those who are near

* The Census of Ireland, p. 13.

† Boston Medical Journal, July, 1851.

‡ Lancet, vol. i. p. 740. 1830.

§ Dict. des Sc. Méd., tom. xxxviii. p. 38, art. "Oreille," 1819; et Essai sur les Maladies d'Oreille interne, p. 20. 1827.

‖ Adams, op. cit., p. 19 and 124; quoted by Dr. Joseph Brown.

akin. Mr. Wilde, in his report on the deaf and dumb in Ireland (p. 17), and Mr. Adams in his work already referred to (p. 124), have furnished some valuable information on this subject. Dr. Bemis, of Kentucky,* who has lately published a work on 'Intermarriage between Cousins,' shows that defects both of sight and hearing are among the natural consequences of such alliances in families. From statistical reports obtained from the different hospitals in the United States, he ascertained that of one hundred deaf and dumb, ten were the offspring of such intermarriage; and of one hundred blind, five belonged to the same class. Dr. Chazarain,† a young physician of Bordeaux, has written a very able thesis on the same subject, which contains numerous observations on the influence of consanguinity on deaf-dumbness. It appears that in the Deaf and Dumb Institution at Bordeaux, of thirty-nine boys, deaf and dumb, six were the offspring of such marriages; and of these six, one boy had two brothers deaf and dumb, and one boy had three brothers deaf and dumb, making a total of eleven. Of twenty-seven girls in the same institution, nine were the issue of such marriages, and of this number six had between them seven brothers and sisters similarly affected, making a total of sixteen; and very lately M. Devay,‡ Professor of Clinical Medicine at Lyons, has brought the same subject before the notice of the Imperial Academy of Sciences, &c., in that city; for to so great an extent has the evil prevailed, that in one of the departments of France (*Ariéges*) the clergy have endeavoured to check the frequency of such marriages, and have appealed to the authorities at Montpellier to aid them in so doing.§

But even after deducting such cases, there still remains a large number of hereditary cases of deaf-dumbness for which no sufficient explanation has been, or can be as yet, offered, for the subject is surrounded with many difficulties, such, for example, as occur in the curious case recorded by Mr. Wilde,‖ of a man in Waterford who had two deaf and dumb illegitimate children by two different females; all his legitimate family being unaffected. I must, therefore, restrict myself to remarking that the above statements contribute to establish an important fact—namely, that its appearance in the direct line may be the rule in some hereditary diseases, and the exception in others, and that, as regards the latter class of cases, the popular notion respecting hereditary disease is only true in a very limited sense; for in a large majority of the cases it would be almost useless to look for any existence of the same disease in the parents, owing possibly to the disease in the offspring being the expression in them of some unrecognised and abnormal condition of one or both parents, such as is

* Report on the Influence of Marriage of Consanguinity upon Offspring, by S. M. Bemis, Louisville, Kentucky: Transactions of the American Medical Association, vol. ii. p. 319. 1858.

† Gazette Hébdomadaire, p. 598, Sept. 14th, 1860.

‡ Nouvelles Observations sur les Dangers des Mariages entre consanguins au point de vue sanitaire, par M. F. Devay: Note lué à l'Académie Impériale des Sciences, &c., de Lyon: Gazette Hébdomadaire, p. 595, Sept. 14th, 1860.

§ Gazette Hébdomadaire, p. 599, Sept. 14th, 1860. ‖ The Census in Ireland, p. 24.

meant by that sarcastic proverb of the Jews, "The fathers have eaten sour grapes, and the teeth of the children are set on edge;" or it may be that excess has reversed the action of some natural law in development, as seems to occur when the children of the over-wise are born fools.

Consequently there are many causes which might interfere with sexual limitation in the deaf and dumb; and although I have referred to some well-marked examples of its occurrence in this disease, yet in most of the cases I am acquainted with the sexes seem to be somewhat indifferently affected. I will therefore proceed to consider briefly those cases of sexual limitation in hereditary deafness where the defect is developed after birth. Dr. Kramer,* whose observations on diseases of the ear extend to two thousand cases, states that "in some families several, or even all, the members suffer from difficulty of hearing in a greater or less degree, especially of a nervous character;" but he does not allude to any sexual peculiarity. Dr. Fosbroke† remarks: "I have been consulted upon deafness by more than one member of the same family at the same time, as in the instance of Lady B., and her daughter, Miss G. B."

Mr. Wilde,‡ in speaking of nervous deafness from some peculiarity of the auditory nerves, observes: "I know many cases in which mothers and daughters are deaf. I have also known several members of the same family and its collateral branches deaf in one ear. In the upper ranks of society the disease is much more frequent in females than in males."

Mr. Wright§ refers to the case of a "noble family, all of whom hear least on the left side; and in M. Saissy's case (p. 537), already referred to, the deafness affected only the males on their attaining the age of forty.

That the preceding are not to be regarded as cases taken at haphazard, but as really illustrating the principle of sexual limitation in hereditary disease, admits of further proof, for still confining our attention to the class of cases we have hitherto been considering, it is possible to show that where the offspring inherit from each parent a distinct form of disease, limited in its appearance on both sides to the same sex, as, for instance, to the female, the limitation is maintained with comparatively as much strictness as where only one kind of hereditary disease is involved; for the husband in such intermarriage is merely the medium of transmission to the female offspring; and whilst, therefore, the sons as a rule remain free from any inherited disease, the daughters are liable to suffer from the double taint. This is well shown in the following case, which at the same time will serve to illustrate further the connexion which exists between imperfections of the organs of sight and hearing: "A woman, born of a microphthalmic mother, but having the two eyes perfectly developed, was

* The Nature and Treatment of Diseases of the Ear, by Dr. William Kramer, translated from the German by James Risdon Bennett, M.D. London, 1837, p. 31.

† Lancet, vol. i. p. 740. 1830–1.

‡ Practical Observations on Aural Surgery, p. 382. 1853.

§ Lancet, vol. ii. p. 413. 1830–1.

married to a man whose grandmother was deaf and dumb. Of this marriage there have been born five children—three boys and two girls; these last two are affected with microphthalmia. In one of these girls, who is at the same time deaf and dumb, there is complete absence of the iris; the other is married, and has a male child three years of age who is deaf and dumb, at the same time that it is affected with microphthalmia and coloboma iridis."* The absence of any hereditary defect in the three brothers in this case, whilst their two sisters inherited disease of the eyes from their maternal grandmother, and disease of the organ of hearing from the paternal great-grandmother (which, as regards one of them, did not show itself till the next generation), is decisive in favour of the influence of sexual limitation, for the exception which occurs in the little son of one of these females is no more than might be reasonably expected in cases where the inheritance, as on this occasion, extends to five generations.†

I shall conclude my remarks on sexual limitation in defects of the organ of hearing with a case of congenital malformation of the external ear, which occurs in my own practice. A little boy, aged three years, an only child, has no external ear on the left side; below the situation of the external meatus, which is imperforate, there is a small lobe or fold of integument; the right ear is well developed. The father of this little boy has a male cousin, married, with three children, of whom the eldest and the youngest are boys, and the intermediate child is a girl. The two boys have deficiency of the cartilage of the left ear above, which I have had the opportunity of knowing to be congenital, as I attended the mother at their birth. The daughter is free from the defect. The limitation in this case to the male sex, and to the same side of the body in each, leads to the inference that it was hereditarily associated with some abnormal condition of the great-grandfather, if not a previous ancestor of these three boys. The readiness with which such congenital peculiarity of the ear can be transmitted, is familiar to all of us in the lop-eared varieties of domestic animals, which have now become so common that Mr. Darwin informs us, "Not a single domestic animal can be named which has not in some country drooping ears."‡ And in like manner other defects of the ear may become hereditary. Portal§ refers more than once to hereditary peculiarities in the shape of the external ear. Mr. Wright states that he knows a family "every member of which has, more or less, a malformation of both ears;"‖ and in some nations hereditary peculiarities of this structure have become permanent, as in the Kalmucks, who are no

* Annales d'Oculistiques, tom. xiii. p. 31. 1845.

† As a companion to the above case I may refer to one published by Morgagni (De Sed. et Caus. Morbor. Epist. Anat. Med. iv. Art 4), of a man attacked with stone as his paternal uncle, and who perished from apoplexy like his father. Thus combining in himself, as Portal states (op cit., p. 43, note), two hereditary diseases. And lest there should be any doubt as to the direct inheritance of the former disease in this case, I will cite the case of the celebrated essayist Montaigne, who suffered from stone in the bladder like his father (Essais de Michel Montaigne, liv. 11, chap. 7).

‡ On the Origin of Species, p. 11. 1859. § Op. cit., p. 16, and note, pp. 16–17.

‖ Lancet, vol. ii. p. 464. 1830–1.

less distinguished for their large and unsightly ears, than their neighbours, the Chinese, are for the angular position of their eyes.

In all of the foregoing illustrations I have limited myself to such hereditary diseases as may be grouped under one head, for it is now very generally admitted that "the skin is the seat of all the senses, which are but modifications of that of touch, so that their organs are special apparatuses superadded to this great membrane."* It will be perceived, therefore, that in speaking of ichthyosis as favouring the statements made respecting the limitation of hereditary disease, and then passing to the consideration of the hereditary diseases of the organ of vision by a brief allusion to albinoïsm, that my object has been to avoid the temptation of hastily forming an opinion on this subject by some striking but disconnected cases, which can be easily gathered from the medical literature of this and other countries. Such a proceeding would be wrong in a preliminary inquiry of this nature, for these cases, curious and decisive as they might appear individually, would not allow of any general conclusions being drawn from them, or satisfy any one of their being more than isolated and exceptional departures from the normal standard of disease. But when, on the contrary, we take diseases hereditarily affecting certain structures of the body and their dependencies, and inquire how far the development of these diseases is limited in the way I have mentioned, it seems to be quite allowable for us to strike the balance between any conflicting evidence afforded by such cases, and then to have recourse to any additional evidence which might be derived from similar phenomena in the hereditary diseases of other parts of the body.

It has been necessary to avoid any allusion to diseases affecting hereditarily the organs of smell and taste, for they are as yet but imperfectly known. There are, however, many curious facts connected with these senses which might be studied with advantage: such, for instance, as the power possessed by some individuals of recognising odours imperceptible to others, the singular and sometimes alarming effects of certain odours on certain individuals, and some peculiarities connected with the organ of taste; all of which have been hastily disposed of as idiosyncrasies, but which, if more fully inquired into, might be often found to be hereditary, as is said to be the case with regard to the influence of perfumes on Roman ladies, even in the present day, who cannot in consequence bear the presence of flowers in their rooms. When such peculiarities are noticed, it would be far better to trace their extent and history, rather than to shelve them with a name signifying nothing.

Among the hereditary diseases of the skin in which sexual limitation prevails, might be included the so-called hæmorrhagic diathesis, which is said to be so common in the west of Germany and in the Rhenish provinces, that "in some families scarcely a single male arrives at maturity;"† the person thus diseased bearing the significant name of *bluter* or bleeder. This peculiarity of system seems to be due to

* Dictionnaire de Médecine, art., "Albino," p. 122.

† British and Foreign Medico-Chirurgical Review, vol. ix. p. 247. Jan. 1840.

congenital malformation of the solids, rather than to any altered condition of the blood itself; and although I have hesitated about referring to it as a hereditary affection of the skin, from deference to the statements of others on the subject, I cannot refrain from saying, that there are many strong arguments in favour of such an opinion, among which I may offer the following:—

1st. The hereditariness of the affection itself, extending to as many as five, if not more, generations of the same family.*

2nd. The sexual limitation of the disease, which from its affecting almost exclusively the males for several generations, and never, so far as I am aware, having been observed to affect correspondingly the females (who, as a rule, have a greater tendency to hæmorrhage than the other sex), may be referred to as an instance of sexual preference in disease; but in some of these cases it is recorded, that whilst the males alone have suffered from the disease, the females alone have been able to transmit it, as in the case of Mr. Appleton,† whose daughters conveyed the complaint to his grandsons, and who in their turn transmitted it, through their daughters, to their grandsons; the males in this family, as in many others similarly affected, never inheriting the disease direct from their fathers, but always through females from their grandfathers, as occurred in my case of ichthyosis, and which Mr. Lane,‡ in an otherwise able paper on the subject, somewhat erroneously supposes is an invariable rule in all cases of the hæmorrhagic diathesis.

3rd. The association, in some cases, of the hæmorrhagic diathesis with hereditary rheumatism—a disease essentially characterized, if not initiated, by perverted action of the skin. This occurred in the case related by Dr. Osborne,§ and also in Dr. Hughes' case:|| in the latter the united diseases prevailed for five generations, affecting exclusively the males; whilst the females, although they have never presented the same tendency to either disease, have, without exception, transmitted both to their male children; the hæmorrhage never manifested itself alone, but was constantly accompanied by rheumatism, which last always followed, and never preceded, the hæmorrhage.

4th. That in some of the cases recorded there has been a distinctly hæmorrhagic eruption on the skin, as occurred in the case related by Mr. Murray,¶ of the Coutts' family, in whom the bleeding was always preceded by the appearance of blue spots and dots on the skin; and when these began to fade, it was a sign that the attack was over. So, likewise, Dr. Elsaesser states** that in one case, for some time before

* New England Journal of Medicine and Surgery, 1813, vol. ii. pp. 222–5; Transylvania Journal, April, 1832.

† New England Journal of Medicine and Surgery, vol. ii. pp 221–5. 1813. Edinburgh Medical and Surgical Journal, vol. xxxvi. pp. 317–20, 1831; and vol. lxxvii. pp. 1–10, 1852.

‡ Lancet, vol. i. p. 188. 1840–1. § Dublin Journal, p. 34. 1835.

|| Transylvania Journal, April, 1832; and American Journal of Medical Science, pp. 542–3. 1832.

¶ Edinburgh Medical and Surgical Journal, vol. xxvi. pp. 33–5. 1826.

** Hufeland's Journal, Feb. and Sept. 1824; quoted in the Edinburgh Medical and Surgical Journal, vol. xxv. pp. 291–3. 1825.

the hæmorrhage, the ears became of a deep red colour; after the bleeding they became cold and exsanguine. Intolerable itching of the skin was also observed; and in one case the whole abdomen became blue on one occasion, and on another a bright red rash occurred. Blueness of the body in these cases has also been observed by other writers, as by Nasse, in his 'Essay on Cyanosis.'

5th. The statement of Fordyce and other writers, that in these cases deep wounds bleed less freely than superficial; this statement, however, has been disputed, and needs confirmation.

6th, and lastly. The geographical range of the disorder, which in Europe is between 45° and 60° north latitude, and in America between 30° and 45° north latitude;* an important fact when at the same time it is remembered that in America yellow fever, which is characterized by deep-seated or internal hæmorrhage, does not extend its ravages in the epidemic form beyond the 46° of north latitude,† at which limit the so-called hæmorrhagic diathesis begins to be developed.

From the great stress laid on hereditary diseases of the skin and its dependencies, it must not be supposed that hereditary diseases occurring in other parts of the body do not often show a like peculiarity of sexual limitation, either with regard to their appearance or their transmission; for in all hereditary diseases there is a tendency to it, and even in mixed cases, where the parentage of the disease is doubtful, there is sometimes, as it were, a cropping out of this interesting phenomenon. In phthisis, for example, the late Dr. Theophilus Thompson, in a paper read before the Medical Society of London, remarked, "it is worthy of observation, that the influence (of phthisis) tends peculiarly to extend in the direction of the sex in which it first appeared."‡ And in 'The First Annual Report of the Hospital for Consumption and Diseases of the Chest,' it is stated, that of 669 male and 341 female patients suffering from phthisis, and which was ascertained to be hereditary in 18·2 per cent. of the former, and in 36·3 per cent. of the latter (showing that females are twice as liable as males are to inherit the disease from their parents), it was found that whilst the father transmits the disease to his sons in 59·4 per cent., and to his daughters in only 43·5 per cent., the mother transmits it to her sons in 40·6 per cent., but to her daughters in 56·5 per cent.

Among the cases which I have at present under observation in private practice, there are some well-marked illustrations of sexual limitation in hereditary disease, in addition to those already mentioned. One is a case of hereditary imbecility limited to the female line. The mother suffered from the occasional recurrence of mental derangement, and died lately from phthisis at the age of fifty-two years, leaving a family of three children, consisting of one boy, aged fifteen years, who, being very intelligent, is qualifying himself for a teacher in one of our

* Schmidt's Jahrbuch, vol. lxx. p. 35; quoted in British and Foreign Medico-Chirurgical Review, vol. ix. p. 516. 1852.

† William Farr, Esq., F.S.S.; Statistical Journal, vol. xv. p. 167. 1852.

‡ Hints on some relations of Morals and Medicine, with special reference to Pulmonary Consumption: London Journal of Medicine, p. 402. 1851.

Government training schools; and two girls, aged eighteen and twelve years, who are both imbecile. A sister of this woman has had occasional attacks of mental derangement to a greater extent than herself; and there are three maternal aunts (maternal grand-aunts of the children) in whose families a like condition prevails. The first aunt (Mrs. S——) has three children, the eldest of whom is a daughter, very eccentric in manner and silly in conversation; the other children are a son and a daughter, both intelligent. The second aunt (Mrs. G——) has had four children: two sons, who are intelligent, one being a modeller and the other an accountant; and two daughters, one living and very eccentric, the other died in Hanwell Lunatic Asylum. The third aunt (Mrs. J——) has two children—namely, one son, who is an intelligent schoolmaster; and a daughter, who is intelligent on most subjects, but is a religious fanatic, and has lately taken up with the Mormons.

Another case is that of a shoemaker in my neighbourhood, who has had a family of eight children, consisting of four sons, who are all alive and apparently healthy, the eldest being now nineteen years old; and four daughters, three of whom have died from cerebral disease, one at the age of six months, one at the age of two years, and one at the age of four years and a half; the remaining girl is an infant, at present under treatment for hydrocephalus. The birth of the daughters in this case alternated with that of the sons; and the mother informed me, at my first interview with her, that in consequence of this tendency to disease of the brain, she had no hope of being able to rear a daughter.

Another is a case of enuresis affecting the eldest and the youngest sons of a family of five children, who have survived the age of infancy; the three other children are two boys, aged fifteen and eleven years, and a girl aged seventeen years, who have not suffered in the same way. The disease is inherited from the father, who was subject to it till he was past the age of puberty; and he has three male cousins, the children of a maternal uncle, who have in like manner suffered from it; these three males have a half-brother by their father's first marriage, and two sisters by his second marriage with their mother, who have long passed the age of puberty, and have always been free from the complaint.

Another is a case of hereditary twins in one of my female patients living near Dorset-square. The mother, the maternal aunt, the maternal grandmother, and the maternal great-grandmother have all had twins; but none of the sons in these families have ever been known to transmit in this way a double heritage, although some of them have been twins themselves with twin brothers, both of whom have in some instances married and have had large families of children, but none of their wives have ever been enabled to favour them with two children at a birth.

The number of these cases might be greatly increased by adding to the list such cases as exhibit a tendency only to the occurrence of this restriction, but sufficient have been cited for our purpose; and from a general consideration of the preceding cases, it will be evident that sexual limitation may affect either the development or the transmission of hereditary disease, and that these two conditions are often antagonistic to each other; for in many cases where the disease is limited in its appearance to one sex, its transmission is restricted to the other. From a careful analysis of a large number of cases, I am at present disposed to think that sexual limitation in the transmission of disease is more common in females than in males.* In several of the cases I have mentioned, where the appearance of the disease has been strictly limited to the males, its transmission has been quite as strictly limited to the female sex, occasioning the phenomenon of *double atavism*, as where neither sons nor daughters ever inherit their father's disease, but only the grandsons in the third and fifth generations, by transmission through the females of the second and fourth generations.

This subject is at present exciting some interest in connexion with the transmission of hereditary syphilis, which M. Cullerier† has lately announced to be due only to maternal influence, the male parent exerting none; and this opinion has been supported by the observations of M. Notta, surgeon to the Lisieux Hospital, and by those of M. Follin.‡ And as an illustration of the power of the female to transmit even disease which she cannot be supposed by any possibility to share, I may cite an interesting case related by Sir Henry Holland, "of hydrocele occurring in three out of four generations in one family—the omission adding to the singularity of the fact, from its depending on a female being a third in the series, in whose son the complaint reappeared."§

These facts are strongly opposed to some statements which have been lately advanced in favour of the male sex being chiefly concerned in the transmission of hereditary disease. Mr. J. B. Thomson, of Perth, has published some short papers, "On the Comparative Influence of the Male and Female Parent upon the Offspring,"‖ in which he endeavours to prove that "the influence of the male is greater than that of the female parent in the transmission of the *skin* texture to the progeny;" and I may state, moreover, that in Norway, where leprosy, a form of disease closely allied to ichthyosis, has of late years greatly increased, and is regarded by the most distinguished physicians of the country as purely hereditary, a proposal has been laid before the Storthing or Norwegian Parliament, to prohibit the marriage of a leper; and it has been a topic of public and professional discussion among the Norwegians, how far it would be just to deprive the male offspring of leprous parents of the power of propagation.¶

To whatever extent the practical application of such scientific truths might be thought allowable in the present day in Norway, it is but

* In the report of a case of hereditary malformation of the hands, affecting ten generations of the same family, it is stated "that it was the women only who had the misfortune of entailing this defect on their offspring" (Edinburgh Medical and Surgical Journal, vol. iv. p. 252, 1808); and similar statements often occur in the records of hereditary disease.

† Mémoires de la Société de Chirurgie de Paris, tom. iv. 1855; quoted in the British and Foreign Medico-Chirurgical Review, vol. xix. pp. 156–8. 1857.

‡ Medical Times and Gazette, Aug. 1860, pp. 116–17. § Op. cit., p. 31.

‖ Edinburgh Med. Journal, pp. 501–4. 1858–9. ¶ Ibid.; and ibid., p. 175. 1855–6.

fair to state that this opinion is quite in accordance with that which at one time prevailed in our own land; for even so lately as the last century it was gravely discussed at one of the meetings of the Royal Society,* whether the history of the Lambert family did not justify the assumption that, under certain conditions, a new variety of the human race might be developed, consisting of men whose skins would be clothed, and that, too, permanently, with the peculiar appendages of the hedgehog; and when, in 1803, the two brothers, John and Richard Lambert, grandsons of the celebrated Suffolk labourer, exhibited themselves in Paris, the idea promulgated at the Royal Society was boldly advanced to increase the interest of the Parisians in their case, and they were announced as natives of a district in Australia, adjoining Botany Bay, peopled (it was said) with a race of porcupine men similar to themselves.†

In like manner, there have been and still are certain delusions entertained with respect to the external ear, which are interesting in connexion with my case of hereditary malformation of this organ. Everybody is aware that the most characteristic feature of an ass is its ears, and it is curious that in this, as in many other instances of a similar kind, the popular notion is confirmed by science, for there is a certain definite connexion between the development of the external ear, and different forms of insanity, and both the form of the ear and the insanity may be hereditary. This was observed by M. Foville at the Charenton Lunatic Asylum, and has since been confirmed by Dr. Conolly at Hanwell.‡ In addition to which, there is the remarkable fact that in some of the Swiss Cantons, 1 in 206 of the population is born deaf, owing in great measure to the local prevalence of cretinism and idiocy.§ But the most interesting point in the ear-case I have related is connected with a prevalent belief in former days, peculiarly applicable to the subject before us, which associated the ears with the reproductive function, and which would, therefore, have given to this case, in earlier times, a painful significance. As I have already referred to some of the practical applications of the subject of sexual limitation in hereditary diseases, I must be content for the present with saying, that if such opinions regarding ichthyosis and leprosy as those already mentioned, could be maintained by the highest scientific societies of this and other countries on the inheritance of man's physical nature, we need not feel surprise if, in a ruder age, and with less knowledge to guide them right, our ancestors acted upon a like belief respecting the inheritance of man's moral nature, and had recourse to means capable, as they thought, of preventing the perpetuation of crimes. This they sought to accomplish, not by means of juvenile reformatories, which have become so popular in our own day, but by cutting off the ears of their criminals, lest, as it was said, they should produce their like.‖ This old-established practice was designed rather to benefit society than

* Philosophical Transactions, vol. xlix. part i. p. 23. 1755.
† Alibert: op. cit., tom. ii. p. 174. 1822.
‡ British and Foreign Medico-Chirurgical Review, p. 597. 1845.
§ Wilde: The Census in Ireland, p. 9. ‖ Rees' Cyclopædia, art. "Ear."

to punish the offender, and although it is impossible for me to tell by what substitutes for clinical observation they attained this knowledge, it is well known that the practice survived the theory, for this mutilation was practised on criminals long after the object for which it had been instituted had been lost sight of; for even our College of Physicians had recourse to this means of punishing their refractory members.* That a certain amount of reasoning preceded the application of this preventive treatment may be inferred from the fact that among the Scythians a like belief in its efficacy prevailed even so far as to lead them to have recourse to bleeding behind the ears as a means of inducing sterility:† a kind of derivative treatment which was no doubt highly approved of in those days, for it was a custom also with this nomadic race to "get themselves cauterized on the temples, to counteract the disagreeable effects to the other end of their persons from their long and rough riding."‡ The case therefore related of congenital malformation and absence of the ear would probably have possessed a singular importance in that golden age of ignorance, but now,

> "Manners are changed, old times are gone;"

and when the young people referred to have arrived at years of discretion, they may not only be permitted to marry, but they may also expect to have children to inherit and transmit the family peculiarity of *flaccidæ aures,* or drooping ears.

In tracing the hereditary transmission of disease, there is one source of error against which it seems necessary to be on one's guard, although some may be inclined to doubt whether it can be so or not. I refer to the influence of maternal emotion on the development of the offspring. Apart from any inquiry into the arguments which could be adduced in favour of this popular belief, which has prevailed in all ages, and the effective influence of which seems to have been known and applied before Israel became a people, I allude to the subject now, in consequence of having observed some curious cases of sexual limitation in congenital disease, in which it appeared doubtful whether the disease was hereditary in the common acceptation of the term, or acquired through the influence of maternal emotion, for the first appearance of the disease in many of the preceding cases has been ascribed to this cause. Strictly speaking, there is not much need to dwell upon the subject here, as it would not be difficult to show that the distinction referred to is a distinction almost without a difference; for whether the morbid condition influencing the development of the child be one affecting the mind or the body of the parent, the taint in the offspring is in each case an inherited taint, differing chiefly in the permanence of its influence as regards the parent, and the period of its transmission as regards the offspring.§

* Shakspeare's Medical Knowledge, by John Charles Bucknill, M.D. 1860.

† Rees' Cyclopædia, art. "Ear."

‡ A. M. Edwards, Esq.: British Medical Journal, March 31st, 1860.

§ I have at the present time under observation, two brothers with cleft palate, where the defect in each was apparently due to maternal emotion; the period of the pregnancy to which the emotion was referred being quite consistent with its action in causing an

In conclusion, it may be urged that there is perhaps no subject connected with our profession which needs a deeper research than the hereditary transmission of disease, and if the preceding cases of sexual limitation in hereditary disease are insufficient for any practical deductions to be as yet established, I will venture to claim for this branch of the subject a greater consideration than it has yet received, and at the same time suggest the necessity for admitting a more extended application of the principle of hereditary transmission than some writers of the present day seem willing to allow. The absence of any satisfactory explanation of the phenomena of sexual limitation in disease does not destroy the interest of our observations, or the possible usefulness of our research; for its occurrence in the human subject is paralleled by what occurs in the development of the lower animals, and Mr. Darwin has consequently availed himself of its influence, as affording a strong argument in favour of his theory of natural selection to account for all the diversified forms of animal and plant life. And just in the same way that we recognise in the human body traces of organs no longer needed, but which in a lower state of existence were not only useful, but perhaps also necessary for its development, so likewise it would appear that whilst in our own race this restriction is exceptional, in many of the humbler tribes of animals, as well as in plants, its influence is so strong as to supersede what, as regards the higher classes, seem to be the ordinary rules affecting the transmission of the species.

It is so much the custom to look upon disease as a state contrary to nature, and in consequence as opposed to order—for one of the most common synonyms for it is disorder—that we can hardly realize the fact that in a very large number of cases of hereditary disease, evidence is to be found of some uniformly-acting and controlling influence, which is strikingly manifest in the cases referred to, where disease has been shown to have both its appointed sex, and its appointed period to occur. So completely, indeed, has this latter fact forced itself upon the attention of the public, that we constantly find precautions have been taken to guard individuals of a family prone to hereditary disease, such, more particularly, as phthisis, against what common observation has proved to be the dangerous period of their lives; for the friends in such cases hope that if this period can be got over safely, the danger may be less; and we all know that it is so, although in too many instances every precaution fails to secure such inheritors of disease from that danger which they are born, as it were, to encounter at one period of their lives.*

arrest of development; and it must be admitted that in cases like these, where so large a superstructure of fable has been raised on a substratum of truth, it would be prudent where such action could not possibly have occurred, to reject altogether the cause assigned.

* The principle of sexual limitation may be traced in family statistics of mortality from diseases not in themselves hereditary. In a case of measles affecting the only son of a respectable mechanic, I ascertained that the disease was peculiarly fatal to the females on the father's side, but that the males, when attacked, recovered. The father and his only brother had both recovered from the disease without any unfavourable symptoms, whilst all their five sisters had died from it. The little boy referred to recovered quickly, but his only sister had died the previous year from the disease, which at its commencement

But in thus insisting on sexual limitation in certain forms of hereditary disease, it must not be supposed that the limitation, when it does occur, is always strictly confined to one sex, and that the disease may not show itself occasionally in the other. Such a supposition would be beyond what any one could reasonably hope to establish, and all that it is possible to contend for is, that when this limitation does occur, the tendency of the restriction to become hereditarily attached to one sex is sufficiently decided to merit special notice, and that although the disease may show itself in the opposite sex in succeeding generations, yet there is always a marked preference for that in which it first occurred, and that the preference so established is continued by inheritance to this favoured sex.

Illustrations, however, of the strictness with which certain peculiarities are limited to one sex are extremely common in the development of the lower animals and in plants. In hybrids, for example, the distinguishing characteristics of one or the other parent continue constant, where the selection of the parents is the same. In the case of insect hybrids, such, for instance, as result from the union of the male of Sinerinthus ocellatus with the female of S. populi, the markings and colours of the male and female continue constant under similar conditions. So also in the case of plants, sexual limitation is evidenced by the fact that vegetable hybrids resemble "the male parent most in foliage, and the female in flower." (Lindley.) But as hybrids are usually sterile, such transmitted peculiarities fail to become permanent, owing to the extinction of race. It may, therefore, be said, that whilst sexual limitation can be cited as one among many corresponding examples of laws, if we may so speak of such controlling influences, affecting all classes of animals and plants, from the highest to the lowest, it appears to affect them inversely to the rank which each class may be allowed to take; for whilst many of them exhibit in their development an abject submission to its influence, it becomes less and less binding as we ascend in the scale of organized beings, and is seldom to be met with in our own race, except in conjunction with morbid development, when, through what must be termed the degrading influence of disease, sexual limitation is occasionally seen to occur, and perhaps more especially in connexion with hereditary diseases affecting the skin and its dependencies.

was apparently destined, in her case, to prove fatal. This sexual limitation of mortality in families may be independent of any special disease, as for example in those cases where the tendency to death from almost every disease is so strong, that it can only be referred to a hereditary inability in one or the other sex to maintain life, or to live beyond a certain age; and consequently death may be the result of whatever disease happens to them previous to the attainment of that limit of their existence. It is not very unusual to meet with families in which, for example, it would seem impossible to rear a son, whilst no such difficulty prevails with respect to daughters; or the reverse may be the case, where the sons grow up, but no daughters arrive at maturity, owing to the peculiar fatality which attends disease in the one sex and not in the other. When daughters are concerned, no special importance is generally attached to this fact, but in the case of sons it is otherwise, for where the inheritance of property or of title is subject to the provisions of the Salic law, the failure of "heirs male," must always be viewed as a greater loss than that of children who cannot inherit either the one or the other.

ART. III.

On Mitral Regurgitation arising independently of Organic Disease of the Valve. By J. S. BRISTOWE, M.D. Lond., F.R.C.P., Physician to St. Thomas's Hospital.

THERE are, probably, no diseases so well understood at the present day as those affecting the valvular apparatus and the muscular parietes of the heart. The conditions under which valvular lesions occur, the local signs and general symptoms which they produce, and their morbid anatomy, are detailed with minuteness and precision in systematic works. The causes of hypertrophy and dilatation, too, are for the most part well appreciated and defined. Yet no one can have been very long engaged in the practice of his profession, provided at least clinical experience hasbeen associated with pathological research, without recognising cases which established principles have not enabled him to fathom, or have led him, it may be, to misinterpret. Such cases are far from uncommon. Scattered among these, and constituting, in truth, a large proportion of them, are a number of cases, related to one another, and evidently forming a class; in which, with a history of cardiac disease, and with clinical proofs of mitral incompetence, the mitral valve and the orifice it protects are found to present a perfectly healthy appearance; and in which, without necessarily any obvious cause, a certain amount of dilatation and hypertrophy of the muscular parietes are found to co-exist. These latter obscure, and, so to speak, exceptional cases, cannot be said to have been entirely disregarded by systematic writers; yet, for the most part, they have referred to them incidentally, or have spoken of them vaguely, or have confused and embarrassed their account of them by grouping them with cardiac affections of a different kind, and have therefore necessarily failed to furnish anything approaching to a vivid or life-like picture of them, or to give them that prominence which their importance and frequency justly claim for them. Occasional cases of the kind, however, have been recorded with great accuracy and clearness; and several papers have been published, in which the subject has been handled with more or less completeness and ability, and with a full appreciation of its claims upon our attention. I refer especially to a series of communications by Dr. McDowell to the fourteenth, sixteenth, and eighteenth volumes of the 'Dublin Quarterly Journal;' and to an unfinished article by Dr. W. T. Gairdner, in the 'Edinburgh Medical Journal' for July, 1856.

The interest and importance of the cases to which the above remarks relate, and the frequency of their occurrence, the imperfect recognition of them which is manifested by systematic writers, the comparatively little attention which seems to have been accorded to those papers in which they have been discussed, and the ignorance which still prevails in many quarters in regard to them, will, I trust, in combination, be deemed a sufficient apology for my re-introducing the subject to the consideration of my medical brethren, by placing on record some of the results of my own experience thereon in the pages of this Journal.

During a period of upwards of ten years, in which I conducted the post-mortem examinations of medical cases at St. Thomas's Hospital, it by no means infrequently fell to my lot to inspect cases of reputed mitral disease, in which all the secondary effects of that lesion—pulmonary apoplexy, anasarca, nutmeg liver—were indisputably present, but in which the heart was found to present but little departure from the healthy state, and in which all the valvular structures appeared to be perfectly sound and competent. I have felt convinced, for some years past, that these cases were neither exceptional nor rare, but have only latterly been induced to collect and compare the scattered facts on which the general opinions I entertained were based. I soon found unfortunately that, owing to imperfection in the clinical records, many of my earlier cases had lost the value they once possessed, and that more than one it had become impossible to identify. A sufficient number, however, have come under observation between October, 1856 (from which date the clinical reports at the hospital have been more accurately prepared and preserved), and October, 1860 (when my duties as pathologist terminated), to render my report, if not so complete as I could have wished, at least, I hope, not altogether devoid of value.

My first object will be to prove the fact of regurgitation through the left auriculo-ventricular orifice in certain cases in which the mitral valve is found to exhibit a perfectly healthy appearance, and to establish the frequency of its occurrence, by detailing such well-marked examples of the phenomenon in question as have occurred in the hospital during the four years above specified.

CASE I.—*Systolic murmur at apex of heart; dilatation of left ventricle, with slight hypertrophy; apparently healthy mitral valve.*—Rosina Morton, a girl, sixteen years of age, employed in the manufacture of paper boxes, was admitted under Dr. Barker's care on the 29th December, 1859. She had had shortness of breath, with some orthopnœa and palpitation for seven weeks; five weeks since the legs began to swell; and two weeks ago cough came on. She had never suffered from rheumatism, her illness had not been preceded by scarlet fever or sore throat, and she had never had hæmoptysis; indeed, previous to the access of her present malady she seems to have enjoyed good health; the catamenia had not yet appeared. The symptoms above detailed increased in severity up to the time of admission, at which date her aspect was pale, her lips livid, and her lower extremities œdematous; she had extreme dyspnœa, on account of which she was compelled to maintain the sitting posture; and presented the usual general symptoms of advanced cardiac disease; there was a distinct systolic murmur audible at the apex of the heart. R. Liq. ammon. acet. ʒvj, spt. æth. nit., spt. æth. co., ana ʒss, quartîs horîs, cum mist. camph. Hyd. chlor. gr. iij, ext. hyoscy. gr. v, omnî noctê. Emp. lyttæ sterno. Haustus sennæ co. cras. Her condition was in no degree relieved by the treatment employed; she gradually sank, and died on the 5th of January, 1860.

Post-mortem examination.—There was considerable anasarca, but in other respects the aspect of the body was natural. *Chest:* The pericardium contained several ounces of serum, but was healthy; the heart appeared enlarged, but did not weigh more than 7 ozs.; the muscular tissue was firm, and perfectly healthy-looking; under the microscope it was perhaps a little granular, but certainly not fatty. The left ventricle, however, was decidedly, indeed considerably, dilated; and its walls were comparatively thin. The aortic and mitral valves were perfectly natural; the former was tested and found to be competent, the latter appeared to be so. The remaining valves were healthy. The left ventricle contained a little dark-coloured uncoagulated blood, the right presented in addition a fibrinous coagulum. The right side of the chest was distended with slightly opaline fluid—in all, about two or three pints. The entire surface of the pleura was lined by a layer of straw-coloured, elastic, reticulate, freshly-deposited lymph. The lung was much reduced in size, and compressed against the spine; the apex only was crepitant; the rest was airless from compression. In the lower lobe were two somewhat recent apoplectic clots, of the size of a chesnut and filbert respectively. The left pleura presented a few old adhesions. The left lung was of moderate size, crepitant, and generally healthy; but the lower lobe contained a clot, about the size of a marble, the edges of which were decolorized. Larynx, trachea, and bronchial tubes healthy. *Abdomen:* Peritoneum healthy; liver much congested; spleen of usual size, and exhibiting well-marked Malpighian corpuscles; kidneys congested, but otherwise healthy. All the other viscera were in a healthy condition.

In the above case the symptoms of mitral incompetence were well pronounced; yet the heart, beyond being dilated and a little hypertrophied, seemed perfectly healthy. There was no chronic affection of either lungs, liver, kidneys, or any other organ, to which her symptoms could possibly be referred. The only morbid conditions observed, in addition to those exhibited by the heart, were such as usually follow on disease of the mitral valve—pulmonary apoplexy, nutmeg condition of liver, and anasarca. It should be observed that the pleurisy (which constantly arises in heart disease, and from the cause here assigned) was obviously solely attributable to the irritation excited by the pulmonary clots.

CASE II.—*Systolic apex-murmur; hypertrophy of heart; apparently healthy mitral valve.*—Margaret Humes, a married woman, twenty-two years of age, was admitted into St. Thomas's under Dr. Peacock's care, on the 14th September, 1858. She had suffered from a dropsical state of the abdomen and legs for three months before admission, but for eight months had laboured under considerable dyspnœa and palpitation, previously to which time she had always enjoyed good healh.

Her face was somewhat sallow, her eyes dull and yellowish; but she stated that she had never had liver complaint or, although residing at Woolwich, ague. There was considerable anasarca, and much difficulty of breathing; there was an increased area of dulness in the cardiac region, and a systolic bruit, loudest at the apex of the heart; the pulse was small and frequent; the urine scanty, without albumen, and 1012 of specific gravity. She continued in much the same state until the day before her death, which took place on the 26th September. At this time she was seized with a severe attack of palpitation and dyspnœa, followed by faintness and convulsions. These continued on and off until the following morning, when she suddenly died, although appearing just before to be better. The dropsical symptoms had considerably abated. The treatment consisted at first of ℥iss of the mist. scoparii co. thrice daily, and a couple of pills each night, containing blue pill, squills, digitalis, and hyoscyamus, to which, after a week, a little extract of colchicum was added. On the day preceding death, the above mixture was replaced by one containing nitric ether and tincture of sesquichloride of iron; and a glass of gin was ordered to be given daily.

Post-mortem examination.—Of ordinary stature; anasarcous; weighing 8st. 4lbs. *Chest:* Pericardium healthy, but containing a few ounces of serum. Heart enlarged, weighing 16½ ozs.; the hypertrophy was most marked on the right side; the muscular tissue and the valves appeared perfectly healthy. There were a few adhesions in the pleuræ, and upwards of a pint of serum in each. The lungs were of ordinary size, and somewhat heavy; both of them, but more especially the right, were much carnified. The bronchial tubes contained a good deal of frothy secretion. *Abdomen:* Peritoneum healthy; all the abdominal viscera were in a natural condition.

The symptoms of heart-disease in the present instance appear, so far as could be ascertained, to have come on without obvious cause eight months prior to death. While under treatment there was a distinct systolic murmur at the apex, and the usual indications of mitral-valve incompetence; yet the valve was found to be apparently healthy. The heart was hypertrophied, chiefly on the right side, but whether dilated is not expressly mentioned. The only other morbid conditions present were anasarca and carnification of the lungs—both ordinary sequelæ of cardiac disease.

CASE III.—*Systolic murmur at apex; dilatation, with hypertrophy, of heart; atrophy of musculi papillares of left ventricle; apparently healthy mitral valve.*—W. Shillingford, a labourer, fifty-four years of age, was admitted into St. Thomas's, under Dr. Bennett, on the 11th of November, 1858. He stated that he had been in perfect health until six weeks before admission, when he began to suffer from some shortness of breath. A month ago severe cough set in, and from this time he has been laid up; three or four days later the legs began to swell; there was much palpitation from the first, and for the last three weeks orthopnœa. Two days before admission he first spat blood, about a teaspoonful in all; the bowels had been rather loose; he had never suffered from rheumatism or from winter cough.

On admission he was in a very precarious condition; the complexion dusky, the extremities cold and livid, the respiration rapid and laboured. He sat up in bed, bending forward. The legs were very œdematous, the urine scanty, slightly albuminous. The area of cardiac dulness was much enlarged, extending from the right edge of the sternum to the left of the left nipple. The impulse was diffused and heaving, but not very strong; the sounds feeble. A systolic murmur was detected at the apex of the heart when he first came in, but ceased to be audible soon after, apparently from increasing feebleness of the circulation. Cat. sinapis amplum sterno. Spt. æth. chlorici ♏x, spt. ammon. aromat. ♏xx, træ. scillæ ʒss, ex mist. camph. quartîs horîs. Gin two glasses.

Nov 12th.—He was evidently dying; the legs were covered with gangrenous-looking patches; the tip of the nose and the hands were blue and cold; he was expectorating a dark, non-coagulating, frothy, sero-sanguineous matter. Pulse imperceptible at the wrist. He died the following morning at ten minutes to twelve.

Post-mortem examination.—Of usual stature, somewhat anasarcous. The tip of the nose was extremely livid. The feet and hands were congested, and a diffused blush occupied the lower third of the left leg and the outer part of the right thigh. On cutting into the latter, the tissue between the skin and the fascia lata was found to be much injected, and to contain a little puriform fluid. On the front of the right leg was a thin black eschar about the size of a shilling. *Chest:* The pericardium contained three or four ounces of serum. A patch of old œdematous villous false membrane existed on the front of the apex of the heart, and similar but smaller patches studded the surface of the auricles. The heart appeared very large, but its cavities were much dilated and its walls thin; it weighed fifteen ounces and a half. All the valves were healthy-looking, and the aortic were competent. The musculi papillaries, however, of the mitral were small and conical; and they, with the chordæ tendineæ, appeared far too short to allow of the complete closure of the mitral valve. All the cavities contained soft, recent, dark-coloured clots, with fluid blood; but the left ventricle presented in addition large numbers of adherent softening clots. These varied in size from that of a walnut downwards, were somewhat buff-coloured and ribbed on the surface, and contained in their interior a thick, grumous fluid; the largest occupied the apex. Two or three similar cysts were found in the right auricular appendage. The pleuræ presented few old adhesions, and no recent lymph. The left cavity contained upwards of half a pint, the right more than a pint, of serum. The left lung was very large and heavy; its upper lobe was congested, but crepitant; its lower one contained several large masses of pulmonary apoplexy, surrounded by congested and carnified tissue. The right lung was congested, but for the most part crepitant; its lower lobe was airless in consequence of the pressure of surrounding pleural effusion, and contained an apoplectic clot as large as a hen's egg. The clot was imperfectly separated from the surrounding lung-structure by a buff-coloured margin, in which here and there suppuration was going on. The bronchial tubes of both lungs were loaded with tenacious, deeply blood-stained fluid, but were themselves healthy. Some of the branches of the pulmonary artery leading to the clots were distended with cylinders of black coagulum. There was no trace of emphysema in either lung. *Abdomen:* Peritoneum, healthy; liver, large, firm, and in a well-marked nutmeg condition; pancreas, stomach and intestines, kidneys and supra-renal capsules, healthy; aorta, healthy.

The case just related was but a day or two under observation; but there can be no question, I think, that it was strictly a case in point. His illness had lasted only six weeks, and he was admitted with the usual symptoms and signs of mitral incompetence. There was no disease of other internal organs to which these could have been secondary, and we can scarcely regard the mitral murmur, therefore, as a mere accidental precursor of death. The pulmonary apoplexy, the anasarca, the nutmeg liver, and even the slight albuminuria, associated as it was with structurally healthy kidneys, all pointed to the heart as the seat of primary disease. It is worthy of remark that the heart was considerably dilated, as well as hypertrophied; and that, although the mitral valve itself appeared healthy, its musculi papillares were unusually small and conical, and, with their tendinous cords, apparently of insufficient length to fulfil perfectly their normal functions.

CASE IV.—*Systolic murmur at apex of heart; dilatation, with hypertrophy of the organ; chordæ tendineæ of left side on the stretch; dilatation of mitral orifice, but apparent healthiness of valve.*—J. Bunting, a potboy, fifty-six years of age, was admitted into St. Thomas's Hospital under Dr. Peacock's care on the 12th January, 1858. He had been an intemperate man. He had been ill for about two years, but much worse for three months. His illness had consisted in cough, hoarseness, and great dyspnœa, accompanied by severe pain across the chest. He spat a good deal at first, but had never had hæmoptysis.

On admission his face and legs were œdematous, and had been so for about a fortnight, and he suffered from extreme dyspnœa. The thoracic resonance was everywhere sparing, especially below. Sibilant and sonorous rhonchi were audible over the whole chest, except at the extreme base, where the respiration was deficient. The cardiac dulness was increased in extent, especially in breadth; the first sound at the apex was flapping and prolonged. Pulse 100; urine slightly albuminous, of 1030 sp. gr.

Feb. 3rd.—On this day a distinct short, harsh, systolic murmur was audible over a limited space below and within the line of the left nipple. Pulse very feeble, but regular; no œdema of legs; vomiting nearly every day.

Feb. 10th.—Increasing dyspnœa, hæmoptysis; heart's sound more feeble, but murmur still audible; very little œdema of legs. In this condition he continued, slowly sinking, until the 9th of March.

Post-mortem examination.—Body spare, but not œdematous. *Chest:* Both pleuræ presented a few adhesions, and contained a good deal of serum. The lungs were œdematous, congested, and sparely crepitant in the greater part of their extent, but carnified and airless below. The bronchial tubes were congested, and contained a large quantity of muco-purulent fluid. The pericardium was healthy; the heart was large, and weighed sixteen ounces. The left ventricle was much dilated, its walls seven French lines thick midway between the base and apex; it was full of dark, soft coagula; and closely connected with the parietes, in the neighbourhood of the apex, was a considerable quantity of older clot, ribbed, and presenting alternate red and buff-coloured streaks, but not yet softened internally. The valves were perfectly healthy in texture; the aortic was competent; but the chordæ tendineæ were stretched, and probably prevented the mitral from completely closing its orifice. The right ventricle was relatively larger than the left; it was dilated, and its walls measured three French lines in thickness at the base. It was filled with coagula, and its valves were normal. The left auriculo-ventricular opening measured fifty-seven French lines in circumference; the right, sixty-three; the aortic measured three inches and four lines; and the pulmonic, three inches and two lines. *Abdomen:* The liver presented a nutmeg character. The kidneys were of usual size; they contained a few small cysts, and were somewhat congested; but their surface was smooth, and they appeared generally healthy. The rest of the abdominal viscera were healthy.

Here, again, the signs of mitral incompetence were clearly displayed. The history of the disease extends over about two years. Many of the symptoms complained of were those indicative of bronchitis, but the condition of the lungs was scarcely compatible with the existence of chronic bronchitis alone, for there was neither emphysema nor dilatation of tubes. The probability is that the pulmonary symptoms were from the beginning secondary to cardiac incompetence, with which view the condition of other organs was quite accordant. Slight albuminuria was detected, but this depended evidently on mere passive congestion of the kidneys. It should be observed that in this case, as in the last, the heart was dilated as well as enlarged, and that the chordæ tendineæ appeared to be relatively short, and on the stretch. But it must be added that all the orifices, including the mitral, were unusually large; the valves presenting, notwithstanding, a healthy appearance.

CASE V.—*Hypertrophy of heart; apparent healthiness of mitral valve; systolic murmur at apex.*—Michael Gnoude, a carman, forty-one years of age, was taken into one of Dr. Barker's beds on the 18th May, 1858. He had had cough every winter for eleven years. Had never suffered from rheumatism. His present illness began eight weeks ago with cough, short breath, and œdema of the legs. For the last five weeks he has complained of severe palpitation on walking, or any other exertion. All the above symptoms have gradually increased in severity, and the urine has become scanty.

On admission he had severe paroxysmal cough. The legs were very œdematous and tense; the pulse small and irregular, and he lay in a semi-recumbent posture from partial orthopnœa. On examining the chest, general bronchitic râles of a large mucous character were audible in all parts. The cardiac dulness was enlarged, and a systolic murmur was audible with the heart's action, most distinct at a point an inch below and internal to the left nipple. The urine was albuminous.

There was no great change after admission. He was slightly improved for a time by counter-irritation, diuretics, and purgatives; but the anasarca soon after increased, so as to require relief by acupuncture. He died on the 11th June.

Post-mortem examination.—Height, 5 ft. 9 in.; weight, fourteen stone; anasarcous in an extreme degree. *Chest:* There were numerous firm adhesions and a little serum in both pleuræ. The right lung was voluminous, but not emphysematous, crepitant throughout, congested, and œdematous. Several branches of the pulmonary artery, from the size of a crow-quill downwards, were distended with partially decolorized, more or less adherent coagula. The left lung was smaller than its fellow; its upper lobe was crepitant but œdematous; its lower lobe contained but little air, and presented several small encysted cretaceous masses, and a few cicatrix-like patches of fibroïd tissue connected with superficial depressions. The bronchial tubes of both lungs contained a good deal of secretion, but their mucous membrane was healthy, and their calibre normal. The mucous membrane of the larynx and trachea was congested, and a little excoriation was present at the posterior angle of each vocal cord. The pericardium contained at least half-a-pint of serum; the heart was large and firm, and weighed twenty-one ounces and three-quarters; the walls of the left ventricle were three-quarters of an inch thick in the thickest part, and those of the right at least a quarter of an inch. All the valves appeared perfectly healthy, and the aortic were proved, by sustaining a column of water, to be competent. The ventricles and large arteries contained a considerable amount of fibrinous coagulum; the auricles were distended with black-currant-jelly-like clots. Aorta, healthy. *Abdomen:* The peritoneal cavity contained half a pailful of serum. The liver was rather large and congested; spleen small, soft, and pale; pancreas healthy. In the centre of the posterior wall of the stomach was the cicatrix of a shallow ulcer. Intestines, healthy. The kidneys were of usual size, together weighing thirteen ounces and a quarter; their surface was smooth, and a little mottled; their cortex presented a good many thin-walled cysts, which were filled with thin serous fluid, and of which the largest was the size of a marble. They were healthy in other respects. The supra-renal capsules were large, but natural in structure.

In this case, as in that of J. Bunting, there is a history of old bronchitis; but in the present instance the symptoms of pulmonary affection had lasted on and off for eleven years; moreover, pulmonary mischief of a chronic character was revealed post-mortem. It cannot be denied, therefore, that pulmonary disease had probably preceded cardiac incompetence. There was, however, no emphysema or dilatation of tubes, and the condition of lung was neither sufficient to explain the patient's symptoms nor to account for the hypertrophy of the left side of the heart; there is no doubt, too, that latterly, at least, cardiac symptoms had predominated. I look upon the case, therefore, in its later stage as one essentially of cardiac incompetence. Yet, again, though the heart was hypertrophied, the valves seemed healthy. Although there was some albuminuria, and the kidneys were not perfectly normal, their condition was manifestly not one of primary disease, but such only as constantly arises in the course of chronic heart or pulmonary affections.

After careful perusal of the five cases just narrated, it will, I think, be generally admitted that no practitioner, however well-informed he might be, and however much alive to the difficulties which beset the diagnosis of cardiac affections, would have regarded them, prior to actual inspection of the heart, as other than well-marked, if not typical, examples of regurgitant organic disease of the mitral valve. The histories were in no respect incompatible with this view; the symptoms —shortness of breath, cough, with occasional slight hæmoptysis, anasarca, lividity, palpitation, irregularity of pulse—all pointed in the same direction; and the condition of lungs, liver, and kidneys, as observed after death, was in all cases more or less confirmatory of the same opinion. Add to which, there was in every instance a well-marked, and for the most part persistent, murmur audible solely or loudest in the usual situation of the apex of the organ, and distinctly attending its systole. Although, then, no actual organic mischief was discoverable in the mitral valve, it must be conceded, I think, that, from some cause or other, mitral incompetence had existed, and that to this incompetence the patients' symptoms and death were, to say the least, in great measure attributable.

It may almost be regarded as an axiom in medicine, that the presence of a systolic apex murmur is a positive proof of regurgitation through the *mitral* orifice. I have not hesitated to adopt it in reference to the cases already detailed. But occasional exceptions may present themselves. Regurgitant murmurs do, though with extreme infrequency, become developed in connexion with the tricuspid orifice. It is doubtless possible in some such cases to recognise the seat of murmur; yet, when we consider how nearly the points of greatest intensity of the murmurs produced at the auriculo-ventricular apertures coincide with one another, and, at the same time, how liable the heart is, from various causes, to undergo slight changes of position, we cannot but admit that there is room for occasional error. The following case, which I recorded in the eleventh volume of the 'Transactions of the Pathological Society of London,' at the same time illustrates the difficulty I speak of, and forms an interesting addition to the foregoing series. The patient, as will be seen, suffered from the usual symptoms of cardiac disease; and a permanent systolic murmur in the situation of the apex of the heart appeared to indicate the mitral valve as the part specially affected. All the valves were found after death structurally perfect; but while the cavity and walls of the left ventricle retained as nearly as possible their normal dimensions, the right ventricle had become greatly dilated, and its parietes hypertrophied. Moreover, the heart itself, owing probably to deformity of the chest, was so tilted, that the edge of the right ventricle projected nearly directly forwards, and the apex of this ventricle alone could have impinged on the thoracic parietes. Hence, and for reasons which will more fully appear in a subsequent portion of this paper, I was compelled so far to modify the opinion I had originally formed, as to refer the murmur which had been heard during life to the orifice of the tricuspid instead of that of the mitral valve.

CASE VI.—*Systolic apex-murmur; tilting forwards of right margin of heart; hypertrophy and dilatation of right ventricle; smallness of musculi papillares; apparently healthy state of tricuspid valve.*—J. Brownley, a hunchback of diminutive stature, aged twenty-four, was admitted under my care on the 27th of September, 1859. From his fifth year, at which time the angular curvature appeared, he had enjoyed constant good health until last spring, when he was attacked with small-pox, which kept him seven weeks from his work. Three weeks after resuming his occupation (now three months since) he perceived his ankles beginning to swell. The œdema soon became general, and his breathing difficult.

On admission, he was suffering from general anasarca, with lividity of the lips and fingers. He laboured under severe dyspnœa, increased by the recumbent posture, and complained of a short dry cough, coming on in frequent spasmodic attacks. His sleep was disturbed; his tongue somewhat dry and furred; his appetite was fair, his bowels natural, and his urine healthy; the pulse was regular, 108. There was a distinct but not very loud systolic murmur, loudest in the usual situation of the apex of the heart. The left side of the chest was generally dull, with some diminution of vocal fremitus, and loud, dry, crackling sounds were audible at every part; the right side was resonant, and the respiratory sounds were mixed with rhonchus and sub-crepitation.

While under treatment, he presented intervals of apparent amendment. No real improvement, however, took place; and he died on the 12th October, the cardiac murmur and the local pulmonary signs remaining audible to the last.

Post-mortem examination.—Of small stature; anasarcous; great angular curvature in the upper part of the dorsal region. *Chest:* The pericardium contained an ounce or two of serum; the heart looked unusually large, and weighed eleven ounces; it occupied its natural position in relation to the front of the thorax, but its right edge was tilted a little more forward than usual. The apex of the organ was bifid, the most projecting portion being the apex of the right ventricle. The left ventricle was very small, and its walls proportionately thin; the aortic and mitral valves were perfectly healthy-looking, and doubtless quite competent. The right ventricle was extremely dilated, its walls very firm, and at least as thick again as natural; the tricuspid and pulmonic valves appeared quite healthy, but the musculi papillares were small, certainly not hypertrophied in proportion to the dilatation of the cavity. The left auricle was small; the right, dilated and hypertrophied. The cavities on the right side were loaded with coagulum, those on the left nearly empty. Both lungs were universally adherent, the adhesions being somewhat œdematous, and, in the angle formed by the spinal curvature, particularly dense. The left lung was unusually small, very sparingly crepitant, generally congested, and more or less carnified. The right lung, also, was small, yet considerably larger than its fellow; it was crepitant, though less so than natural, and congested. The bronchial tubes were loaded with somewhat tenacious mucus, but otherwise seemed healthy. Several of the bronchial glands were converted into more or less friable earthy masses; one of which, irregular and angular in form, and about as large as a horse-bean, communicated freely, by an angular orifice of corresponding size, with one of the branches of the right bronchus. Larynx and trachea healthy. *Abdomen:* There were three or four pints of serum in the abdominal cavity. The viscera, with the exception of the spleen, which was enlarged, and the kidneys, which were congested, were healthy. There were no indications of abscess, old or recent, connected with the spinal curvature.

1. In attempting to elucidate the obscure class of cases which forms the groundwork of the present paper, the first point to be determined is, "What is the cause of the systolic apex-murmur?" In reply to this question I have little to say beyond what has been already incidentally adduced. I have shown that there were in all my cases not only the local signs commonly accepted as indicative of auriculo-ventricular regurgitation, but that both the general symptoms and the condition of internal organs (as revealed post-mortem) were such as characterize this form of disease. I have remarked that it may be regarded as an axiom, that the existence of a systolic murmur at the apex of the heart is a sure indication of incompetence of one or other of the auriculo-ventricular valves; and, that so rarely is this phenomenon manifested in connexion with the right side of the organ, that it might almost for practical purposes be accepted as the proof of mitral incompetence alone. This statement merely expresses the current doctrine of the day—a doctrine which no one will call in question, and one, indeed, which cannot be controverted without entirely upsetting the present well-established principles of cardiac pathology. If true, it leaves no alternative. I believe it true, and cannot therefore hesitate to put implicit faith in the explanation which I have given of the cause of the murmurs heard in the cases which have been adduced, and am quite satisfied that no new proofs are needed to insure for it general acquiescence.

2. Admitting incompetence of the auriculo-ventricular valves as the cause of the murmur in question, a second inquiry, of no less importance, but of much greater difficulty, presents itself, "On what circumstance does that incompetence depend?" In endeavouring to furnish a reply to this question it will be best to state and discuss *seriatim* the various theories which have been or may be suggested in

explanation, and to estimate as we proceed the relative and absolute value of each.

(*a*) *Spasm of the musculi papillares.*—The occasional presence of cardiac murmur in choreic patients is a well attested fact. That the murmur in many cases depends on organic disease, resulting from coexistent or antecedent rheumatism, is certain; that in some, when the murmur is systolic and at the base, it is due to the same cause (whatever that may be) which operates to the like end in cases of pure anæmia, may be admitted; but cases also now and then occur which neither of the above explanations satisfactorily covers. Thus, it is asserted that a systolic apex murmur sometimes appears in the course of chorea, which subsides as the patient's health becomes re-established. Mere anæmia cannot produce valvular *incompetence*, and organic disease can scarcely be conceived so perfectly remediable as to leave (except here and there in an isolated case) no clinical evidence of its existence. Hence it has been sought to explain such temporary mitral inadequacy by assuming a spasmodic or irregular action of the musculi papillares. It is doubtless quite conceivable that spasmodic action of these muscles would be competent to prevent the perfect coaptation of the edges of the mitral flaps. But such an explanation is quite a gratuitous one; choreic spasm does not affect the purely involuntary muscles; and even if it did, would scarcely limit itself to special regions of them. The heart gives no indication of being specifically influenced in this affection, its rhythm for the most part remains unimpaired; surely, then, there can be no grounds for assuming a spasmodic state of its papillary portion? We may remark, too, on the improbability of spasmodic action being so regular in its irregularity, and so continuous in its operation, as to produce the effects assigned to it in a uniform degree and over an extended period of time. But if spasm of the papillary muscles affords an insufficient explanation in cases of chorea, it is obviously an untenable hypothesis in reference to the class of cases now under consideration, where no general spasmodic tendency is present, and where, too, the murmurs frequently coexist with perfect regularity of the heart's action.

(*b*) *The presence of coagula in the cavities of the heart* has also been assigned as an occasional cause of regurgitant mitral murmurs; and the fact that such murmurs do now and then make their appearance in cases of other than cardiac diseases, during the period of impending death, gives countenance to this view. Unquestionably the coagula which form in the process of dying, prolonged as they generally are through the various cardiac orifices, must prevent complete closure of the valves; and we can conceive that bruits may hence occasionally arise. Murmurs thus produced, however, have obviously no relation to the subject in hand. But coagula of a different kind are not infrequently found, coagula which have evidently been formed some considerable time anterior to death, and to which, from their shape and character, such terms as globular concretions, softening clots, and so forth, are sometimes applied. It is impossible to say that these bodies can never be productive of cardiac murmurs. But when situated in the auricle, their usual seat is the auricular appendage; and when occupying the ventricle, they almost invariably affect its apical portion; and in neither of these situations does it seem to me that they can impede the tricuspid or mitral valve in the performance of its prescribed functions. Granting, however, their adequacy, from accident of position, to occasion in rare instances valvular incompetence, it is still easy to show that they have really no more bearing on the question at issue, than the ordinary coagula previously spoken of. We may test this assertion by the cases that have been already adduced. In two only out of the six (those numbered III. and IV.) were these globular concretions found, and in both they occupied the apex of the left ventricle. Now, in Case IV., it is tolerably certain that the clots, from the characters which they displayed, had been formed a few days only before death, whereas the murmur had been audible for at least a month previously; and although, in Case III., the coagula must have been in existence during the whole time the patient was under observation, there were such clear proofs of cardiac inefficiency, both in the history and in the morbid anatomy, as to render these bodies, to say the least, unnecessary to explain the phenomenon. If we add to this, that globular concretions are deposited more frequently in cases of heart disease* than in any other class of affections, it will be seen that we have fair grounds for regarding their presence in the cases just referred to as secondary and accidental; and that we are fully warranted in coming to the general conclusion which we have already expressed with regard to their irrelevance to the question before us.

(c) *Dilatation of the auriculo-ventricular openings.* — A far more probable explanation than either of the above, and one which has received considerable support, is that the regurgitation depends upon dilatation of the auriculo-ventricular orifices, and consequent insufficiency of the valves to close them. Dr. Peacock's careful researches on the dimensions of the heart† show conclusively that the auriculo-ventricular, as well as the arterial openings, exhibit great varieties of size in different individuals, both in health and in disease; and a very short experience of pathological anatomy will further convince any one that the former (and the tricuspid especially) are often dilated to a degree which cannot escape the notice of even the casual observer. Hence it becomes a natural inference that incompetence should occasionally arise from this cause. But in order to this it is essential that the valves themselves should have expanded, either not at all, or in a less ratio than the orifices they were intended to guard.

The suggested explanation is certainly a plausible one; but I am by no means satisfied that it affords the correct solution, and for the following reasons: First, it must be borne in mind that we are speaking of cases in which for the most part the orifices do not appear unusually large, and in which the valves themselves present a perfectly natural appearance. In the majority of my cases there was no noticeable enlarge-

* See Paper by the author on "Softening Clots in the Heart:" Transactions of the Pathological Society of London, vol. vii. p. 137.

† Monthly Journal of Medical Science. 1854.

ment of the auriculo-ventricular apertures, or disproportion between them and their valves; and in a carefully detailed case recorded by Dr. Gairdner in the paper before alluded to, the mitral orifice was found by actual admeasurement to be considerably within the limits which Dr. Reid assigns to the part in its healthy condition. Yet it must be allowed that in one of my cases (IV.) the mitral aperture was actually extremely large, though at the same time no relative smallness of the valvular flaps was even here remarked. Secondly, it is quite certain that the apertures of the heart vary considerably in dimensions in the case of persons who do not appear to have suffered at any time from cardiac symptoms, and in whom, therefore, they may be assumed to be in a healthy condition; in hypertrophy, again, whether simple or complicated with other morbid changes, great dilatation of certain of the orifices may be present without any evidence of regurgitation through them; so that the mere fact of unusual size of the openings must not be taken for more than it is worth. Thirdly, each aperture is surrounded by a ring of tough fibrous tissue, which, under any circumstances, must be slow to yield. It really is difficult to see what power there is, beyond that of growth and adaptation, which regulates *ab initiô* the relation between the parts, capable of affecting it in any material degree. And if it yields through any inherent or acquired deficiency of strength, the muscular tissue of the heart will doubtless have yielded from the same cause both previously and in a more than proportionate degree, and there will thus (as I hope to show) have been associated with dilated apertures a far more efficient cause of auriculo-ventricular regurgitation. Fourthly, considering how extremely difficult, if not impossible, it is to test post-mortem the efficiency of the valves between the auricles and ventricles, it seems fair, in reference to this question, to argue from the analogies afforded by those valves which do admit of having their competence or incompetence determined—namely, the valves situated at the arterial orifices. These, like the others, vary in size within certain limits; these also are surrounded by rings of fibrous tissue. Yet, notwithstanding that they are thus similarly circumstanced as regards their attachments, and similarly subject to the influence of dilating forces, I am not aware that there is any case on record in which, apart from organic disease or visible atrophy of the semilunar folds, mere unusual size of orifice has permitted of regurgitation. When to the several objections which have just been urged, I add my belief that a more efficient cause of incompetence can be shown to be in operation, I have said enough, I think, to justify the reiteration of my conviction that dilatation of the auriculo-ventricular apertures does not afford the true solution of the phenomenon of regurgitation in the class of cases to which my observations relate. Nevertheless, having regard to the nature of the evidence on which my opinion is based, I do not venture absolutely to deny that regurgitation, from this cause alone, may occasionally take place; still less to maintain that such abnormal dilatation may not play at times a distinct, though subordinate part, in association with other and more efficient causes.

(*d.*) The condition of parts to which I am disposed to ascribe the chief, if not the exclusive share in rendering the auriculo-ventricular orifices incompetent, is *disproportion between the size of the ventricular cavities and the length of the chordæ tendineæ and musculi papillares.* That this condition would be fully capable of producing such a result cannot be denied. It is self-evident that, if in the healthy heart a certain proportion between the several parts just named is requisite to insure accurate coaptation of the auriculo-ventricular valves, that proportion cannot be disturbed without preventing in some degree this due adjustment; that, if a ventricle becomes dilated without a corresponding elongation of the musculi papillares and their tendinous prolongations, its valve will be retained during the systole in a partially patent condition; and that equally, if the musculi papillares and chordæ tendineæ become from any cause reduced in length, while the cavity to which they belong retains its normal dimensions, incompetence will necessarily ensue. The point to be determined is whether any such disproportion can be shown actually to have existed in the cases before us. Let us examine, then, in reference to this question. In all of them there was some degree of hypertrophy, though in nearly every one the increase of muscular tissue was less than the apparent bulk of the organ would have led one to suppose. In four of them there was also marked and considerable dilatation, with actual or relative thinness of the ventricular wall; and in the remaining two, though dilatation is not expressly mentioned, there is no doubt that the cavities were at least proportional to the hypertrophied walls, and so considerably larger than they would have been in the healthy state of the organ. Hypertrophy, then, with more than commensurate dilatation of the ventricles, was a prevailing, if not a universal feature. Again, in three of the six cases it was expressly observed that the musculi papillares and chordæ tendineæ were small and on the stretch. Now, real or apparent atrophy of the parts in question is the natural sequence of simple dilatation. But in proportion as dilatation is accompanied by increase of the muscular substance of the heart, the papillary muscles, sharing in that increase, will give less and less distinct evidence of relative insufficiency. And yet so long as dilatation is in excess of hypertrophy, some such insufficiency will probably exist, and however small its degree, will, especially if conjoined with unnatural dilatation of the auriculo-ventricular openings, produce, or tend to produce, incompetency of the valves. But the eye is incapable of appreciating slight derangements of proportion between the parts of so complicated an organ as the heart, and hence slight deviations of relative size (however important in their results) in the papillary muscles and ventricular cavities would probably be invisible to the pathologist, even if his attention had been specially directed to the heart as the seat of disease. The mere fact, therefore, of such a disproportion not having been noted in any case, is no fair proof that none existed; and the fact of its recognition in any instance is a clear indication that it was considerable. Bearing in mind, then, that the condition was clearly marked in three out of six cases, and that, although it was not recognised in the others, the natural tendency of dilatation is to produce such a condition, it is not unfair to assume that it really did exist in some degree or other in every one of the series. When I add that the pathological records of the above cases were written by me at a time when I had no theory to maintain or to combat, and, in several instances, before any intimation of the cause of death had been conveyed to me, it will be seen that they do support in a very unmistakeable manner the theory which I have explained and am endeavouring to enforce. I may here adduce, in corroboration of my views, a case published by Dr. Hare in the 'Transactions of the Pathological Society' (vol. iii. p. 72), in which, with tricuspid murmur and regurgitation, there was no trace of valve disease, but great dilatation of the right ventricle and manifest atrophy of the musculi papillares. And I may state that in at least two or three other cases which have come under my notice, in which I have known that mitral regurgitation existed during life, but which, in consequence of no clinical notes having been preserved, have been rendered in great measure useless for my present purpose, the same remarkable smallness of the papillary muscles and their tendons was observed.

There is yet another condition, necessarily associated with dilatation of the ventricles and insufficient length of the chordæ tendineæ and musculi papillares, which has not been adverted to, but which doubtless aids in some degree in the production and maintenance of valvular incompetence—I allude to the lateral displacement of the origins of the musculi papillares in consequence of the rounded form which dilatation imparts to the heart; and to the altered direction therefore in which the force of these bodies has to be exerted. Dr. Hare specially refers tricuspid incompetence to this cause in the case already quoted.

In confirmation of the explanation which I have ventured to give, I may refer to Mr. Wilkinson King's well-known paper* "On the Safety-valve Function in the Right Ventricle of the Heart." He there attributes the regurgitation which, as a normal process, takes place occasionally through the tricuspid aperture, to temporary over-distension of the thin and yielding ventricular walls, and consequent displacement and insufficient length of the musculi papillares and chordæ tendineæ. He shows also how, from the close coaptation of the mitral valves, and from the comparatively unyielding state of the parietes, such regurgitation is impossible on the left side so long as the heart is in a healthy condition. But it is obvious that, as its cavity becomes enlarged and its walls comparatively thin, the left ventricle loses in some degree its own distinguishing features, and acquires in their place some of those peculiarities which naturally belong to its neighbour, and upon which its safety-valve function has been proved to depend. It must acquire with its peculiarities the liabilities which belong to them. And hence if we admit the truth of Mr. King's observations in reference to the healthy right ventricle, it is impossible not to admit their applicability to the dilated left; and it is equally impossible, I think, not to concede, in reference to both ventricles, that regurgitation, which may be

* Guy's Hospital Reports, No. 4. 1837.

occasional only so long as the dilatation is confined within certain moderate limits, will probably become permanent so soon as these limits have been transgressed.

3. It follows from the previous observations that, in those cases in which incompetence of the mitral valve exists, without apparent disease of the valve itself (and the same holds good with respect to the tricuspid), the incompetence, important though it may be in its results, is not the primary malady; but must have been preceded for a longer or shorter period by an abnormal condition of the cardiac parietes. This statement does not rest on theoretical grounds alone. Its truth is attested by such cases as that of J. Bunting, in which unmistakeable cardiac symptoms had existed long anterior to the specific signs of mitral regurgitation. But the best proof, perhaps, is furnished by the occurrence of cases resembling, in all essential respects, those which have been narrated, yet in which no cardiac murmur has at any time been recognised. Such cases are far from uncommon. I have met with many, but as they are foreign to my present purpose, refrain from introducing any of them here.

The essential morbid condition preceding and causing incompetence of the apparently healthy auriculo-ventricular valves, I have shown to be dilatation of the ventricles of the heart; and since this forms the very foundation of the class of cases of which this paper treats, although I have nothing of novelty to impart in reference to it, I should almost feel myself guilty of disrespect were I to dismiss it without bestowing on it a few remarks. Dilatation of the heart is, as is universally allowed, a result and an indication of weakness of its muscular tissue. Weakness alone, however, can scarcely produce dilatation. Whence, then, does it arise? It is easy to give an answer to this question that shall receive general acquiescence, but far less easy, I suspect, to furnish specific proofs of the various propositions which that answer may comprise. Debility of the heart, however excessive it may be, provided the duties which the organ has to perform are adjusted to its capabilities, will not, as the records of phthisis and of cancer show, be followed by unnatural distension. A far lesser degree of feebleness, however, in a heart which is called on to act beyond its strength, will soon succeed in producing greater or less dilatation. Hence we may expect this condition to arise in chronic forms of non-fatal cachexia, where the heart's action is unduly affected by mental conditions, or where it is over-stimulated by too laborious occupation; anæmia, syphilitic dyscrasia, chronic malarious affections, debility from over-work, and insufficient food, may be cited as examples. Hence, too, it may not improbably be engrafted on attacks of acute disease, where the heart has been disproportionately affected, and where its progress towards recovery has lagged behind that of the rest of the organism. I allude to such affections as typhus, in which the heart is notoriously weakened to excess; small-pox, in which I have seen extreme fatty degeneration, and which, I think, probably initiated the cardiac disease in the case of J. Brownley; eruptive fevers generally; diphtheria and purpura, in both of which latter excessive fatty degeneration may occur; and other blood-diseases which need not be specified. And hence also we may, perhaps, occasionally meet with it in cases where the circulation becomes obstructed independently of heart disease, as in chronic bronchitis and in morbus Brightii. Age alone has evidently little influence, for the affection occurs with nearly equal frequency in the young, the old, and the middle-aged.

It will, perhaps, be asked whether these dilated hearts are in a state of fatty degeneration. So far as the cases which have come under my notice are concerned, I may reply by an unqualified negative. In every instance the muscular tissue presented to the naked eye a perfectly healthy appearance, and in those cases in which it was subjected to microscopic examination the fibres exhibited slight, if any, deviation from the normal state. It should be borne in mind, however, that extreme feebleness and atrophy of voluntary muscular tissue may occur without any apparent structural change of the ultimate fibres, and there can be no doubt that the muscular tissue of the heart is also liable to be thus affected. It should also be borne in mind that unnatural thinning is virtual weakness; and that if dilatation and thinning have been produced by any temporary impairment of the ultimate tissue of the heart, these conditions are liable not only to continue, but even to progress, after the ultimate fibres themselves have resumed their healthy state. We must not, therefore, assume that the heart's tissues are really healthy because they look so; yet, neither must we take it for granted that they are really unsound because the presence of dilatation attests that unsoundness once existed.

ART. V.

Experiments on the Chromic-acid Test for Alcohol.
By J. HALL SMITH.

THE following experiments were undertaken in Dr. Chambers' Laboratory at St. Mary's Hospital, with the view of testing the applicability of the process proposed by Messrs. Lallemand, Perrin, and Duroy, for the detection and comparative quantitative estimation of alcohol in organic tissues and excreta.

An apparatus similar to that figured and described by the authors was fitted up, the principal differences being, that the aspirator was placed last instead of first in order, so that the air was drawn instead of forced through the apparatus. This form was adopted, as it was found that air passed through a caoutchouc tube turned the test-liquid first green and then brown, from the deposition of carbon. This arrangement also prevented the possibility of the escape of vapours through imperfections in the corks or joints, as, if such existed, a current inwards would be established. Another modification was, that the air was made to traverse a tube containing test-liquid before passing over the substance under examination. This arrangement served to show the action of air alone upon the test-liquid, and to prevent vapours capable of changing the test-liquid from entering the apparatus.

The experiments of a negative character gave the following results:

Air alone passed through the test-liquid, which has a bright orange-red colour, causes it to assume a dark greenish-yellow colour. This effect is scarcely perceptible within one hour.

Air carrying vapours from cerebral matter or from urine through caustic lime, does not affect the test-liquid more than air alone.

Other experiments showed that after the ingestion of small quantities of alcohol, that body can be readily detected in the breath and in the urine.

The experiments cited by the authors give no grounds for regarding the test as suitable for the comparative estimation of alcohol, and a few experiments made for the purpose of testing its applicability on this point, gave results by no means leading to such a conclusion.

To ten c. c. of test-liquid in a tube upon which a stream of cold water was running to prevent rise in temperature, a very weak solution of alcohol was added from a burette. Fourteen measures of this solution were required to induce exactly the same green tint as was possessed by a portion of test-liquid to which an excess of alcohol had been added, but the tint produced by ten measures was only to be distinguished from it by careful comparison against a white ground. Another experiment gave the same result.

Fifty burette measures of this weak solution of alcohol, when placed in the apparatus, served to give a green colour to three successive portions of test-liquid of two c. c. each. The shades of green were not the same in each case.

One hundred burette measures of the alcoholic solution converted seven successive portions of two c. c. each of the test-liquid to a light green colour, the seventh only differing slightly in tint from the others. Of these, the last three portions were converted by the alcohol retained by the lime, the flask having become perfectly dry, and the lime requiring the application of considerable heat to drive off the vapours of alcohol.

Twenty burette measures of the alcoholic solution served to turn five portions, equal to ten c. c. of test-solution, of a light green colour, though not the same tint as obtained by the direct addition of alcohol to the test-liquid. In this case also the greater portion of the alcohol had to be driven from the lime after the flask was dry.

A number of other experiments point in the same direction. In order to drive the alcohol from the lime, it is necessary to apply such heat to it as drives off water, and sometimes organic matter, which, by giving a different colour to the test-liquid, renders it impossible to determine the end of the reaction. The time required when any great bulk of matter is to be operated upon, as in the case of urine, &c., is not the one hour and a half or two hours mentioned by the authors, but in some cases the process took as much as two days, and in consequence of the necessity for removing the lime, slaked from time to time, the results were useless, as perhaps as much alcohol was retained by the slaked lime as passed through it.

[We regret that the correction of the proof of this article is rendered impossible by the death of the author.]

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON PHYSIOLOGY.

BY HERMANN WEBER, M.D.
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I. GENERAL PHYSIOLOGY.

1. R. VON VIVENOT, Jun.: *On the Influence of Change of Atmospheric Pressure on the Human Organism.* (Virchow's Archiv, vol. xix., p. 492, 1860.)
2. W. W. GULL: *An Oration delivered before the Hunterian Society, London,* 1861.

1. VIVENOT has examined the phenomena observable in man when exposed to sudden alterations of atmospheric pressure. Rapid and considerable *diminution* of pressure,—as experienced by the ascent in the air-balloon to the height of about 18,000 feet above the sea, or by the climbing of mountains of a similar elevation, connected with a reduction of pressure to one-half of what we are generally exposed to,—is found to cause: 1. Increased evaporation from the lungs and surface of the body; 2. Increased oxidation, in consequence of the loss of warmth occasioned by the augmented evaporation; 3. Increased frequency of respiration and pulsation, the former being induced as well by the smaller amount of oxygen contained in a certain volume of air, as also by the greater amount required in the formation of heat; 4. Congestion of blood towards the peripheric parts, and in consequence dilatation and even rupture of bloodvessels; 5. Diminished approximation of the corresponding portions of joints; 6. Diminished secretion of urine, as an effect of the increased evaporation. The observations on *increased* atmospheric pressure have been made in Tabarié's air-compressing apparatus, as it is used for therapeutic purposes at Nice, Montpellier, and other places, under the name "*bain d'air comprimé.*" The author's inferences are based on researches made at Nice on himself and four other male individuals. For the description of the apparatus we refer to the essay itself. The experiments were made on eight successive days, between the hours of 12 at noon and 2 P.M., before the principal meal, the mean temperature being 11° Cent. (51·8° Fah.), the atmospheric pressure in the apparatus amounting to $1\frac{1}{5}$ atmospheres (925·04 millimetres* mercury). The effects of the compressed air were: 1. A decided decrease in the frequency of pulsation; the greatest decrease being 18 beats per minute, the average 10 beats. 2. Diminution of the number of respiratory movements, amounting in the mean to 1·1 per minute. The influence, as well on the pulse, as also on the respiration, lasted beyond the time of the experiment; it seemed to be greater where the frequency of pulse and respiratory movements were morbidly increased. 3. Diminished evaporation from the surface of the skin and lungs. 4. Increased

* A millimetre=0·03937 inch, or 0·47 line.

secretion of urine. 5. Repulsion of the blood from the periphery, especially checking of congestive conditions in the periphery. The author further expresses the opinion, that the slight variations of atmospheric pressure occurring under usual circumstances, with change of weather, are of little influence on our feeling of health and strength, and that the phenomena frequently ascribed to these slight changes are due to other influences, as temperature, moisture, and winds.

2. The important question, "What is life?" has been treated on by Gull in his oration before the Hunterian Society. "Daily advances in science," he maintains, "make it more and more probable that organized beings *are the necessary development of the physical conditions of our globe.* The elements of which they are composed are remarkable for their ordinary physical and chemical properties. They are such, indeed, as a knowledge of these properties alone would have led us to select, had we proposed to ourselves the construction of a being endowed with ordinary animal capabilities." The author allows that the ultimate changes in our nervous system connected with sensation are obscure, but points out that the first steps of the process are according to the rules of physics, and thinks himself entitled to "infer that 'physics' prevail at the centre also." Gull then shows that the surest plan for us is to study the material relations between vital and physical forces, alluding to the ingenious views of Carpenter, Hinton, and Leconte. He gives several instances of chemical affinity acting as a source of organizing power. Thus he suggests that the "decarbonization of the blood, by the action of oxygen upon it, though serving for the production of animal heat, and the removal of effete materials, may have the higher purpose of yielding force to the blood itself, for the maintenance of its vital state." For our food the author assumes, besides the two uses generally acknowledged—*i.e.*, nutrition and calorification, a third use—viz., the production of an upward force, as the food, by its oxidation, falls into a lower plane. The fact that exercise of our organs leads to their increased growth, is likewise explained by the view that through the decomposition of the tissue fresh organizing force is supplied.

II. Food and Digestion.

1. *Food—What it is.* (The Cornhill Magazine, No. xvi., p. 460, 1861.)
2. Mège-Mouriès: *On Wheat and Wheaten Bread with regard to Public Wealth and Health.* (Compt. Rend., vol. l., p. 467, 1860.)
3. G. Meissner: *Researches on the Digestion of Albuminous Substances.* (Zeitschr. für Rat. Med., vol. x., p. 1, 1860.)
4. Davidson and Dietrich: *Contribution to the Theory of Digestion* (Professor Heidenhain's Communication from the Physiological Institution at Breslau, Reichert and Du Bois-Reymond's Archiv, 1860, p. 688.)
5. W. Braune: *A Case of Preternatural Anus, with Contributions to the Physiology of Digestion.* (Virchow's Archiv, vol. xix., p. 470, 1860.)

1. The author of 'Food—What it is,' gives a popular description of some of the principal properties of various articles of food, but in doing so, he alludes to some actions of food which hitherto have not been sufficiently appreciated. Thus he says with regard to the use of sugar: "If a solution of sugar is kept in a warm atmosphere, exposed to the air, and a small quantity of yeast be added, we know that it ferments; the sugar resolves itself into carbonic acid and alcohol, *and the yeast grows.* There are two opposite processes going on together and mutually connected, the decomposition of the sugar (which is attended with the absorption of oxygen), and the growth of the yeast." The yeast, the author continues, is albuminous, and it grows on the decomposition of sugar. May it not be, he asks, that in our bodies, too, the albuminous,

yeast-like elements of our food grow and develop into more life, while the sugar and the analogous substances are burnt away? Sugar and food in general would, therefore, besides the usually mentioned properties of nutrition and calorification, have another action—viz: that of setting free of "upward force." "Food is force. The transference of vegetable matter to the animal, in its eating, is like the placing of a tense spring within a watch. The animal structure is the mechanism, and the force, which is in the food, operating through it, produces the animal functions as its results."

2. Mège-Mouriès communicates the results of his researches on wheat, wheaten flour, and the process of making bread from it. He gives a description of the different parts of the wheat-grain, and shows that by the usual way of preparing white bread a considerable portion of the most nourishing substances of the wheat-grain is lost, a part of them being removed with the bran, another part destroyed by the process of baking. The author calculates that by the adoption of the proceedings he proposes, France would gain every year about two hundred millions of francs, and the quantity of the so-called best flour ("farine de première qualité") would be increased by sixteen per cent.

3. Meissner continues his researches on the digestion of the albuminous substances. In the present essay he occupies himself with the Syntonin and Casein. The former of these bodies is divided under the influence of artificial digestive fluid (pepsin dissolved in diluted hydrochloric acid), into three different albuminous substances—viz., pepton and metapepton, which are soluble in water, and parapepton, which is insoluble in water. In his first essay,* Meissner mentions only the pepton and parapepton as the constituents of albumen, but since then he has found also a small proportion of metapepton in it. The metapepton differs from the pepton by being precipitated from a neutral solution by the addition of a very small quantity of hydrochloric or acetic acid, amounting to less than 0·1 per cent., being again dissolved by the addition of a larger quantity of acid. Metapepton is also less soluble in water than pepton. The casein is divided by the process of digestion into four albuminous bodies—viz., pepton, metapepton, parapepton, and dyspepton. The last substance seems to be peculiar to casein; it is intimately combined with a small proportion of a neutral fat, which can be separated by either; it is insoluble in water and alcohol and very diluted acids, but is dissolved by more concentrated acids. Diluted solutions of soda and potash dissolve it easily; the neutralization of the alkaline solution causes the precipitation of the substance as a fine powder. For further chemical characters of the dyspepton we must refer to the original, as also for the proportion in which the different kinds of pepton are obtained from the syntonin and casein. The continued influence of boiling water appears to cause the same alterations in syntonin and casein as the digestive fluid, the action of the latter being, however, much more rapid than that of the former.

4. Davidson and Dietrich's experiments on artificial digestion, made under the guidance of Professor Heidenhain, have shown, that powerful digestive fluid may be prepared not only by the hydrochloric and lactic acids, but also by the nitric, phosphoric, oxalic, tartaric, and acetic acids; that, however, the degree of concentration most active in the digestion of coagulated albuminous substances is different for the different acids. Instead of the usual pepsin the mucous membrane of the stomach of the frog was used, and found to possess energetic digestive properties. Heidenhain is of opinion that C. Schmidt's theory, according to which the acid and pepsin form a conjugated acid, which effects the solution of albumen, is disproved by the authors' researches, as the greatest degree of efficacy of the digestive fluid prepared by different acids is not obtained by adding the acids according to their equivalents, but that the quantity of acid most useful is independent of any such principle. Heidenhain himself

* Conf. this Journal, No. 49, p. 227, 1860.

inclines to the view, that the function of the acid is merely to expand the albuminous substances, and thus render the interstices accessible to the pepsin, the latter being the true solvent.

5. W. Braune has had the opportunity of examining the phenomena of digestion in a fistula of the ileum, or rather a preternatural anus, situated about twenty-four centimetres (rather more than nine inches) above the ileo-cæcal valve. The fistula was the consequence of a neglected or maltreated hernia near the umbilicus, in a woman, aged forty-nine. The communication between the upper and lower portion of the intestinal tract was completely closed. The chyme issuing from the fistulous opening after meals was, in general, rather thin, had an acid smell and a distinctly acid reaction, while the mucous membrane of the ileum turned the red litmus-paper blue. During fasting the reaction of the chyme was neutral. The chyme contained no cholepyrrhin, but a considerable quantity of biliary acids; there was no proof of unchanged casein, while Busch,* in his case of fistula of the jejunum, distinctly noticed it; sugar was absent; uric acid was not found.. The aqueous extract of the chyme exercised no influence on coagulated albumen. The excretions from the anus consisted only of mucus and epithelium, occurring about once in four weeks, and emitting a very fœtid smell. With regard to the time elapsed between the ingestion of food and the appearance of the remains in the chyme, the first portions of meat were recognised after three hours, and no more traces were found after six hours from the meal; the skins of grapes, when taken fasting, were passed after three hours five minutes, when taken with other food, after three hours and three-quarters. In Busch's case, where the fistula was in the commencement of the jejunum, the first portions of meat appeared from between twenty-two to thirty minutes after the commencement of the eating; the passage from the jejunum to the lower portion of the ileum appears therefore to require between two hours and three-quarters and five hours and a half. The *temperature* in the intestine was always rather higher than in the axilla. After meals there was constantly a perceptible increase of temperature as well in the intestine as also in the axilla, the greatest increase amounting to 0·9° F.

III. Respiration and Circulation.

1. H. H. Salter: *The Nature and Cause of the Respiratory Murmur.* (Lancet, vol. ii. 1860, p. 462.)
2. L. Mandl: *Researches on Pulmonary Osmosis.* (Compt. Rend., vol. l., p. 645, 1860.)
3. L. Mandl: *On Pulmonary Osmosis, or Researches on the Absorption and Exhalation of the Organs of Respiration.* (Arch. Gén. de la Méd., vol. ii., p. 49, and p. 161.)
4. J. H. S. Beau: *Experimental Researches on Death by Submersion.* (Compt. Rend., vol. l., p. 1019, 1860).
5. G. B. Halford: *On the Time and Manner of the Closure of the Auriculo-Ventricular Valves.* (Reprint from Med. Times and Gaz., No. 568, vol. i. 1861.)
6. J. C. Dalton, Junr.: *The Physiology of Circulation. A Course of Lectures delivered at the College of Physicians and Surgeons, New York.* (The American Medical Monthly, Extra, April, 1860, p. 321.)
7. F. Goltz: *On the Interpretation of the so-called Automatic Movements of the Cut-out Frog's Heart.* (Virchow's Archiv, vol. xxi., p. 191, 1861.
8. A. Chauveau, G. Bertolus, and L. Laroyenne: *Rapidity of Circulation in the Arteries of the Horse.* (Brown-Séquard's Journal de la Physiologie, vol. iii., p. 695, 1860.)

* Conf. this Journal, No. 45, p. 228, 1859.

9. J. Marey: *Researches on the Form and Frequency of the Pulse by Means of a New Sphygmographion or Registering Apparatus of the Pulsations.* (Compt. Rend., vol. l., p. 634, 1860.)
10. H. Luschka: *The Thoracic Part of the Inferior Vena Cava in Man.* (Reichert and Du Bois-Reymond's Archiv, 1860, p. 624.)

1. Salter, after having alluded to the views of other observers, and to the anatomy of the lungs, arrived at the following conclusions with regard to the nature and cause of the respiratory murmur:—"1. That the air-cells are structurally incapable of producing a respiratory murmur by their slight dilatation. 2. That the respiratory murmur is essentially a fine-tube sound. 3. That the lobular passages and ultimate bronchial radicals are probably its immediate seat. 4. That while the lung parenchyma, from its heterogeneous constitution, completely muffles all sound, the unbroken column of air in the bronchial tree is an excellent conductor; that thus the respiratory murmur has a double cause, and is in part the result of air-and-wall friction at the spot, and in part the bronchial sound of the larger tubes (although essentially modified) conducted by the unbroken column of air to the ultimate bronchial twigs. 5. That the restriction of the sound to inspiration (excess of inspiratory over expiratory sound) depends mainly on convection, but probably also in part on the great amount of air-and-wall friction produced by the impact of the air at the points where the tubes divaricate."

2 and 3. The frequent coincidence of diabetes and pulmonary tubercles has led Mandl to perform experiments on the influence of solutions of sugar and some other sweet substances (cane sugar, glycose, sugar of milk, glycerine, and mannite) on various species of aquatic animals, as infusoria, annelida, crustacea, water-insects, fishes, and batrachia. The results are:—1. That all aquatic animals perish in solutions of sweet substances; there is, however, a difference as to the time of death, depending on the degree of concentration of the solution, on the quality of the substance contained in the solution, and on the species of animal. Thus, the infusoria died almost instantaneously in solutions of 20 per cent. of cane sugar, glycose, glycerine, and mannite, but lived about five minutes in a concentrated solution of sugar of milk; they died after six or eight minutes in a solution of 4 per cent. of mannite, while they lived three times as long in a solution of equal strength of cane sugar. Some fishes (12 to 15 centimetres long) perished in a solution of glycerine of 10 per cent. after about forty minutes; in a solution of cane sugar of the same strength only at the end of four or five hours. 2. That the death is not caused by a chemical action of these substances, or by fermentation, or absence of air, or by the viscosity of the fluid, but only by the osmotic action (endosmosis and exosmosis) exercised by the solutions of the sweet substances. 3. That this osmotic process takes place through the permeable membranes, and especially through those of the respiratory organs. The not fermentable sweet substances possess a greater osmotic power, and act also more rapidly on the life of the animals. 4. Experiments made with animal membranes, to demonstrate which of the elements of the blood pass over into the solution of sugar, have shown that the first to pass over is water charged with the salts of the serum, after which follows albumen, the colouring matter being the last. The passage of the albuminous substances is retarded by greater concentration of the osmogenous substance. Mandl promises to communicate in another paper the results of his injections of solutions of sugar into the respiratory organs, with regard to the formation of tubercles of local nature, in opposition to the tubercles resulting from the tubercular diathesis. In the present essay, the author points out how the osmogenous power of sugar explains various physiological and pathological phenomena, as the sensation of thirst after the ingestion of sugar; the antiseptic power of sugar; the digestive property of small

quantities of sugar, causing exosmosis of gastric juice; the absorbing power of large quantities of sugar in dropsies; the great thirst of diabetic individuals; the impossibility of serous effusions in such patients. To the great osmotic action of the glycerine, the author ascribes also the influence of this substance as a local remedy.

4. Beau has performed on dogs several series of experiments, to elucidate the cause of death by submersion, especially with the view to explain the fact, that the air-passages of drowned animals contain only a very small quantity of fluid. The inference is, that the immersion of the natural orifices of respiration causes, "by sympathetic or reflex action, spasmodic occlusion of the sphincters or orifices of respiration, and stopping of respiratory movements." The death from drowning, the author argues, is therefore analogous to that from tetanic affection of the muscles of respiration. The two principal experiments are, that of two animals, in whose opened trachea a canula has been inserted; the lungs of the one which had been entirely submerged under water contain only the usual small quantity of frothy fluid; while the air-passages of the other, whose head had been kept out of the water, are filled with that fluid.

5. According to G. B. Halford, the auriculo-ventricular valve is closed already before the commencement of the ventricular systole. "Immediately the auricle contracts," the author states, "its contained blood passes into (distending and lengthening) the ventricle; the force which it transmits not being sufficient to overcome the arterial pressure and weight of blood upon the upper or arterial surface of the semilunar valves, is expended in distending the ventricle and closing the auriculo-ventricular valve, which then forms one of the walls of the ventricle. To this succeeds the ventricular contraction; the auriculo-ventricular valve being already closed, now becomes tense, the pressure in the ventricle overcomes that in the artery, and the semilunar valves are raised." This description is in exact harmony with that given by Dalton in his lectures presently to be mentioned.

6. Dalton gives, in his course of lectures just alluded to, a very lucid description of the phenomena of circulation in their connexion with various other functions and conditions of the body. The first lecture is introductory, treating on the nature of physiological studies, the methods of investigation, the complication of physiological phenomena, the rules for conducting physiological experiments, and similar questions. In the second lecture, the author considers the action of the heart, the form and position of the ventricles, the structure and action of the ventricular valves, the difference between the mitral and tricuspid valves, the closure of the valves from behind forward, the action of the arterial valves, and the position of the different parts of the heart in the chest. With regard to this last question, Dalton observes that it is impossible to judge of the natural situation of the thoracic viscera by inspection, after the chest has been opened in the usual manner, as practised in post-mortem examinations; for by the collapse of the lungs, the support naturally afforded by these parts is taken away, and the heart necessarily falls out of position. The method of fixing the organs, by thrusting long needles through the integuments in various directions into the chest before opening the thorax, gives, according to the author, likewise only an imperfect view; he prefers that of tying the trachea before opening the chest, and thus preventing the collapse of the lungs. From examinations conducted in this manner, Dalton finds "that the ascending portion of the arch of the aorta lies nearly in the median line. The bases of the aorta and pulmonary artery, and of course the aortic and pulmonary valves, are situated almost exactly in the median line, at the level of the third costal cartilage or the third intercostal space. The base of the aorta, where it begins to show itself above the body of the heart, is behind and a little to the right; while the upper part of the conus arteriosus

and base of the pulmonary artery are placed in front and a little to the left." Of the heart proper, the anterior surface of the right ventricle "occupies the median line, and lies directly behind the whole width of the sternum, and is placed, moreover, on a plane altogether posterior to that of the ventricle. The body of the auricle, in fact, does not come up at all in contact with the sternum; and it is only a portion of the appendix auricularis that overlaps, from the right, the base of the ascending aorta."

It is principally from the consideration of the natural position of organs, as described in the preceding statement, that Dalton maintains that the pulsating tumour observed in the sternal fissure of the well-known Mr. Groux is not, as stated by the majority of observers, the right auricle, but the right ventricle, the tumour being situated rather to the left of the median line, and its contraction being, besides, synchronous with the impulse of the heart.

In the third lecture the author describes the movements of the heart during life, the appearance of the lungs in respiration, the visible alteration in the colour of the blood in passing through the lungs, and the effects of stopping the respiration on the blood, the heart, and the great vessels. The order of succession and the mechanism of these changes are delineated, as they show themselves in dogs poisoned by woorara, the circulation having been kept up by artificial respiration. Dalton divides the changes into two stages. First stage: As soon as the lungs are allowed to collapse there is a *very slight* swelling of the cavities of the heart; the blue or venous blood is seen to pass as such through the capillaries of the lungs and through the pulmonary veins into the left side of the heart. There is, therefore, no stoppage of the blood in the capillaries of the lungs, but a passing through of unarterialized blood. Second stage: As soon as the venous blood is transmitted through the arteries into the capillaries, the arterial system and the ventricles of the heart become excessively distended; but the distension of the arteries and the aorta distinctly precedes that of the ventricles. The lecturer infers from this, that the impediment in the circulation is not in the lungs, but in the capillaries of the body; that the distension of the cavities of the heart does not, as frequently described, begin on the right side, but on the left. This view is strengthened by the fact that the venous system, especially the venæ cavæ, are at first not enlarged, but rather scantily supplied with blood. Dalton then states what further changes take place at the moment of death, when the heart actually ceases to pulsate. At the moment of death the arterial system is distended, not only in consequence of the difficulty in the passage of the venous blood through the capillary system, but also in consequence of the contractions of the left ventricle forcing additional quantities of blood into the aorta. As soon, however, as the heart begins to be paralysed, the engorgement of the arterial system diminishes, while that of the heart increases. The arteries, by the elasticity of their own walls, gradually force the venous blood through the capillaries, and return to their original size, and even become smaller. The left auricle in the last stages of the heart's action diminishes sensibly in size, while the right remains distended, and the venæ cavæ become filled with blood. The pulmonary artery remains distended longer than the aorta, but finally contracts likewise. Later, a post-mortem contraction of the heart, similar to the cadaveric rigidity of other muscles, takes place, by which the cavity of the left ventricle, which at the time of death was distended, is more or less completely obliterated, and that of the right perceptibly diminished.

The fourth lecture is devoted to the consideration of the rhythmical character of the heart's action, which is regarded as not dependent on the blood, or on the nerves or other stimuli, but on peculiar vital endowments inherent in the organization of its muscular fibres. The influence of the nervous system, especially the pneumogastric nerve, the effect of narcotic poisons, of ether and chloroform, and the action of hydrocyanic acid and of the different kinds of Woorara, are likewise treated on in this lecture.

8. Chauveau, assisted by Bertolus and Laroyenne, has examined, by means of a new hemadromometer, the motion of the blood within the arteries of the horse. The authors give a description of the instrument used and of twenty-five experiments, which lead to the following results. The velocity of motion of the blood in the arteries offers considerable variations during the different periods of which one contraction of the heart is composed. 1. In the large arteries, as in the carotid, the blood exhibits in the moment of the ventricular pulsation a rapid motion, equal to about 52 centimetres* in the second. At the end of the ventricular systole, in the moment which precedes the closure of the sigmoid valves, the velocity of the blood's motion decreases rapidly, becoming even equal to zero. In the moment of the closure of the sigmoid valves, the circulation experiences a new acceleration to an average velocity of 22 centimetres per second. After the closure of the sigmoid valves, the acceleration imparted to the motion of the blood by the dicrotic pulsation, due to the closure of the aortic orifice, decreases in general rather slowly. At the end of the period of rest of the heart, in the moment immediately preceding a new ventricular systole, the average velocity amounts only to about 15 centimetres per second, the motions seeming sometimes altogether arrested 2. In the arterial ramifications, farther removed from the heart, the circulation is always comparatively more active than it is in the large arteries during the ventricular diastole, and the acceleration imparted to the blood by the pulsation of the heart is comparatively much more feeble. The impulse coincident with the secondary pulsation (due to the closure of the valves) is still less perceptible, and is sometimes altogether wanting. 3. The state of activity of an organ causes a considerable increase in the velocity of the blood in the arteries supplying that organ. Thus, while the animals feed, and the masticatory muscles and the salivary glands are in activity, the carotid conveys five or six times as much blood as, during the same space of time, while these organs are at rest. 4. The phenomena of the circulation are considerably changed during arterial hemorrhages; they can therefore throw no light on the state of the circulation in the unopened artery. 5. The section of the pneumogastric nerves does not cause any other alteration in the arterial circulation than that resulting from a more rapid succession of the heart's movements. 6. Section of the sympathetic nerve seems, by paralysing the coats of the vessels and by dilating the capillaries, slightly to accelerate the circulation in the large arteries; but this acceleration is scarcely to be compared with that induced by the functional activity of the organs. 7. The circulation becomes always much accelerated by section of the spinal marrow between the atlas and the occipital bone. 8. The rapid increase in the velocity of the circulation, caused by the functional activity of organs, is always associated with diminished tension in the arterial system.

9. Marey describes a new sphygmographion, a modification of Vierordt's instrument, showing more distinctly not only that the period of rising of the pulse is much shorter than that of sinking, but also that the different moments of a single pulsation exhibit an infinite variation.

10. Luschka gives a description of the relations and dimensions of the thoracic portion of the inferior vena cava, as he thinks that pressure on this portion, especially in cases of hypertrophy of the right ventricle, is often the cause of hyperæmia of the liver, and œdema of the lower extremities. The termination of the inferior cava is marked by the Eustachian valve, and the posterior inferior portion of the septum of the auricles; the commencement of the thoracic portion by the foramen quadrilaterum in the tendinous centre of the diaphragm. The average length of the right and posterior part of this

* A centimetre = 0 3937 inch.

thoracic portion is 3·8 centimetres, that of the anterior and left part only 2·2 centimetres. The average breadth amounts to 2·7 centimetres, the circumference to 9 centimetres, while the breadth of the superior vena cava near its termination is only 2·2 centimetres, and the circumference 7 centimetres. In infants, Luschka states, the length of the thoracic portion of the inferior vena cava is very limited, the auricle commencing almost immediately above the foramen quadrilaterum; in the adults, on the contrary, the dimensions just given are frequently exceeded. To this difference, caused by age, the author ascribes the discrepancy in the statements of various anatomists on this subject (Haller, Portal, Winslow, Krause, Arnold.)

Luschka gives also a careful account of the arrangement of the muscular fibres in both auricles, and describes a small muscular fasciculus connecting the left auricle with the inferior vena cava, a fasciculus which appears to have escaped the notice of others. To these muscular fibres the author ascribes the function of stretching the walls of the vein and thus preventing the closure of the passage. In some instances an excessive development of this fascicle may occur.

IV. Secretion; Metamorphosis of Matter; Animal Heat.

1. V. Friedländer and C. Barisch: *Contribution to the Knowledge of the Secretion of Bile.* (Professor Heidenhain's Communications from the Physiological Institution at Breslau; loc. cit. p. 646.)
2. F. W. Pavy: *Contribution to the Physiology of the Liver.—Influence of Alkalies.* (Proceedings of the Royal Society, vol. xi. p. 90, 1861.)
3. W. Roberts: *On the Estimation of Sugar in the Diabetic Urine by the Loss of Density after Fermentation.—Manchester,* 1861. (From the 'Memoirs of the Literary and Philosophical Society of Manchester, 3rd series, vol. i.)
4. Liebermeister: *Physiological Researches on the Quantitative Alterations in the Production of Heat.* (Reichert and Du Bois-Reymond's Archiv, 1860, p. 520 and p. 589.)

1. Friedlander and Barisch have examined, under Professor Heidenhain's superintendence, the secretion of bile in guinea-pigs, by means of establishing fistules of the gall-bladder. The colour of the bile was light amber, the reaction alkaline, the percentage of solids smaller than in any other animal hitherto examined, varying from 1·23 to 1·35 per cent.; the presence of the usual biliary acids could not be proved by Pettenkofer's test. The quantity of the bile secreted was very large, amounting for 1000 grms. guinea-pig to 175 84 grms. in twenty-four hours, or 7·326 grms. per hour, and for 1000 grms. liver of guinea-pig to 185·54 grms. per hour. The liver of the animals in question is large, its weight bearing to that of the whole body the proportion of 1 : 27·3, in the average of seven animals. The animals took no food after the operation, yet there was no decrease in the secretion of bile, corresponding to the increased duration of abstinence from food. These results, compared with those obtained by Bidder and Schmidt on rabbits and sheep, of which animals the former secretes for one kilogramme of body only 5·07 grms. per hour, the latter, even as little as 1·109 grms., lead the authors to infer that with the increasing weight of body of the animal, the proportion of the fluid bile-secretion becomes diminished. They remark at the same time that the weight of the liver in proportion to that of the body is smaller in the heavier species of animals, being in the rabbit 1 : 33·5, and in the sheep 1 : 53·57. The amount of solids contained in the bile is, on the contrary, larger in the rabbit than in the guinea-pig, being in the latter for one kilogramme per hour 0·092 grm., in the rabbit 0·103 grm., and in the sheep 0 0672 grm. All the animals operated upon died within twenty-four hours. For the authors' re-

searches on the amount of pressure under which the bile is secreted, we must refer to the essay itself.

2. Pavy has examined the influence of injections into the jugular vein of carbonate of soda in solution, on the appearance of diabetes after section of the sympathetic nerve on the neck. He injected in other experiments solutions of carbonate of soda into the stomach and into the portal vein. The principal inferences of the author are: 1. The introduction of carbonate of soda into the circulation prevents the production of saccharine urine, after lesions of the sympathetic nerve, otherwise occasioning it. 2. Carbonate of soda, injected into the general venous system, does not prevent the urine from becoming saccharine after the destruction of life, when the circulation is kept up artificially; but injected into the portal system, so that all may pass into the liver, it has the effect of keeping the urine entirely free from sugar. 3. Carbonate of soda injected into the portal system during life, causes a rapid disappearance of hepatine from the liver, without any sign of the production of sugar. 4. In the disappearance of hepatine under the influence of carbonate of soda, the hepatine is not concealed, nor transformed into sugar, nor destroyed by any direct chemical power, but may be transmuted by a process analogous to catalysis, into another substance which, as yet, has escaped discovery. 5. Carbonate of soda injected into the liver after death, does not effect a disappearance of hepatine, but even when injected in moderate quantity, holds the saccharine metamorphosis completely in check. The author farther suggests that there is a close connexion between the disappearance of hepatine, the production of fat, and the state of the bile.

3. The fact that the specific gravity of saccharine urine is considerably reduced by fermentation, from between 1030 and 1050 to 1009, and even a much lower density, and the consideration that "the diminution of density must be proportional to the quantity of sugar broken up by the ferment," have induced Roberts to use this loss of density as a means of calculating how much sugar any urine contains, "always provided that the remaining ingredients of the urine continue unchanged, or become changed in some uniform ratio." The author, after having convinced himself of the accuracy of the fermentation method, gives the following sketch for conveniently carrying it out as well in private as in hospital practice: 1. The specific gravity is taken at the ordinary temperature of the room. 2. Three or four ounces of the urine are poured into a 12-oz. phial, together with a lump of German yeast of the size of a large filbert. 3. The bottle is lightly corked, or covered with a slip of glass, and set aside in a warm place to ferment. 4. In about eighteen hours, when the fermentation has entirely ceased, the bottle is tightly corked, and removed to a room of ordinary temperature, so that it may cool to the temperature at which the specific gravity was taken the day before. 5. The urine in the meantime clears, and in five or six hours it may be decanted into an appropriate vessel, and the specific gravity taken again. 6. The amount of "density lost" is thus ascertained, and the following simple and most convenient rule expresses the result of the analysis. *Each degree of 'density lost' indicates one grain of sugar per fluid ounce of urine.*

4. Liebermeister's experiments on man prove that the degree of temperature normally existing in the closed axilla is not lowered by abstraction of heat through the skin, provided this be not too intense or too long continued, but on the contrary, often increased. He further shows that abstraction of heat does not only cause loss, but also increased production of heat within the body. Thus in a bath of from 20° to 23° Cent. (68° to 73·4° Fahr.), the production of warmth is from three to four times, and in a bath of 30° Cent. (86° Fahr.), twice as great as the average production under normal circumstances.

V. Nervous System.

1. R. Boyd: *Tables of Weights of the Human Body and Internal Organs in the Sane and Insane of both Sexes at Various Ages.* (Proceedings of the Royal Society, vol. xi. p. 124, 1861.)
2. H. Paris: *A Case of Circus-Movement ("Mouvement de Manège") from Hæmorrhage into the Pons Varolii.* (Brown-Séquard's Journal, vol. iii. p. 717, 1860.)
3. J. Rosenthal: *On the Influence of the Pneumogastric and the Superior Laryngeal Nerves on the Movements of the Diaphragm.* (Compt. Rend., vol. lii. p. 754, 1861.)

1. The following figures from Boyd's paper on the weight of the brain are based on the results of 528 examinations made in cases of insanity, at the Somerset Lunatic Asylum, between 1848 and 1860. The weight of the right cerebral hemisphere varied in males from 20·89 oz. to 18·97 oz.; in females from 19·21 oz. to 17·20 oz.; the left cerebral hemisphere varied in males from 21·05 oz. to 18·62 oz., and from 19·51 oz. to 17·39 oz. in females. The author had found the same predominance of the left hemisphere with regard to weight, in 200 other cases.* The weight of the cerebellum in males varied from 5·42 oz. to 5·06 oz., and in females from 5 oz. to 4·74 oz.; that of the pons Varolii and the medulla in males from 1·15 oz. to 1·02 oz., and from 1 05 oz, to 0·95 oz. in females; that of the entire encephalon in males from 48·7 oz. to 43·87 oz., in females from 44·55 oz. to 40·55 oz.; in the sane at the same period of life the average varied from 48·2 oz. to 45·34 oz. in males, and from 43·7 oz. to 39·77 oz. in females.

2. Paris describes the case of a cat affected with hæmorrhage into the left half of the pons Varolii through accidental violence. This hæmorrhage injured especially "the transverse and superficial fibres" of the left half of the pons, fibres, therefore, which are "in connexion with those of the middle peduncle of the cerebellum" (crus cerebelli ad portum); "it caused also alteration of this peduncle itself." The symptoms exhibited by the animal were at first general convulsions, which ceased after a few minutes, being followed by circular movements "from the right side to the left;" the circles described "were regular and equal, having about one foot in diameter." The observation is of importance, as the remainder of the encephalon was normal, and the lesion itself limited. It is not in accordance with Longet's experience, who describes the circular movements as occurring in the direction from the side of the injury towards the other, which would be in the present case from the left to the right.

3. Rosenthal communicates to the Academy of France that he has found that irritation of the central portion of the dissected superior laryngeal nerve influences the action of the diaphragm. The manner, however, in which irritation of the superior laryngeal nerve acts is different from that manifested by irritation of the central end of the pneumogastric nerve below the giving off of the superior laryngeal; while moderate irritation of the pneumogastric nerve arrests the diaphragm in the state of contraction, that of the superior laryngeal arrests it in the state of relaxation; while very weak induced currents applied to the pneumogastric cause a remarkable acceleration of the movements of the diaphragm, the same applied to the superior laryngeal nerve cause retardation. Rosenthal regards the superior laryngeal nerve as an "inhibitory nerve."

* Conf. Medico-Chirurgical Transactions, vol. xxxix.

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.
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I. *On the employment of Powdered Coal-tar, and Saponized Coal-tar in the Treatment of certain forms of Skin Disease.* By M. Devergie, of the Hôpital St. Louis. (Bulletin Général de Thérapeutique, April 15th, 1861.)

Powdered coal-tar was formerly much employed as a disinfectant, but M. Devergie has found it useful in the treatment of some skin diseases. The first case of the latter kind which came under his notice was that of a person suffering from eczema impetiginodes, covering the whole surface of the body, and pouring out an abundant, rather sanious and fetid secretion, followed by thick crusts of a yellowish-grey colour, resembling impetigo scabida or rupia. With the view of destroying the fetid odour which exhaled from the whole of the body, M. Devergie ordered the affected surfaces to be dusted lightly, night and morning, with powdered coal-tar. In two days the smell had entirely disappeared, but the discharge had formed, by combining with the coal-tar, a kind of horny envelope, constituting prominences on various parts of the body, and partly detached, so as to leave spaces where the skin was not covered. But on examining these spaces, it was found that the skin was improved in appearance, and no longer secreted any discharge. Three effects had therefore been produced—namely, disinfection, the diminution of the morbid secretion, and the solidification of the crusts. The latter soon became so hard that they formed foreign bodies of a very painful kind to the patient, who could not turn in bed without pressing upon these firm and almost stony substances, which irritated the skin. This inconvenience was relieved by the application of hog's-lard under poultices for several hours daily, and the repeated removal, by cutting, of fragments of the hardened secretion, so that the skin was at length rendered clean. After the successful treatment of this case, M. Devergie employed the powdered coal-tar on a large scale, in rupia, ecthyma cachecticum, zona, pemphigus, ecthyma, herpes, scabies purulenta, impetigo, and eczema. In the generality of cases there was a marked improvement—namely, that in a rather short period the secretion diminished and the inflammation subsided, the skin began to resume its normal condition, at the same time that the painful or disagreeable sensations of the patient were relieved. M. Devergie, in order to ascertain whether the improvement in the successful cases was due to the plaster formed upon the surface, employed common modelling plaster in the same manner as the coal-tar, but he found it very far inferior in its effects. The fact seems to be that tar, which is a vegetable production, is an efficacious medicine in the treatment of cutaneous diseases. In cases of acne, the coal-tar is also beneficial, but its use is attended with certain inconveniences, for it has a greyish colour, and gives a disagreeable appearance to the face; M. Devergie is, therefore, now occupied in determining the question whether creosote, as representing vegetable tar, or phenic acid, as representing mineral tar, might not be substituted for powdered coal-tar.

Powdered coal-tar should never be employed except in very small proportions; the diseased surfaces should be merely dusted with it so that the powder is scarcely visible, and not laid on thick as is too often done. As to saponized coal-tar it has not yielded well-marked satisfactory results, for the enormous proportion of alcohol which it contains irritates the diseased surfaces in a very short time.

II. *On the use of Euphorbia Prostrata as an Antidote to the Poison of the Rattlesnake.* By Dr. B. J. D. Irwin. (American Journal of the Medical Sciences, January, 1861.)

Dr. Irwin's attention was drawn to the circumstance that very few of the native inhabitants of Mexico lost their lives from the bite of the rattlesnake, although this reptile is very abundant in the south and south-western regions of the United States. After making numerous inquiries, he found that although injuries from poisonous reptiles were very common among the natives, they had an efficacious antidote in a substance called *gollindrinera*, a plant of a delicate appearance, found growing abundantly throughout the southern portion of Texas, New Mexico, Arizona, and Sonora. This plant is the euphorbia prostrata, growing plentifully in dry, hard, sandy places, having long filiform reddish stems, and abounding, like the other *euphorbiaceæ*, in a milky juice, which is said, however, not to be of an acrid character. Dr. Irwin found that the medicinal properties existed in the milky juice of the stem, root, and leaves; but the preparation usually employed was the fresh juice extracted from the plant by bruising it in an iron mortar and diluting it with a considerable quantity of water. The physiological effects of this juice are emetic and cathartic when given in large quantity. Dr. Irwin's experiments were made upon dogs, which were purposely bitten by the rattlesnake, and afterwards had the juice administered to them. The results were most striking and satisfactory, and the animals appeared to be rescued from impending death by the employment of the juice. The effects were the same in all the cases except one, in which the antidote appears to have been given too late. Dr. Irwin has heard of many cases of rattlesnake bites cured by the Mexicans by the use of this plant, the administration of which is unattended with any danger to the animal economy. It appears also that other varieties of the *euphorbiaceæ* enjoy a reputation as remedies against the poison of noxious reptiles, especially the Euphorbia capitata, E. corollata, E. palustris, and E. villosa.

III. *On the Comparative Merits of Ether and Chloroform as Anæsthetics.* By Frederick D. Lente, M.D., of New York. (American Journal of Medical Sciences, April, 1861.)

Dr. Lente, in common with other American writers, prefers ether to chloroform as an anæsthetic, and justifies his preference by the greater safety which he alleges to attend the employment of the former agent. He analyses several of the deaths said to have been caused by ether, but in which the fatal result seems attributable to other causes. In one case, for instance, death did not occur until the third day after the operation; in another, until forty hours after the operation; in a third case, the operation was a very severe and dangerous one, death happening three hours afterwards; and in a fourth, the patient, a feeble old man, who had had a large cancerous tumour removed from his breast, died after the lapse of seven hours. In experiments upon the lower animals, the employment of ether appears to be much less fatal than that of chloroform, although insensibility may be equally produced. It has been proposed to combine chloroform and ether as anæsthetics with a view to obtain the beneficial effects of both, without the danger attending the use of chloroform alone; but Dr. Lente thinks that this mixture is more dangerous than either of the anæsthetics separately. Dr. Lente admits that chloroform is generally a very safe anæsthetic, considering the great number of cases in which it is employed; but he thinks that if it is possible to diminish still further the small number of deaths which have followed its use, it is the duty of medical men to do so. He contends that ether is more safe than chloroform as an anæsthetic; and he states, that at the New York Hospital the use of ether has been resumed in consequence of the dangerous results occasionally arising from the use of chloroform, which had been temporarily substituted in its place.

IV. *On the Operation of Aniline upon the Animal Body.* By Dr. SCHUCHARDT. (Archiv für Pathologische Anatomie und Physiologie und für Klinische Medicin, 1861.)

Dr. Schuchardt made a series of experiments to prove the effects of aniline upon animals. Aniline is an oily liquid, of powerfully basic properties, obtained by the distillation of indigo, and the name aniline is derived from the specific name of the indigo-plant (*Indigofera anil*). The physiological properties of aniline are but little known, but the researches of Dr. Schuchardt prove that it is a poisonous substance. The results at which he arrived are the following:—Aniline may act injuriously on the animal organism, and in large doses may even cause death. Frogs introduced into a weak solution containing aniline died in periods varying from a quarter of an hour to two hours and a half, and death was also caused by the introduction of aniline into the mouth or into a wound in the back. Rabbits were also poisoned by this substance, a small animal being killed by fifty drops in six hours and a quarter, and a larger one by one hundred drops in four hours. In all the animals experimented upon, violent clonic and tonic spasms ensued after the application of the aniline, and continued almost uninterruptedly until death. There was also loss of sensibility, commencing at the lower extremities and extending to the upper, and the temperature of the body was also reduced. Wherever the aniline was applied locally, as in a wound of the back, on the stomach, on the posterior part of the tongue, or on the conjunctiva, appearances of irritation were observed, which are probably connected with the power possessed by aniline of coagulating albumen. The aniline was never detected in the urine, and it is probable that this substance is eliminated from the body rather by the organs of respiration than by the kidneys.

V. *On the Physiological Action of Santonine and of the Essential Oil of the Seeds of Artemisia Contra.* By Dr. EDMUND ROSE, of Berlin. (Archiv für Pathologische Anatomie und Physiologie und für Klinische Medicin. 1860.)

Santonine is an organic principle obtained from the *Artemisia contra,* and possesses vermifuge properties, but its operation has only recently been minutely studied. Dr. Rose has taken it himself, in the dose of a gramme (about fifteen grains), without injury. Its taste is bitter, very disagreeable, and persistent, and it is slightly diuretic, but rather confines the bowels. It produces a peculiar and indescribable narcotic effect, and causes a species of colour-blindness, tinging all the objects seen with a yellow hue. The essential oil of *Artemisia contra* is poisonous; it kills rabbits in the dose of about two grammes, producing convulsions which descend from the head to the lower extremities, followed by ascending paralysis. This oil is not vermifuge, for in medicine and medicinal doses it is absorbed in the stomach and the upper part of the small intestines, and does not reach the lumbrici, which generally exist lower down; in larger doses it is poisonous. Santonine, on the other hand, is a good vermifuge; it is innocuous, and traverses the whole of the digestive canal, and is found in large quantity in the fæcal matters, for it is almost insoluble in water, and a small part only is transformed into soluble santonate of soda, after contact with the bile, and it is santonine which most rapidly destroys the worms. Santonate of soda ought not to be substituted for santonine, as has sometimes been recommended, for it possesses no superiority over the latter, and is besides of a more disagreeable taste, and is poisonous in large doses.

VI. *On the Treatment of Prolapsus Ani by Subcutaneous Injections of Sulphate of Strychnia.* By M. DOLBEAU. (Bulletin Général de Thérapeutique. 1861.)

M. Dolbeau met with two cases in children, where the prolapsus of the rectum was cured by the subcutaneous injection of sulphate of strychnia. The first case was that of a little girl, aged three years and a half, who had suffered from prolapsus for two years. M. Dolbeau injected with a syringe ten drops of a solution of sulphate of strychnia (thirty centigrammes of the sulphate to thirty grammes of water) at a short distance outside the anal orifice. This operation did not cause much pain, and the child immediately afterwards was as lively as usual. The prolapsus returned, however, and another injection was practised, after which there was no return of the malady. The other case was that of a boy five years old, who had prolapsus, for which cold baths, cold injections and astringents, and, lastly, electricity had been prescribed without success. It was therefore determined to try the injection of sulphate of strychnia. A small puncture was made quite close to the anus, and a canula was inserted a little way into the puncture; eleven drops of the solution were injected, and the puncture was compressed for a few seconds to prevent the escape of the liquid. The child did not suffer any pain, and there was no return of the prolapsus.

VII. *On the Employment of Iodine Inhalations in Pulmonary Phthisis.* By Dr. SIMON. (L'Union Médicale, March 16th, 1861.)

The treatment of pulmonary consumption by iodine is very frequent in Belgium, and has been especially recommended by M. Chartroule. Under his directions twenty-eight patients in the hospital were treated by the inhalation of the vapour of pure iodine, and of this number only eleven could be said to derive no benefit from the treatment. In these unsuccessful cases, the pulmonary lesions were not modified, but still the symptoms were not aggravated in any case. In opposition to the statement that iodine vapour produced hæmoptysis, it was found that pulmonary hæmorrhage ceased more rapidly under this kind of treatment than under other plans which are more generally employed. Seventeen patients derived positive benefit from the iodine treatment, and this improvement was observed not only in relation to the general symptoms, but also to the pulmonary lesion itself, as was proved by percussion and auscultation. Out of the seventeen patients, four might be considered as actually cured. One of the cases of cure is the following:—A youth, sixteen years of age, entered the hospital in such an alarming condition that at first the physicians hesitated to submit him to the iodine inhalations. He was in a state of great emaciation, and his skin was almost constantly covered with profuse perspiration; he had diarrhœa which had lasted for two months, and he had repeatedly suffered from hæmoptysis. There were very extensive indurations in the lungs, and at the apex of the right lung there was a cavity of some size, as was shown by very obvious gargouillement. The expectoration was also characteristic. After resting a few days, this young man was subjected to the iodine inhalations, and all the symptoms which had appeared so serious were soon modified in a most remarkable manner. The general symptoms disappeared first, and the body recovered its plumpness with great rapidity. The perspiration, diarrhœa, fever, cough, and expectoration were soon relieved or removed, and six weeks after admission into the hospital the patient went out quite well.

Several other cases of the same nature are recorded from both the public and private practice of M. Chartroule, and in all of them the beneficial effects of the iodine inhalations are remarkably exhibited. Dr. Simon, who relates the cases, attributes a great part of the efficacy of the treatment to the apparatus employed for inhalation, which, however, is not described in the paper. By this apparatus, it appears, a degree of precision is given to the treatment which consequently becomes the more efficacious, for the dose of the vapour may be estimated with exactness, and the remedy may be made proportionate to the severity of the disease and the strength of the patient.

VIII. *On the Employment of Dover's Powder in the Perspirations of Phthisis.* (Gazette Médicale de Lyon, January, 1861.)

This medicine has been recommended in phthisical sweating by M. Descamps, who has found it uniformly successful. It might be doubted, on theoretical grounds, whether the Dover's powder, being itself a sudorific, would be likely to check undue perspiration; but according to M. Descamps, the effect has even surpassed expectation, the sweating being suppressed from the first, and the success of this mode of treatment is proved by the results of experience during a period of eighteen years. The mode of administration recommended by M. Descamps is the following:—"We possess," says he, "several records of phthisical cases in which the perspiration was arrested up to the period of death. The powder was generally given in the dose of fifty centigrammes (a centigramme is ·1543 of a grain) in the evening at different hours, according to that which announced the commencement of the sweating; and not only was it always observed that it prevented this symptom, but it also diminished diarrhœa, allayed cough, and predisposed to sleep. It sometimes happened that the powder was vomited; in such cases the dose was divided into two parts, one of which was given in the evening, and the other during the night, when the patient was awoke."

IX. *On the Employment of Opium in Large Doses, and Inhalations of Chloroform, in Idiopathic Tetanus.* (Bulletin Général de Thérapeutique, January 15th, 1861.)

According to the results of experience, opium is still the most efficacious remedy in the treatment of idiopathic tetanus, and its use may be continued with safety for a long period. Two cases which occurred in the practice of M. Grisolle appear to leave no doubt upon this point; and one of the cases presents this peculiarity, that it shows how much superior opium is to inhalations of chloroform, to which latter great efficacy was at first attributed. The fact is, that although the inhalations of chloroform suspend for a short period the symptoms of tetanus, they do not remove them altogether, and with the elimination of the chloroform the disease resumes its primitive character and pursues its course. The two cases recorded were those of a man and a woman, both of whom, without any appreciable cause, were attacked with tetanus. In the first, which was that of the man, inhalations of chloroform were repeatedly employed, with temporary relief; but as soon as the effects of the anæsthetic passed away, the tetanic convulsions reappeared. At last, opium was given in the dose of five centigrammes every hour (a centigramme is ·1543 of a grain); and after some days of this treatment, during which a large quantity of opium was taken by the patient, the tetanic symptoms disappeared, and the patient completely recovered. In the other case, that of the woman, the opiate treatment was pursued from the first, the bowels, however, being relieved by castor oil and purgative injections, and after persevering in this treatment for some days this patient was also cured.

X. *Case of Purpura Hæmorrhagica rapidly cured by the Perchloride of Iron.* (Journal de Médecine de Bruxelles, January, 1861.)

M. Van Holsbeck has communicated the following case to the Société de Médecine of Brussels. A girl, aged eighteen, was suddenly seized with uneasiness, pain, nausea, and shivering. The next day she perceived that her saliva was bloody, and that her skin was covered with blue and red spots. M. Van Holsbeck saw her the same day, and found the chin, neck, trunk, and limbs covered with small circular spots of various size and colour; and there were large patches of ecchymosis, of a violet and greenish tint, on the breasts, elbows, loins, and calves of the legs. Here and there were also spots on the lips, the edges of the tongue, the gums, the buccal mucous membrane, and the velum palati. The patient expectorated continually frothy mucus reddened with blood, and the gums bled slightly. A mixture was immediately prescribed containing one gramme of perchloride of iron, and it was continued on the following days. In about a week the patient began to feel better, the spots were blacker, and some new ones had made their appearance, but the expectoration was no longer bloody. In a day or two afterwards she felt quite well, no more spots were observed, and the ecchymoses had become much less extensive; there was no more bloody expectoration, and the gums were altogether restored to their natural condition. In a fortnight after the admission of this patient into the hospital she was completely cured, and the skin no longer presented the least spot or ecchymosis.

XI. *On the Remedies for Tape-worm in Abyssinia.* By Dr. COURBON. (Bulletin Général de Thérapeutique, April 30th, and May 15th, 1861.)

The remedies for tape-worm stand at the head of the Abyssinian Materia Medica, and they are perhaps the only remedies that the Abyssinians are acquainted with, and that are really useful to them. Among these remedies, the kousso and mesenna must be ranked first, and afterwards the fresh bark of the pomegranate, the habi-tsalim, habi-tchogo, belbelta, and soaria. The *kousso* is furnished by a rosaceous plant of the tribe of the spiræas, and approximating to the genus agrimonia, and is called the *Brayera anthelmintica.* This is a beautiful diœcious tree, terminated above by a bunch of leaves, and with long, pendant bunches of flowers. The latter are the parts employed, and the dose is a handful. The Abyssinians reduce the kousso into a coarse powder, and swallow it mixed with water. An hour after taking it, there is an ordinary evacuation, and half an hour or an hour later there is a liquid motion, and at the end of three, or sometimes four or six hours, the tænia is expelled in the form of a whitish ball. The Abyssinians swallow the kousso fasting, and take no food until after the expulsion of the tænia, but afterwards they drink and eat abundantly, and use the most exciting kinds of food and drink. The *mesenna,* another remedy for tape-worm, was once supposed to be obtained from the *Juniperus procera,* but it is really the product of a leguminous plant, the *Albizzia anthelmintica,* a tree of some three or four feet high, with a thick and very rugous bark. The latter is the part used in medicine. The Abyssinians take it in various ways, but they always employ the powdered bark in the dose of about sixty grammes. They mix it with water, or they make it into bread with flour, or they incorporate it with butter or honey, so as to form a kind of bolus, which they swallow. The consequence is, that on the evening when the drug is taken there is a semi-solid motion, in which there are some fragments of the worm. It is only on the next day and the following days that the rest of the tænia is expelled in sero-mucous evacuations. The mesenna is said to be the best remedy for tape-worm, and completely expels this parasite from the body. The *habi-tsalim* is obtained from two kinds of jasmine—the *Jasminum Abyssinicum* and the *J. floribundum*—and the leaves are the parts employed, mixed with the young shoots of the *Olea chrysophylla*, a kind of olive. A handful of this mixture is pounded very carefully between two stones, with the addition of a little water, and thus a kind of liquid paste is obtained and swallowed by the natives. It is said to be very efficacious in expelling the tænia. The *habi-tchogo* is now proved to be the *Oxalis anthelmintica*, a handsome plant with a subterranean stem, terminated by an oval bulb of the size of a chesnut. The bulbs are the parts employed, in the dose of sixty grammes or more; the Abyssinians eat them by handfuls like small onions, or bruising them on a stone, they mix them with fluid, and drink the juice after it has been strained through linen. The *habi-tchogo* is said to be almost as efficacious as the kousso. The *belbelta* belongs to the family of *amarantaceæ*, and is said to be obtained from the *Celosia adoensis*. According to Schimper, the leaves, flowers, and fruits are employed for the tænia; but MM. Ferrel and Galinier state that it is the powdered seeds which are used. The *soaria* is a small shrub belonging to the family of the *Myrtaceæ*, and is the *Mœsa picta*. The part employed is the fresh fruit, or the same part dried. The *roman* is the native name of the *Punica granatum*, or pomegranate. In Abyssinia, as in Europe, the bark of the root is the part employed, but it is rarely used. Among all these remedies for tape-worm, the kousso, the mesenna, and the habi-tchogo are almost the only anthelmintics employed by the Abyssinians.

XII. *On the Internal Employment of Chloroform.* By M. Bonnet and Dr. Debout. (Bulletin Général de Thérapeutique, April 15th, 1861.)

In consequence of the difference in density between chloroform and water, it is rather difficult to administer the former drug as an internal remedy, because it falls to the bottom of the bottle and can only be suspended by continual shaking. M. Bonnet has therefore proposed to employ glycerine as a medium for the administration of chloroform. Equal parts of chloroform and glycerine are poured into a mortar, and the mixture is rubbed up gently until no more drops of chloroform can be perceived; then the distilled water or other adjuvant is added, and a very limpid mixture is obtained, not exhibiting the slightest trace of chloroform in the free state. The glycerine should be quite pure—that is to say, it ought to be carefully freed from organic matters, fatty volatile acids, a little sulphuric acid, and certain salts, with which it is often adulterated in commerce. Dr. Debout approves the suggestion of M. Bonnet as to the mixture of chloroform with glycerine, but he recommends that the proportions of the two should be different. Dr. Debout's formula consists of thirty grammes of glycerine and two grammes of chloroform, a teaspoonful for a dose in a glass of water. The dose of chloroform thus taken amounts to twelve drops.

XIII. *On the Use of Perchloride of Iron in some Cutaneous Affections.* (Gazette des Hôpitaux, Jan., 1861.)

M. Devergie, of the Hôpital St.-Louis, has observed some very satisfactory results from the use of perchloride of iron in the treatment of certain cutaneous diseases, and other practitioners have confirmed the merits of this kind of treatment. The two following cases are recorded by M. Bouron des Clayes, of

Créteil. The first was that of a man, sixty-five years old, who was suffering from eczema, accompanied with insupportable itching, extending from the scrotum to the margin of the anus. Poultices of potato-starch, ointments made with tar, and oil of cade were employed without success, and at length M. Bouron des Clayes lightly touched all the affected surfaces with a camel's hair pencil dipped in liquid perchloride of iron, over which he laid a thick coating of collodion. On the next day, the skin was a little brown, and completely dry over the whole extent of the eczema, and a fresh application was made of the perchloride and of collodion. The day afterwards, nothing remained except the colour of the skin to indicate that the patient had been affected with any eruption. The second case was that of a strong man, forty-seven years old, who was suffering from itch, but who had also had for a long time *lichen agrius* of the forearm. M. Bouron des Clayes applied an ointment composed of perchloride of iron, bicarbonate of soda, and hog's lard, and at the end of a few days the itch had completely disappeared, the lichen was altogether improved, and he was rapidly cured.

XIV. *On the Use of Digitalis in Large Doses in the Treatment of Menorrhagia.* (Gazette des Hôpitaux, 1861.)

A woman who had suffered from menorrhagia for six weeks, entered the Hôtel Dieu as a patient. She was immediately ordered to take ergot of rye, but without any beneficial effect. M. Trousseau then recommended a very strong infusion of digitalis (eight grammes of the leaves to two hundred grammes of water.) This medicine, according to M. Trousseau, ought to be administered in a certain manner: the strong infusion should be given in the dose of a tablespoonful every half-hour. When digitalis is taken in a large dose, warning is given of the danger which it may produce, by a series of symptoms arising from the irritating action of the digitalis on the gastro-intestinal mucous membrane—namely, vomiting, diarrhœa, and pains in the stomach and bowels. As soon as the medicine is absorbed, a second series of poisonous symptoms is observed, as vertigo, disturbance of vision, pain in the head, delirium, stupor, &c. It is necessary to discontinue the medicine as soon as the first set of symptoms is induced. In the case of the patient above alluded to, the digitalis was given for five hours. As soon as the first symptoms of vomiting were observed, the hæmorrhage from the uterus was arrested, and did not return. Although the digitalis in this case did not produce any very serious symptoms of poisoning, yet the pupil remained for a long time enormously dilated.

XV. *On the Employment of Digitalis and its different Preparations in the Treatment of Organic Disease of the Heart.* By Dr. E. Richard Pfaff. (Bulletin Général de Thérapeutique, Feb. 15th, 1861.)

Dr. Pfaff considers that digitalis certainly belongs to the class of narcotico-acrids, but as it contains two active principles—namely, *digitaline* and *skaptine*, it exercises two different kinds of action. Digitaline exercises its influence on the nervous system of the heart, the morbid activity of which it usually reduces. According to Dr. Pfaff, it acts by paralysing the nerves of the circulatory apparatus; when, for instance, the pulse, after being morbidly excited, falls, under the use of digitalis, from 100 in the minute to 30 or 40, it is because the abnormal activity of the heart and arteries has passed from one extreme to the other, and that a condition of morbid excitement has been succeeded by a kind of paralysis. It must be stated, however, that the depressing action of digitalis on the pulse is by no means constant, for certain delicate and sensitive constitutions exhibit a peculiar excitement of the circulation under the use of the drug. The action attributed to the *skaptine* is a stimulating one upon the venous and lymphatic absorbents, and upon the glandular system in general. It is thus that the diuretic action of the medicine may be explained, and although some authors have considered this effect to be peculiar to digitalis, it is only manifested in proportion as the absorbent function has been augmented by its administration.

With regard to the precise action of digitalis on the heart and arteries, M. Pfaff is convinced, after numerous experiments, that rather large doses of digitalis produce more or less increase in the activity of the heart, and that although after the employment of small doses the depressing action is manifested at the end of twenty-four or forty-eight hours, or after some days, yet that the phenomena of depression are not to be considered as primitive. As to the duration of this first period, or period of acceleration, it depends entirely upon the dose, large doses usually producing a more important acceleration, but one which does not last long, and is followed by a more prolonged diminution in the cardiac pulsations. In relation to the remarkable action of digitalis on the venous and lymphatic systems, M. Pfaff remarks that just as digitalis excites and then depresses the action of the heart and arteries, so does it exercise the most opposite influence upon the venous and lymphatic systems, depressing them first and exciting them afterwards. In short, remarks M. Pfaff, digitalis acts, first, by accelerating, and secondly, by retarding the action of the heart; it acts in the same manner in relation to the stomach, but in a degree somewhat less marked than tartar emetic; it diminishes diuresis at first and increases it afterwards; and lastly, it acts upon the sexual organs in the same manner as upon the urinary organs, at first depressing and then exciting them.

In the inflammatory diseases of the heart, as pericarditis, myocarditis, and endocarditis, M. Pfaff does not much approve the use of digitalis. According to his views, it should only be employed in cases where there is no congestion associated with the inflammation, or where the latter has been already reduced by suitable treatment, and then it should be given only in very small doses, as one-quarter to one-third of a grain of the powder four times a-day. In hypertrophy of the heart, M. Pfaff insists that the same prudence must be observed, because disturbance of the digestive function is easily induced in such cases. It is therefore in the organic affections of the heart properly so called—namely, those of the valves and orifices of the heart, that M. Pfaff thinks digitalis peculiarly useful; in those cases the physician can scarcely hope for a cure of the morbid changes, and he is compelled to prescribe for symptoms only. But even in such cases great precautions must be taken, for in insufficiency of the mitral valve, for example, there is often catarrh of the digestive canal or the genito-urinary mucous membrane, and if digitalis were employed in such a case, the cardiac symptoms might be relieved while the secondary phenomena would be aggravated.

As to the preparations of digitalis, M. Pfaff considers that the *powder* is an excellent form for administration, but he prefers the *infusion*, prepared with from 1 to 4 grammes of the leaves to 125 to 200 grammes of fluid, in the dose of four tablespoonfuls a-day. The *decoction* often produces colic in a marked degree, but it possesses more diuretic properties than the other preparations. The *alcoholic tincture* has the same action as the infusion, but in a smaller dose it produces cerebral congestion and vertigo. The *ethereal tincture* has the same effect, but the symptoms disappear on the production of symptoms denoting activity of the heart and arteries. M. Pfaff also considers that digitalis may be employed externally in cases where it cannot be administered internally. He recommends a mixture of equal parts of chloroform and tincture of digitalis to be applied externally, or the powder of digitalis to be applied night and morning on a blistered surface.

The general conclusions at which M. Pfaff arrives are the following: 1. Digitalis ought not to be administered in an increasing dose, but in a diminishing one. 2. The dose of the medicine must be reduced as soon as the paralysing action is perceived on the heart and arteries. 3. The sedative action of digitalis, in morbid excitement of the heart, is often prolonged for five or eight weeks. 4. Digitalis should never be continued in any form for more than six or eight days, and if after a week's trial the desired results have not ensued, squills or colchicum must be employed; these latter, as is well known, also act as sedatives to the heart. 5. In torpid subjects, it is convenient to commence the treatment by squills and colchicum, before administering digitalis. 6. In the greater number of cases it is advantageous, in order to avoid the inconvenient effects of digitalis on the digestive organs, to associate it with aromatics or tonics. 7. In aged persons, it is still better to associate it with cinchona. 8. In tuberculous subjects, digitalis should be associated with opium; in dropsical cases, with liquor potassæ and acetate of ammonia, polygala, squills, juniper, &c.; in plethoric persons, with cream of tartar, magnesia, sulphate of potash, and nitre; and in anæmic cases, with ferruginous preparations. 9. By following the employment of digitalis by the administration of arsenic, the cyanotic effects of cardiac diseases may often be considerably alleviated.

XVI. *On the Treatment of Pneumonia by the Acetate of Lead.* (Gazette Médicale de Strasbourg, 1860.)

M. Strohl has lately prescribed acetate of lead in acute pneumonia, to the exclusion of all other remedies, and has obtained such satisfactory results as to encourage him to persevere in the same plan. When, however, there is great congestion, he orders some blood to be drawn in the first place, either from the arm or by means of cupping or leeches. He gives the acetate of lead in rather large doses—namely, from 25 to 50 centigrammes, and the pulse usually falls ten or fifteen beats, and sometimes it falls even below the normal standard. The local symptoms continue at first to increase, the bronchial breathing and the crepitant râles appear to be augmented, but the patient soon experiences a sensation of comfort, and resolution begins to take place. From this period of the amelioration of the local symptoms, M. Strohl discontinues the administration of the acetate, and resolution is completed without any further treatment. Convalescence is observed at the end of five, six, eight, or even twelve days of treatment. M. Strohl expresses himself in the following terms on the subject of this kind of treatment in pneumonia. "It is at least as rapid in its beneficial results as the treatment by large bleedings, antimonials, digitalis, or veratrine. It saves the patient's strength, and it can always be employed, even in spite of the existence of some other morbid condition. Convalescence soon commences, and I have never known any bad symptoms to follow the administration of the salt. I do not assert that this remedy has never failed in my hands, but such cases of failure have been more rare than under the usual methods of treatment."

XVII. *On the Preparation of a Stearate of Iron in the Treatment of Phagedenic Chancres.* (Bulletin Général de Thérapeutique, May 30th, 1860.)

For some months M. Ricord has employed an ointment and an adhesive plaster of stearate of iron, which appears to be a valuable agent for dressing chancres which are complicated with phagedena. This new therapeutical agent was employed for the first time on a patient who had been the subject of experiments in syphilization, and who had, when he came under M. Ricord's care, his thighs covered with phagedenic ulcerations, which had been treated without success by several able practitioners. M. Ricord conceived the idea of making use of an adhesive plaster of stearate of iron, with which he dressed the ulcerations on the right side, and dressings, by way of comparison, were applied on the left side with adhesive plasters of coal-tar. In a short time the phagedenic ulcerations on the right side were completely cicatrized, and in consequence of this remarkable result, the coal-tar plaster was replaced by one of stearate of iron, which effected a perfect cure in less than a month. The ointment of stearate of iron is made by mixing together a solution of sulphate of iron with a solution of Marseilles soap, drying the precipitate and then melting it, and finally adding some essence of lavender, taking care to stir until the mass has completely cooled. The adhesive plaster of stearate of iron is made by taking the stearate prepared as above described, melting it at a gentle heat, and spreading it upon linen as in ordinary plasters.

XVIII. *On the Therapeutical Employment of the Simple Sulphate of Alumina and of the Sulphate of Alumina and Zinc.* (Bulletin Général de Thérapeutique, March 30th, 1861.)

The sulphate of alumina (which is not to be confounded with the common alum, a double sulphate of alumina and potash) was introduced into medical practice by Dr. Homolle. It contains an excess of acid, a small portion of iron, and a little of the double sulphate; and in order to neutralize the free acid, which may burn the linen of the patients and injure the steel instruments of the surgeon, M. Homolle has proposed to add oxide of zinc, thus forming a double sulphate of alumina and zinc. This latter has a more energetic action on heteromorphous tissues than the simple sulphate, and M. Homolle prefers it when it is desirable to apply it to a deep portion of altered mucous tissue, or to destroy an accidental growth. The affections in which it has been successfully employed are inflammations of the tonsils and pharynx, hypertrophy of the tonsils, polypus of the nasal fossæ, ingrown finger-nail, scrofulous ulcers, nævi and vascular growths, inflammatory affections of the neck of the uterus, displacements of that organ, and lastly, cancerous ulcers. In many cases where the tonsils were so much hypertrophied as apparently to require excision, M. Homolle has observed the chronic enlargement to yield to the daily use of the saturated solution of the simple sulphate of alumina. Membranous diphtheric sore-throat, before the larynx was attacked, yielded to the same application, and in a case of polypus of the nasal fossæ, where the morbid growth had sprouted out several times after removal, the patient was finally cured by the same agent. Scrofulous ulcers, touched every day with the sulphate of alumina, began to assume a healthy appearance, their fungous growths being reduced and the cicatrization being promoted; and hypertrophic engorgement of the neck of the uterus, and erythematous, granular, or ulcerous inflammation of its lips, was rapidly improved or cured by the direct application of the double sulphate with the addition of injections of the same salt. In its direct action it favours the exfoliation of the morbid tissue in successive layers; it diminishes the ichorous discharge of the ulcerated surface, destroys the smell, and causes the secretion to resemble healthy pus, and diminishes or completely removes the lancinating pains which are peculiar to cancer. M. Homolle's conclusions, in reference to the use of the sulphate of alumina and the double sulphate of alumina and zinc, are the following:

1. These salts, used externally, may be ranked as modifying agents, occupying a place between catheretics and caustics. 2. They are particularly useful in inflammations of the tonsils and pharynx, and in the affections of the neck of the uterus; and 3. They possess, moreover, a special remedial power over cancerous ulcers, the progress of which they retard or modify, acting in this respect at once as caustics, disinfectants, and hæmostatics. They often succeed better than narcotics in relieving the pain peculiar to cancer, and they retard the development of the attendant cachexia.

HALF-YEARLY REPORT ON PATHOLOGY AND MEDICINE.

By JOHN W. OGLE, M.A., M.D. Oxon, F.R.C.P.
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I. (1.) *On Sugar in the Urine.* By Professor Dr. J. SEEGEN, Carlsbad. (Virchow's Archiv, Band. xxi. (Zweite folge, Band i. Heft 2, p. 218.)

(2.) *On Sugar in the Urine.* By H. BENCE JONES, M.D., F.R.S. (Reprint from the Quarterly Journal of the Chemical Society.)

(3.) *On the Detection of Sugar in the Urine.* By Mr. ATTFIELD. (London Pharmaceutical Journal, Jan. 1, 1861.)

(4.) *New Test for Diabetes.* By Dr. E. C. BEDWELL. (Boston Medical and Surgical Journal, Nov. 22, 1860; and New Orleans Medical News and Hospital Gazette, Jan. 1861.)

(5.) *On the Phenomena of Diabetes Mellitus.* By the Rev. S. HAUGHTON, F.R.S., Fell. of Trin. Coll., Dublin. (Dublin Quarterly Journal of Medical Science, May, 1861.)

(6.) *On the Estimation of Sugar in Diabetic Urine by the Loss of Density after Fermentation.* By W. ROBERTS, M.D., Physician to the Manchester Infirmary. (Reprint from Vol. i. of Series 3 of Memoirs of the Literary and Philosophical Society of Manchester, 1859–60.)

(1.) A COMMUNICATION by Dr. Seegen, embracing the particulars of cases of saccharine diabetes carefully watched and reported, cannot fail to be of considerable interest; especially seeing that the physiological relations of starch and sugar as articles of diet are still the subject of disagreement of opinion. The author commences by viewing in juxtaposition the discordant results arrived at by Bernard and Pavy (which are too well known to our readers to demand recapitulation), and he evidently inclines to an opinion antagonistic to the supposition of Pavy, that the change of glycogene or hepatine formed in the liver into sugar is of post-mortem appearance.* He plainly thinks that Pavy's observations regarding the absence of sugar in the hepatic vein involve some mistake, and is at a loss to understand how it is that starch-holding diet in diabetic patients increase so considerably the sugar in the urine, whilst in the healthy man, even by the abundant use of starch food, no marked amount of sugar is excreted by the urine. He asks how it was that of two rabbits of the same size, of which one had fasted three days, whilst the other, being fed, had eaten three-quarters of an ounce of starch or cane-sugar—in the first, the liver was as 1 to 36 compared with the weight of the entire body, and contained $1\frac{2}{3}$rds per cent. of hepatine; in the last rabbit the proportion of the liver to the body was as 1 to 15, and contained 17 per cent of hepatine? Apparently mistrusting the existing experimental investigations, the author looks for some solution of the question from the scientific observations of the practical physician, and considers himself fortunate in having during last summer fallen in with 14 cases of diabetes, which he minutely watched, remarking that this is the first time that in a large number of diabetics† the *quantitative* analysis (as to sugar) of the urine has been studied during the administration of remedies. In each of the 14 cases quoted, from the commencement of treatment the urine passed during twenty-four hours was collected, and a quantitative determination of sugar made. This was done in each case weekly at least. Contracted space prevents our giving the details of the cases. We will at once pass on to the conclusions arrived at, which are as follows:

* See our Report on Physiology, Jan. 1861, p. 231.

† Observations of this nature, though limited, will be found quoted in a thesis by Neukomm, Zurich.—ED.

Firstly. That the elimination of sugar by the urine is an independent disease, or appears as a symptom of another diseased process. Of the 14 quoted cases, 12 were independent instances of melituria; 2 were symptomatic—in the one the sugar being secreted as a result of a tertian fever; in the other, its secretion accompanying incipient disease of the spinal cord.

Secondly. In some of the cases the sugar apparently originated from the starch-containing nourishment, the increased use of flesh food causing a speedy disappearance of the urinary sugar. In others, however, the sugar-formation occurred at the cost of the gelatinous parts of the body, and remained even on a diet exclusively of flesh. According to Franke, the sugar having origin from food disappears in the urine eight hours after meal-time, and the examination of the night urine becomes in his opinion a means of prognosis. If the morning urine is free from sugar, the patient is in the first form (or stage, as Traube calls it) of disease; but if it contains sugar, then he is in the second. The author's observations are in direct opposition to these views of Traube, and in this respect they accord with those of Rozenstein.* Some of the cases prove that the formation of sugar at the expense of the organism may continue for years even without seriously prejudicing the health of the patient, if only abundant nourishment be afforded.

Thirdly. Among the 14 cases, 11 were men and 3 women; the youngest being twenty years and the eldest seventy-three years old, the majority being between forty and sixty years old. No special cause could be assigned for the disease. In *one* case it was hereditary. In *one* case only was the liver sensibly diseased; in 2 others it was enlarged from hyperæmia. Hæmorrhoïdal congestion existed in almost all cases. Allusion is here made to the opinion of Schiff,† that the presence of an increased quantity in the liver can originate diabetes, as he was able in three days by puncturing this organ to excite this disease. Again, by ligaturing the afferent veins of the kidney (which form the renal portal-system) in eight frogs, and thus causing all the blood from the lower limbs to pass through the hepatic portal vein, all of the frogs in two hours after the operation became diabetic. In many of the author's 14 cases the mind was considerably affected by depression, anxiety, &c.; in 3 cases much headache and faintness existed at the onset.

Fourthly. The use of flesh diet is the treatment found to be of greatest importance in diabetes, not only diminishing the amount of sugar in the urine, but also all other morbid symptoms.

Fifthly. Although flesh diet alone can limit the formation of sugar, it can exercise no influence on the disease itself, the cause of sugar-formation. The most reliable treatment for this purpose is pronounced to be the use of Carlsbad waters. In the 14 observed cases, by their use the elimination of sugar was always essentially reduced or entirely prevented, though not in all cases to an equal extent or with the same rapidity. Whether the benefit arising from the use of Carlsbad water is attributable to any diminution of the quantity of the blood in the liver, or whether it acts directly on the hepatic functions or any action antagonistic to the necessary ferment, are questions yet to be solved.

Sixthly. In most of the cases, after that by treatment a remarkable reduction of sugar in the urine was obtained, the behaviour of the urine with the Fehling's solution became altered. In the earlier periods, on the addition of the urine to a hot solution of copper a beautiful reddish-brown precipitate of oxide of copper formed, which quickly stopped, leaving the remaining fluid clear and colourless, so that one could tell to a drop the limits of the reaction. Later on, this was quite otherwise. A dirty yellowish or orange-coloured deposit was formed, which on further heating became neither reddish-brown

* Virchow's Archiv, Band xii.

† Untersuch. über d. Zuckerbildung in der Liber. Würzburg, 1859.

nor left off clearly, so that the limits of the reaction could not be determined, and consequently the quantitative determination was impossible. The author proposes to seek for an explanation of this.

For the papers marked 2, 3, 4, and 5, containing valuable and extended observations by Dr. Bence Jones, Mr. Attfield, Dr. Bedwell, and the Rev. S. Haughton, we have not now sufficient space. For paper marked 6, by Dr. Roberts, see our Physiological Report, p. 242–3. We propose to consider all these at length in a future critical review of the existing state of our knowledge on sugar as a component of the urine in health and disease.

II. *On Scarlatinal Albuminuria* (*Treatment by Quinine*). By Dr. HAMBURGER. (Archives Générales de Médecine, April, 1861, p. 491.)

In his communication the author confines himself almost entirely to the treatment which, in his experience, has proved most successful. Premising that in scarlet fever the albumen can be considered neither as the result of excessive susceptibility of the skin to cold, &c, nor to any alteration in the mass of the blood allowing of transudation from the vessels, nor indeed as a secondary phenomenon whatever, he attributes it solely to the localization of the virus upon the kidneys, the dropsy being a manifestation or expression of the specific affection, analogous to the primary effect of the virus upon the throat or the skin. As respects the treatment, the author inveighs strongly against the use of digitalis, which diminishes the urine and renders it bloody, and increases local effusion; and also against diuretic remedies, even the mildest; and thinks that vinegar, so useful in Bright's disease, as also mineral acids and hot baths, are useless. The remedy which he has found above all others of service is quinine. Under its use the fever diminishes, the urine is rendered more abundant and less bloody, and the appetite and general power increased. The amount of albumen does not, however, diminish. This remedy was used in 47 severe cases; in 44 cases improvement was at once or very speedily manifested; in 3 cases only was there absence of result, good or bad, but these recovered. When employed in chronic cases, improvement follows the first doses almost immediately—most quickly in adults. In acute cases the quinine must not be at once administered. The dose should be, to infants, from 8 to 10 centigrammes a day, and to adults, from 15 to 20. Its bitterness has alone prevented him from trying it as a prophylactic after the early period of scarlet fever. During the use of the remedy the diet ought to be antiphlogistic, and confined to fluids. An interesting case is quoted at length, showing the value of quinine in a subacute case, in a child.

III. *A Case of Chylous Urine.* By Mr. G. DAVIS, Primary Medical School, Madras. (Madras Quarterly Journal of Medical Science, July, 1860. p. 180.)

The case was that of a healthy, temperate man, aged thirty-three, who had had a "milky state of the urine" for three months. The urine was free from either free acid or alkali, and from any urinous odour. The curdy deposit produced by boiling was dissolved on agitation with ether, forming a straw-coloured semi-solid coagulum. The patient stated that he never had any difficulty in passing urine, and that the peculiar state of the urine during the three years had twice left him during an attack of fever. He was the subject of a rheumatic attack, during which he became feverish, and had copious night sweats; and whilst this was the case his urine became brownish-red and ammoniacal in smell, wanting the usual appearance of chylous urine. It, however, became disposed to coagulate spontaneously, and soon after being voided "became an entire mass of jelly." This disposition to clot was quite removed by iodide of potassium given for the rheumatism, and the urine became less chylous. After recovery from rheumatism, the chylous state of the urine of three years' standing was treated exclusively by iodide of potassium, in doses of five grains thrice daily, fatty matters of diet being reduced, and exercise in the open air recommended. After the iodide had been used for twenty-seven days, the chylous state of the urine "quite disappeared," and no return had taken place after an interval of a year and a half. The writer looks upon the disease as one without danger, and not uncommon in his part of the country. In three cases known to him no complaint was made by the patients, who were stout and strong. The author refers to an interesting case of the same kind, published by the late Medical Board in their 'Medical Reports,' brought forward by Dr. Pearse. It was that of a female, who had three times suffered from this condition of urine during lactation. In that case Dr. Nayer proved the absence of casein, pus, or phosphates; and the presence of albumen, and of animal fat, on which the milky appearance depended.

IV. (1.) *On the Double Intermittent Souffle in the Thigh, as a Sign of Aortic Valve Insufficiency.* By Dr. P. Duroziez. (Archives Générales de Médecine, April and May, 1861, pp. 417 and 588.)

(2.) *On Insufficiency of the Aortic Valves in connexion with Sudden Death;* with Notes Historical and Critical, &c. By J. Cockle, M.D., Physician to the London Free Hospital. (Pamphlet, 1861.)

(1.) After quoting observations from several writers referring to the existence and purport of arterial murmurs in vessels distant from the heart, such as Bouillaud, Corrigan, Charcelay,* Guyot,† Skoda, Stokes, Beau, Triboulet,‡ Marey,§ &c., the author passes on to eulogize the crural artery as capable of affording invaluable helps to diagnosis, being, like the radial, so easily compressible, and larger than the carotids. Their close relationship with the veins which are often obliterated in "phlegmasia dolens," may also render them useful in deciding generally whether all the "bruits" termed "continuous" take place in the veins.

Compression of the crural artery naturally gives rise to a shock or trembling sensation (*frémissement*), and a "bruit" represented by the sound "toc," varying in tone according to the state of the blood, size of the artery, condition of the serum, contractile force of the heart, &c.; and if after continued compression has been exercised it is gently diminished, especially in a chlorotic patient, a continuous murmur, as of the wind or of a gnat, will be heard, and occasionally the bruit de diable. The sound above described, which is single, a simple intermittent souffle, may be found in every crural artery; but there is also another crural souffle, termed double intermittent souffle, and it is this to which our author's remarks are directed. This, although pointed out by many, has never, as a help to diagnosis, been estimated at its proper value. This souffle is not that of the arterial diastole which often exists without compression of the artery, and is dependent on the ventricular contraction; but it is one of systole, which often requires to be carefully sought for, and is owing to the "*systole of the crural arteries*," requiring compression of the artery for its evocation. When the vessels are less completely filled than they should be, pulsating feebly, &c., owing to much narrowing of the aortic or bicuspid orifice, this sound is much less audible and less regularly found than when the aortic insufficiency is not complicated by such conditions, and when the vessels

* Thèse de 1836. † Thèse de 1834. ‡ Thèse de 1854.
§ Thèse Inaugurale de 1859.

react forcibly. And this double souffle may be produced by the gentle pressure of the stethoscope exercised on the artery so as to obliterate the artery, and then a period will be found when this souffle may be heard; or it may be produced by placing the instrument lightly over the vessel and pressing with the hand upon the vessel above or below the instrument. If the pressure be made nearer to the heart, the first souffle will be produced; but if towards the extremity of the limb, the second one will be produced; proving that the second sound is occasioned by the backward influence of the vessels on the course of the blood.

The author proceeds to detail several cases which go to support the following statements:—*Firstly*. That in all cases of disease of the heart wherein the double intermittent crural souffle was heard, post-mortem examination showed insufficiency of the aortic valves. *Secondly*. That the double souffle may be found in the crural vessels, and may be doubtful or absent at the level of the aortic orifice, owing to the proper relation between the contractile force of the artery and the aortic orifice being disturbed. *Thirdly*. That when the double intermittent crural souffle has not been audible, aortic insufficiency has, at the post-mortem examination, been found wanting. *Fourthly*. That when the double intermittent crural souffle is doubtful, the aortic insufficiency is doubtful also.

Thus it appears that this kind of souffle affords precise assistance in diagnosing lesions of the aortic from those of the bicuspid and tricuspid or pulmonary orifices, and that this sign is as important as the duplication of the second sound, which occurs, according to Bouillaud, in narrowing of the bicuspid orifice.

It is also to be remarked that the souffle in question may exist in certain cases of aneurysm, dilated aorta (a retrograde course of blood being permitted) without aortic insufficiency being found after death; or it may exist, though not permanently, in chlorosis, typhus fever, lead-intoxication, when it only lasts for a time, and may be replaced by continuous bruits. Of course, moreover, the combination of mitral-valve lesions may render the diagnosis of aortic-insufficiency more difficult; and we may have cases in which the aortic insufficiency exists and yet no double intermittent crural souffle, as when the force of the arterial circulation is extremely feeble, or closure of the aortic orifice very extensive. Again, aortic insufficiency may be transient, as under the influence of pericarditis, and then the crural souffle may be transient.

(2.) Dr. Cockle's pamphlet, mentioned above, is divided into two parts. Part I. is occupied in tracing the history of insufficiency of the aortic valves, pointing out the development of our knowledge on this matter from the time of Harvey. Part II. commences with a re-consideration of the physiology of the circulation of the blood in the coronary vessels, a subject discussed by Haller, Boerhaave, &c., inasmuch as a right comprehension of the accident of sudden death in aortic insufficiency may depend on our views as to the "time and manner in which the arterial blood enters the coronary arteries." The author, after reviewing opposite arguments, comes to the conclusion that these arteries are filled by the systole of the aorta, and consequently, during a period corresponding to the heart's diastole; and he then passes on to show how far this view "can be made to harmonize in some essential particulars with the pathology of the disease in question." We need not go along with the author in his description of the various stages (as he terms them) which he describes, of aortic insufficiency, or of the clinical symptoms most commonly produced by each. The last one he calls the "degenerative" stage of the left ventricle, during which the ventricle is made to yield, owing to the backward passage of the blood through the aortic orifice. At this period death may occur from emboli in some cases, but in others, and they are the majority, sudden death is syncopal, or blended more or less with asphyxia.

For this two causes exist—the one, sudden pericardial effusion, "induced by the cardiac venous congestion from the diminished vis a tergo, owing to insufficient blood-tension of the coronary arteries;" the other, by reason of some exciting cause (it may be shock, pain, exhaustion, effort, &c.), the defective filling of the coronary arteries, themselves perhaps diseased, as a result of less elasticity in the aorta itself. This cause is intensified by predisposition to paralysis of the left ventricle from the vast blood-pressure on its walls. The paper closes with a long quotation from Traube of Berlin, containing a pathological explanation of the relation existing between a "defective aortic flooring" and the extinction of that first sound; and with remarks on the differential diagnosis of aortic regurgitant murmur, and judicious suggestions as to the management and treatment of the class of cases passed under review.

V. *On the Period of Incubation in Small-pox.* By Professor Dr. VON BARENSPRUNG. (Annalen des Charités Krankenhauses, &c., Band xix. Heft 1, p. 103.)

The author in this paper gives the abstract of several cases, showing that in small-pox the interval of time between the infection and the eruption is always the same even under the most varied circumstances. It is well known that in cases of *inoculation* the local affection began about the fifth day; from the eighth to the ninth the eruptive fever set in; and from the tenth to the eleventh the general eruption. But the action of "*contagion*" has been considered as less uniform, and the period of incubation has been variously estimated by writers. For example, Naumann states it to be three days; Wilson, at from four to twenty days; Canstatt, at from eight to fourteen; Heim, at nine days; Hufeland, at fourteen days; whilst according to Huxham, the eruption may be delayed for a month after the infection. Moreover, whilst fourteen days is considered by most as the medium period of incubation, it is considered that the character of the epidemic, as also the greater or less susceptibility of the patient, may modify it. The cases adduced by the author are certainly remarkable, and have all the character and value of direct experiment; they are as follows: On the 20th of January, 1851, a female who had never been vaccinated fell ill of confluent small-pox eight days after coming to stay in Halle, in which place, at the time, no cases of this disease existed. She died on the sixth day. On the 27th the body was examined post-mortem by Dr. Meckel, in the presence of several students and medical men; and as a result of the examination, no less than seven persons became ill of the small-pox. Of these, four had been present at the inspection, and three others were persons in close intimacy with those who had been present. In every one of these cases the disease broke out at the same time—namely, between from twelve to thirteen days afterwards.

A brief and condensed summary of these cases will be read with interest.

CASE I.—A student began to be affected on the evening of the 8th of February; on the day following the fever greatly increased; on the morning of the 11th a roseola on the hands and feet appeared; on the 12th numerous red knots were seen over the whole body, which soon became actual pustules. The patient was well at the end of the month.

CASE II.—A student felt unwell on the 8th of February, and on the 9th was in a high state of fever. The eruption appeared on the 13th. Termination favourable.

CASE III.—A student was taken ill on the 9th of the month. The eruptive fever was active, but the pocks only very scanty. Termination favourable.

CASE IV.—A physician was affected with fever during the night, between the 8th and 9th of the month. Eruption on the 13th. Termination favourable.

CASE V.—A student who lived with one of those who had been present at the post-mortem examination, but who was not there himself, being previously in sound health, became ill on the 8th, and on the 10th was in a state of high fever, with headache. Two days later the pocks broke out. Termination favourable. He had been vaccinated.

CASE VI.—A physician who was present at the inspection had carried home a piece of the skin affected with the disease for examination. He remained unaffected himself, but his wife, who was pregnant, and who had been vaccinated in her youth *and only a year previously, with good results*, began on the 8th of the month: day following, high fever. On the 10th delirium; on the 11th a scarlatina-like rash on the skin, delirium at evening; and on the 12th she was prematurely confined. On the 13th, remission of fever and lessening of the skin affection. The child lived fourteen days, and was unaffected by the disease.

CASE VII.—Was the child of the attendant at the post-mortem examination, who had sewn up and washed the body. It had not been vaccinated, and although its father and mother remained unaffected, fell ill on the 8th of February; on the 11th the eruption began, and on the 12th the whole body was covered. It died on the 14th with pulmonary symptoms.

Such are the various cases related by the author, which are so uniform in their nature, and occurred under circumstances so favourable to precise and scientific observation as to afford material for most trustworthy and valuable deduction. As he remarks, it is not a little remarkable that seven people so differently circumstanced and predisposed, some being vaccinated, others not so; some being adults, others children; some male, and others female, should, in spite of these differences, be all affected at the same period after exposure to contagion (and this notwithstanding that the intensity of the contagious influence was very unequal), for in every case the outbreak was between the thirteenth and fourteenth day after. The development of the skin-inflammation in the numerous cases was less uniform.

The author closes by relating one or two other cases which completely support the above observations, and which occurred under his own immediate notice in 1858.

VI. *On the Effects of Lightning upon the Human Body.* By Dr. W. STRICKER, of Frankfurt. (Virchow's Archiv, Band. xx. Hefte 1 and 2, p. 45.)

The author, after certain introductory matter regarding the nature of lightning, its conduction, general effects, &c., in which the views of several physicists are considered, passes on to the detailed relation of a great number of cases culled from the medical literature of various countries. From the particulars of these cases he forms a connected description of the usual effects produced on the human body, which is as follows. He supposes the person injured to have been either sheltering under a tree or to have been exposed openly. In the first case, the lightning in passing from the tree strikes the body on the neck or shoulder, causing a burn and much pain, with extravasation and congestion of the vessels over a broad part of the surface. From this a smaller stripe passes, running down to the nates, gradually getting less, and more superficial; but at the buttocks, where in the man the clothes fit tighter, the conduction is more intense. The lightning (1) either is conducted by the skin, thence towards the trochanter on one or both sides, the marks becoming weaker, and so on to the knee, where, owing to the tightness of the dress, it causes a deeper burning, runs along the calf, and then, if boots are worn, passes over and destroys them, or passes along the skin to the heel, and wounds it, and after piercing the shoe, makes a hole in the earth; often the lightning passes along the ankle. Or (2) the lightning may be conducted along the trousers, which it destroys, or pierces with only a round hole.

"When the lightning strikes a person freely exposed on the ground, then the head-covering is destroyed and the vertex smitten. From thence the conduction may be twofold—either (1) from the cranial bones to the brain, producing death by the simple or combined influence of the injury to the brain-mass, or by rupture of the bloodvessels; or (2) along the skin. In the latter case the skin of the face and neck is almost always completely spared, and the lightning effects a considerable burn over the sternum; in some cases it enters the mouth, affects the teeth and tongue, causes bronchitis, loss of voice, &c. Proceeding downwards, the track passes towards the inguinal region, only the shirt being sometimes torn, and then an interruption of conduction often occurs, occasioning deep burning of the groins, genitals, &c.; and occasionally we have mortal laceration of the intestines, or in milder cases injection of the liver, spleen, stomach, &c. The conduction then is through the skin or clothing, or both, to the back of the foot, where a wound is produced. The burning of the hair is very remarkable, and this may happen without any injury to the skin.

The burning produced by the lightning varies from that of the cautery to a mere drying of the tegumental surface.

The general results of injury by lightning are,—

(I.) In those killed, the rapid supervention of putrefaction and the dilatation of the pupils. The hæmorrhage from the nose and mouth is not sufficiently understood.

(II.) In those only outwardly injured (partly, perhaps, from fright).

(1.) Excessive stupefaction.

(2.) Great alternate depression and exaltation, long-drawn inspirations, small slow pulse, cooling of the skin, muscular debility.

(3.) Suppression of urine and constipation; "vomiting, loss of appetite, sometimes purging."

(4.) Great painfulness of the part affected, which increases for two days, and then declines.

(5.) On the uterus of pregnant women no peculiar effect is produced; like any other fright, it tends to arrest existing menstruation.

(6.) Cataract and nyctalopia may possibly be a result.

VII. *On the Pathology of Asthma.* By G. H. KIDD, M.D., Assistant-Physician to the Coombe Lying-in Hospital, Dublin, &c. &c. (Dublin Quarterly Journal of Medical Science, May, 1861, p. 292.)

In this article the author enters into the consideration of the physiology of the action of the bronchial muscles, and comes to the following conclusions:

1st. That during the paroxysm of asthma, the chest is distended to the greatest possible extent.

2nd. That all the muscles of inspiration are in spasmodic action (tonic spasms).

3rd. That the bronchial muscles are muscles of inspiration, and associated in the spasmodic action with the other muscles of inspiration.

4th. That breathing is carried on by the voluntary effort to aid the muscles of expiration, and that as soon as this is relaxed, the muscles of inspiration, like so many stretched bands of india-rubber, distend the chest again.

That the spasm of the bronchial muscles in asthma arises from some morbid action in the medulla oblongata, is to be inferred from the following facts:

1st. The fact that the spasm affects an entire group of muscles. Now, Schroeder Van der Kolk has shown that muscles associated in action are supplied by nerves arising from special groups of mutually associated and connected ganglion corpuscles. Disorder of this group would then manifest itself in the entire class of muscles.

2nd. Van der Kolk has also shown that the skin covering parts moved by muscles is supplied with sensitive nerves arising from the same segments of the spinal centre as the motor nerves of those muscles arise from. Dr. Salter has remarked, as an almost universal premonitory symptom of asthma, that there is itching of the skin under the chin, over the sternum, and between the scapulæ. This it is evident is a subjective sensation, and indicates an irritation existing at the roots of the nerves.

3rd. Paroxysms of asthma are observed to occur in cases of acute hydrocephalus, as in a case mentioned by Dr. Salter, and in one mentioned by Dr. Graves, where there were also general convulsions. In persons liable to epilepsy recurring at regular intervals, fits of asthma occasionally take the place of or serve as substitutes for the epileptic fit.

4th. The state of the patient preluding the fit of asthma indicates an affection of the nervous centres. In one there is mental exhilaration, in another mental depression. A patient of Sir J. Forbes's is awakened by convulsions in one foot and leg, and as soon as the asthmatic fit is developed, the convulsions of the extremity cease.

5th. The exciting cause indicates the same. In one, cold water applied to the instep will cause an attack; in another, going to bed with a loaded rectum, sudden emotion, &c. &c. The latter will also check the paroxysm when fully developed. Hence it may be inferred that asthma depends on a morbid state of the medulla oblongata and spinal centres, which manifests itself by throwing the entire group of inspiratory muscles into spasmodic action.

VIII. *On Syphilitic Diseases of the Larynx.* By C. Gerhardt and F. Roth of Würzburg. (Virchow's Archiv, Band xxi. (Zweite Folge, Band i.) Heft i. p. 7.)

After devoting some space to the history and literature of these affections, the authors notice the laryngoscopic labours of Czermak, Störck, and Türck, briefly alluding to the work by Ruhle on diseases of the larynx. No less than eighteen cases are detailed, illustrating every form of manifestation of syphilis found by means of the laryngoscope in connexion with the larynx and surrounding textures, including catarrh, condylomata, various kinds of ulcers, scars, cicatrices, affections of the perichondrium, &c., and the numerous general and local symptoms associated therewith. As a summary, it is stated that out of 56 people affected by syphilis, 18 were affected by disease of the larynx. Out of 19 men, 5, and out of 37 women, 13, had the larynx diseased. One case was observed in which a child eighteen months old, the subject of hereditary syphilis, had the larynx affected, condylomata following hoarseness, dyspnœa, and pain on pressure of the larynx. Of the 18 cases detailed by the authors, one-half occurred in patients in the third decennium of life; 6 in the fourth; 2 in the second, and 1 in the fifth decennium. Out of 44 patients with secondary (Ricord) syphilis, there were 11 with laryngeal affections, and out of 12 with tertiary symptoms, there were 7; so that it would appear that proportionately to the continuance of syphilitic symptoms is the liability to the disease of the larynx. The authors lay great stress on the fact that even for many years after primary syphilitic symptoms, the patient is not to consider himself free from the chance of having laryngeal affections; and on the fact that many will have such affections who are unaware that any such primary affection ever existed, but in whom cure is obtained by the use of mercury. The original manifestations of syphilis are often slight or are often overlooked, and cold or other irritation of the larynx, which, as it were, brings forth the laryngeal symptoms, is often considered by the patient to be the only cause of them. The communication concludes with a section on the prognosis of syphilitic laryngeal affections according to their character.

The following papers in various journals, which we have no space to analyse, may be mentioned as worthy of notice:

Recherches sur plusieurs maladies de la Peau réputées rares ou exotiques, qu'il convient de rattacher à la syphilis. Par Dr. Rollet. (Archives Gén. de Méd., Janv. Févr. Mars, 1861.)

Des Paralysies dans leur rapports avec les maladies aiguës, et spécialement des paralysies asthéniques, diffuses, des convalescentes. Par A. Gübler. (Archives Gén. de Méd., 1860–61. Seven Articles.)

De l'État mental des Épileptiques. Par Dr. J. Falret. (Archives Gén. de Méd., Dec. 1860, et Avril, 1861.)

De la Cataracte diabetique. Par Dr. E. Lecorché. (Archives Gén. de Méd., Mai, 1861.

Deux Observations d'Ataxie locomotrice progressive. Par Dr. F. Lecoq. (Archives Gén. de Méd., Juin, 1861.)

Bemerkungen über eine Frühyahre, 1860, in der Poliklinik in Kiel beobachtete Masernepidemie, mit besond. Berücksichtegung der dabie vorgekom. Lungen affectionen. Von Prof. Bartels. (Virchow's Archiv, Bd. xxi. (Zweite folge, Bd. i.) Hefte 1 & 2.)

Beiträge zur Lehre v. d. Uræmie. Von Dr. S. Oppler. (Virchow's Archiv, Bd. xxi (Zweite folge, Bd. i.), Heft 3, p. 260.)

Beiträge z. Symptomatologie u Diagn. der Galiensteine. Von Dr. C. Wolff, in Bonn. (Virchow's Archiv, Bd. xx. (Neue folge, Bd. x.), Hefte 1 & 2.

On Croup. By Dr. Whittle. (Dublin Quarterly Journal, Feb. 1861.)

On Chronic Subacute Arachnitis. By Dr. De Ricci. (Do., May, 1861.)

On the Action of Potash, Soda, Lithia, Lead, Opium, and Colchicum on the Urine. By Dr. Moss. (American Journal of the Medical Sciences, April, 1861.)

Clinical Report on 130 Cases of Pneumonia. By Dr. Flint. (Do., Jan. 1861.)

A Report on Epidemics and Endemics. By Dr. O. C. Gibbs. (Reprint from North American Med.-Chir. Review, Jan. and May, 1861.)

Quelques Observations sur les Troubles fonctionnels produits par le séjour prolongé dans les grandes Villes. Par Dr. Bourguignon. (L'Union Méd., Juin 11, 1861.)

De la Responsabilité des Épileptiques. Par Dr. Baillarger. (L'Union Méd., Mars 21, 1861.)

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On Diphtheritic Ophthalmia.* By M. Warlomont. (Annales d'Oculistique, tome xliv. pp. 115 and 196.)

Dr. Warlomont relates a case of this affection which, as far as he is aware, is the first which has occurred in Belgium, in order to contribute to that collection of facts which can alone reconcile the discrepancy of opinion entertained concerning even the reality of the disease; for while Graefe signalizes it as a special and almost endemic affection, other practitioners, no less worthy of confidence, with Mackenzie at their head, deny its very existence, regarding it as a mere symptom of ophthalmitis.

A custom-house officer, aged thirty, applied at the Brabant Ophthalmic Institution, on account of an affection of the eyes of a week's duration. The eyelids were red, swollen, and gorged with fluid, but admitted of eversion without difficulty or much pain, their inner surfaces exhibiting great engorgement, numerous distended vessels, small ecchymoses, and some thick mucopurulent secretion attached to the palpebral conjunctivæ; opposite the edges of the eyelids were stripes of firm, fibrinous, non-vascular false membrane, the detachment of which caused great pain and bleeding, and left a rough, unequal surface beneath. This membrane could not be better compared than with the plastic exudations in croup. Between the external angle and the cornea of each eye was a similar patch of firm exudations; the cornea being otherwise in a normal state. Chlorate of potass and a borax lotion were ordered, and in a day or two the false membranes began to disappear. But while the eyes were thus improving, diphtheritic exudations appeared on the lips and lining membrane of the mouth, covering with large plates the internal surface of the lips, gums, and velum, and rendering the voice hoarse and deglutition difficult. Eventually the patient did well.

In a subsequent paper, M. Warlomont gives an account of three cases which had been observed by M. Legros in the Belgian army, prior to the occurrence of his own case. The first of these presented itself accompanied by such acute symptoms that it was deemed to be a gonorrheal ophthalmia, and was vigorously treated by bleeding, calomel, &c. When the true nature of the disease was discovered, cauterization was resorted to, but its progress was not arrested and the eye was lost. One of the other soldiers was the subject of catarrhal ophthalmia, and the other of granular ophthalmia, prior to the diphtheritic exudation appearing; and both of them had occupied beds adjoining that of the first patient. The last two patients were successfully treated by frequent cauterization with nitrate of silver, cold applications to the eye, salivation being also induced by calomel. It is upon the cauterizations M. Legros lays greatest stress.

In M. Warlomont's opinion, pseudo-membranous conjunctivitis, or rather, the false membranes with which the palpebral conjunctiva is liable to become lined, may present different characters, and be dependent upon very different morbid conditions. The cases hitherto published seem to offer no other analogy than the presence of a newly-formed membrane; the symptoms accompanying it, the development, characters, progress, and stages of the disease being absolutely various. As long as the diagnosis of these various conditions is not rigorously exact, no uniform therapeutical agent can be recommended as a special agent for all these cases.

[In a subsequent number of the *Annales* (January, 1861), a translation is given of a thesis upon this disease as witnessed at the Clinic of Dr. Jacobson, of Königsberg. Its author, Dr. Lewinski, observes that whatever doubt may exist elsewhere, none can prevail in that town (in which diphtheritis may be said almost to be endemic) as to the reality of diphtheritic ophthalmia. In this thesis he details twelve cases, observed during an epidemic which occurred in the spring of 1860 (which has been described by Jacobson himself in the sixth volume of Graefe's 'Archiv'), and was attended with great devastation, many eyes being lost by perforation of the cornea; a pre-existing conjunctivitis usually acting as a predisposing cause.]

II. *On Extraction of Foreign Bodies from the Knee-Joint.* By M. Bauchet. (Moniteur des Sciences Médicales, 1861, No. XI.)

After passing under review the procedures of various surgeons, M. Bauchet observes that authors have not sufficiently insisted upon two points—viz., upon the place occupied by the foreign body at the moment of the operation, and upon the difficulty which often exists in incising the synovial membrane. For foreign bodies which change their position, it is preferable, when possible, to choose the exterior and lower part of the joint for attacking them. There we acquire more mastery over them, fixing better on the deep-seated parts, and directing more easily into the subcutaneous cellular tissue. Above, the synovial membrane is looser and more mobile, and the femur is plunged more into the midst of the soft parts. Below and internally we are too near the saphena and the spread of the tendons, the play of the tenotome is impeded by the tibia, and the manœuvres are more difficult. Once brought to the proper point, the foreign body must be steadily fixed there. In the great majority of cases it can be very exactly maintained *in situ* by the fingers of the surgeon or his assistant; but when it escapes easily, so that there is a fear of its doing so just as the tenotome is directed on towards the synovial membrane, we may without fear transfix it with a strong needle, a very pointed bodkin, or a small shoemaker's awl. The body being thus firmly held, the tenotome is passed in at three or four centimetres' distance through the cellular tissue until the joint is reached. But the opening into the synovial membrane is a delicate procedure, for with whatever care the pressure is made upon the foreign body, this readily slides away over the lateral parts of the joint; and when the tenotome acts directly on the foreign body, so that a button-hole aperture is made in the synovial membrane, the slightest derangement may remove the body from opposite the wound in the membrane. It is at this moment the body should be fixed and pressed, as if for its expulsion; and the author prefers holding it with one hand, while he makes the aperture with the other. This difficulty overcome, the body must be guided into the subcutaneous cellular tissue, where it may be left at two or three centimetres' distance from the articulation, first dividing the pedicle if there is one. At the end of some days, if no inflammation has been set up, and the wound in the articulation has cicatrized, a mere incision of the skin suffices for the removal of the body.

III. *On Sub-pubic or Ovalar Dislocations of the Femur.* By. M. Sédillot. (Gazette des Hôpitaux, No. 24.)

In this paper, M. Sédillot calls attention to the fact that in the most frequent form of dislocation of the femur, the sub-pubic, the ischio-pubic, or the ovalar, there may remain the power of standing and of walking with a certain amount of lameness, notwithstanding that no reduction has taken place. He gives instances in which soldiers have even been able to perform their marches. This persistence of the functions of the limb is explained by the relation subsisting between the head of the bone and the foraman ovale, this last by its depth and its borders presenting points of support favourable to the movements of the displaced bone. In spite, however, of this retention of power of motion, the symptoms of displacement will not be less obvious to the attentive observer. 1. The limb is elongated by from one to three centimetres, and seems to be still more so, owing to the depression of the pelvis on that side. 2. The great trochanter is carried inwards, backwards, and downwards, its region being at the same time depressed and flattened. 3. The entire limb is manifestly carried outwards, the foot being in a state of abduction. 4. The rotation of the foot inwards is usually impossible. 5. Flexion of the thigh on the pelvis is tolerably easy as long as the limb is in a state of abduction, but ceases to be possible if the thigh is placed in a state of adduction. 6. There is no obstacle to extension. 7. Ovalar luxations are the most frequent and least serious of the varieties of dislocation of the femur. 8. They are most commonly met with in the young, and in persons whose joints are naturally very lax. 9. Reduction is effected without much difficulty, even at the end of several weeks, by making traction from within outwards at the upper part of the thigh, while the knee is directed inwards and forwards, and placed in adduction when the head of the femur is supposed to have reached the brim of the acetabulum. 10. The only precaution necessary for the prevention of relapse, is to maintain the limb elongated and in a state of gentle adduction. 11. The recovery is rapid and complete. 12. In case of reduction not having been effected, the patients still are frequently able to employ the limb, but suffer from a certain amount of lameness.

IV. *Case of Fracture of the Sternum.* By Professor Fischer. (Wien Spitals-Zeitung, No. 7.)

Professor Fischer is certainly justified in assigning the rarity of this accident as a reason for reporting an additional example of it; for we find by the statistics which E. Gurlt has collected from various sources, that of 13,000 recorded fractures only 30 were examples of fracture of the sternum, while according to the returns of the London Hospital for the years 1842–60, only 20 instances of fractured sternum occurred among more than 21,000 cases of fracture treated there within that period.

The subject of the present case was a strong labourer, fifty years of age, who fell from a height and continued unconscious for four hours. On recovering he felt severe pain at the front of the chest, which was much increased by every movement, and especially so by inspiration. Twelve days after the accident he came to the Innsbruck Clinic, having his head bent so forward that the chin touched the upper part of the sternum, having maintained this painful position since the accident. The sternum, under the place of attachment of the third rib, exhibited a sharp, angular, bony projection, which extended obliquely downwards from the right side to the left. Above this spot was a deep depression, at the bottom of which the upper portion of the sternum could be felt. On the application of strong pressure, and amidst crepitation and severe pain, the projecting part could be pressed somewhat backwards, returning to its former position, however, with a spring on the cessation of the pressure. The anterior segment of the fourth and fifth ribs projected strongly forward, while the upper three pair having followed the upper portion of the fracture dislocated backwards, the superior part of the anterior wall of the thorax seemed remarkably depressed. The spinal column was bent strongly backwards. The thorax was almost immovable during respiration, a deep inspiration, on account of the severe pain it gave rise to, being impossible. Percussion and auscultation revealed normal sounds only. Ordinarily, the accident results from the application of direct violence to the sternum, and a transverse fracture or separation takes place at the point of junction between the manubrium and the body of the bone. In the present case, the fracture took place obliquely through the body of the bone, and seems to have occurred during the violent curving forwards of the vertebral column at the moment of the man's fall. At least, some injury to the occiput favoured this explanation. The treatment chiefly consisted in keeping the patient in the horizontal position, without a pillow under his head, attempts at reposition of the fragments having been deemed unadvisable. At the end of eight weeks consolidation had taken place, without any appearance of the formation of callus externally. The lower end of the fracture constituted an unsightly projection, and the upper portion of the anterior surface of the thorax continued depressed. The pectoral muscles were on both sides greatly atrophied, and the thoracic vertebræ were strongly curved backwards; but the functions of respiration and circulation were not impeded. Some time after his dismissal, the patient returned to the hospital complaining of his inability to work on account of the still continuing muscular atrophy.

V. *Treatment of Perforated Intestine in Strangulated Hernia.* (Gazette des Hôpitaux, Nos. 26, 29, 32, 35, 38, 41, and 44; and Gazette Hebdomadaire, No. 9.)

A case related by M. Bauchet to the Paris Surgical Society gave rise to an interesting discussion upon the manner in which intestine which has become spontaneously or accidentally perforated in strangulated hernia should be treated. On exposing a femoral hernia, a minute aperture, through which bubbles of air escaped, was observed; and relying upon some successful cases published by Velpeau, the gut was nevertheless reduced without any suture having been applied. The patient went on well for some days, when erysipelas supervened, which carried her off. Fœcal matters had been discharged from the wound; and at the autopsy, an artificial anus was in a fair way of formation. M. Bauchet stated, that in the event of a similar case occurring to him, he would pass a ligature through the mesentery, so as to keep the intestine near the hernial region, reduce the strangulated noose, with or without a suture, according to the size of the perforation, and administer opiates. M. Chassaignac, while believing the reduction of a healthy portion of the intestine, which had become slightly wounded during the operation, justifiable, would prefer the English method of applying a lateral ligature, which is certainly indicated when the intestinal coats are diseased. M. Verneuil objects to the reduction of intestine, even with a very minute perforation, for we have no security against the extension of the ulceration, and even a slight effusion may do enormous mischief. With respect to the application of a suture prior to reduction, Mr. Verneuil relates a case in which a puncture accidentally made during an operation was closed by two points of suture. After doing well at first, the patient succumbed to peritonitis on the sixth day; and it was found that the wound had reopened after the fall of the sutures, and given issue to fatal effusion. M. Verneuil seems to place little reliance on any other means than the formation of an artificial anus. However desirable this may be in the case of large apertures in the intestine, M. Broca cannot agree in its propriety in minute perforations. When these are accidental, he regards the English practice of applying a ligature as the best; while, when they are the result of the action of the stricture, endeavour should be made to excite adhesions between the intestine and parts in its vicinity. M. Giraldes has a high opinion of the efficacy of the suture, especially that of Gely of Nantes. He referred to an instance of its success in his own practice; and without much research he has collected references to twenty-two published cases, in which the suture has been applied, either for gangrenous ulceration, simple perforation, or wounds made during the operation, and in seventeen of these the result was favourable. M. Richet considers that when the intestine exhibits only superficial erosions, it should be gently returned, and left near the internal ring; while when there is ulceration, it should be kept externally by means of a ligature passed through the convexity of the intestine and the sac. M. Jarjavy is of opinion that we sometimes reduce a perforated intestine contained in femoral hernia without being aware of it—the perforation lying concealed at the bottom of the groove produced by the constriction. He believes, also, that rupture has been induced by the administration of purgatives after the operation. The possibility of inducing perforation by drawing out the gut for the purpose of examination having been adverted to, M. Velpeau observed, that although he did not now so exclusively prohibit this practice as heretofore, it was one which called for great care when the intestine had undergone much change. When there is great resistance to this traction, after the division of the stricture, the gut may be gently passed up again, bringing it down at once on the occurrence of the slightest fæcal discharge. M. Velpeau having published five instances in which success had attended the return of perforated intestine, he had been too readily regarded as an advocate of this practice as a general procedure. In all these cases, however, the gut was quite healthy, and only a minute aperture existed at a single point. He considers that the formation of an artificial anus had been too lightly spoken of during the discussion, for not only is it sometimes an incurable affection, but many die before it can be established. One point he desired to insist upon—namely, the impossibility of laying down any absolute rules for operation in hernia. Each case is a new fact, which taxes the ingenuity of the surgeon. M. Huguier has been in the habit of endeavouring to supersede the necessity of making traction on the intestine, by desiring the patient to cough; and when this does not bring it down sufficiently, he applies a forceps to the mesenteric border of the intestine, and to the mesentery. In the case of perforated intestine, he disapproves of both its reduction and the application of a suture. He respects any adhesions that may exist, and endeavours to fix the intestine either just above or within the canal by passing a thread through any portion of omentum that may be found. If this cannot be done, the thread is passed through the portion of the intestinal coats which seems to threaten to become gangrenous. When, however, the intestine is as cleanly divided by the stricture as by a cutting instrument, or when it is accidentally wounded during the operation, three or four points of suture may be executed by passing a fine needle and flat silk through one or two of the tunics only, so as to bring the serous surfaces into contact. M. Gosselin has always, in the sixty operations which he has performed for hernia, made traction upon the intestine for the purpose of examining it. He does not think that a surgeon is justified in endangering the life of his patient by returning a portion of intestine in which even a minute perforation exists. Nor does he approve of the application of a suture or the laying open the perforated intestine in order to form an artificial anus. He believes that the best practice consists in leaving the perforated gut in the wound, after having liberated the stricture. If gangrene supervenes, an artificial anus is the result; while, when a mere fæcal fistula results, reduction takes place slowly and spontaneously, the aperture having become cicatrized or effectually secured by means of the adhesions which have sprung up. No ligature or other means is required to retain the intestine in the wound, for unless compression be employed, it has little tendency to return suddenly. M. Demarquay commented upon the danger of the practice so generally followed in France, of administering purgatives immediately after the operation. He has derived great advantage from substituting divided doses of opium during the first twenty-four hours. M. Robert observed that traction is easily enough made in inguinal hernia, which usually contains an entire noose of intestine: but the case is very different in small femoral hernia, having a portion of intestine very deeply placed, much narrowed at the seat of stricture, and distended above this. If the gut is inflamed and friable, persistence in traction may easily lacerate it. M. Chassaignac advocates making traction in order to ascertain the condition of the intestine, and the reality of its liberation. An artificial anus, in the case of perforated intestine, he observes, is not always of easy formation; while fæcal fistulæ frequently heal spontaneously. The best practice, therefore, consists in relieving the stricture, and leaving the perforated gut near the ring.

VI. *On Polypi of the Rectum.* (L'Union Médicale, 1860, No. 129.)

During a discussion upon this subject at the Société de Chirurgie de Paris, M. Guersant, who has had great opportunities of observation while surgeon to the Children's Hospital, observed that these tumours have in general a more or less long and thin pedicle, which suspends them just as the stalk of fruit retains it to its tree. The tumour is usually very hard, and slides like a cherry-stone pressed between the fingers. These productions are not confined to the rectum, and M. Guersant has met with one attached to the ileo-cæcal valve. M. Demarquay stated that he had removed a fibrous polypus as large as a walnut, having a pedicle four or five centimetres in length, and which, issuing from the anus whenever the patient went to stool, was mistaken for a hæmorrhoid. M. Huguier on a former occasion had laid before the Society a polypus as large as a fowl's egg, and another the size of a chesnut, and which seemed both to consist of hypertrophied cellular tissue and highly-developed vessels. Sometimes polypi do not exceed large cherries in size, or they may be even as small as a nut, a pea, or a fruit-seed. Under these circumstances they are usually multiple; and whether they be multiple or only single, they may give rise to exhausting hæmorrhages. When the polypus is single, it is easily removed, either by excision preceded by the ligature or by linear *écrasement:* but when the polypi are numerous and give rise to copious hæmorrhages, we must have recourse to crushing (*broiement*) them in order to extirpate them. M. Richet treated one of his cases in this manner, and the hæmorrhage ceased for several months. This returning, he introduced a speculum, and having exposed the numerous polypi, twisted their pedicles with a forceps, and then cauterized immediately afterwards the point of implantation by means of a probe heated to whiteness. He in this way removed about seventy polypi, withdrawing the speculum gradually from the deep portion of the rectum towards the anus M. Robin having examined these polypi, found them to consist in a hypertrophy of the mucous follicles and rectal membrane. The patient was perfectly cured. Polypi of the rectum vary no less in their structure than in their size and numbers. Some consist in a hypertrophy of the tubuliform glands of the rectum, while others are cancerous or erectile. Structures so varied should not be designated by a collective name, and as MM. Broca and Verneuil have suggested, it would be well to no longer employ the term polypus, but to designate the tumours by a word indicative of their nature, adding merely the term pediculated—e.g., a pediculated cancerous tumour of the rectum.

VII. *On Operating for Fistula in Ano in Phthisical Subjects.* By Professor Thiry. (Presse Médicale Belge, No. 21.)

These observations are intended by their author as a sort of protest against the doctrine maintained by M. Jobert—viz., that the objection usually held by surgeons to operate for fistula in patients suffering under phthisis is a mere prejudice, to be entirely discountenanced; the fistula, in fact, being just like any other emunctory, a cause of debility in this disease, and as such to be suppressed as soon as possible. If the action of a fistula resembled that of a prolonged blister or an issue, as stated to do by M. Jobert, Professor Thiry would agree with him in regarding it as an unfortunate complication to be got rid of as soon as possible. But this is only a faulty interpretation; for he has found in all the cases that have come under his care, that fistulæ and abscesses occurring about the anus in the subjects of pulmonary tubercle, are the result of tubercular deposit at the margin of the anus, constituting only an additional manifestation of the general diathesis. The discharge from such is not an evacuation of matter enfeebling the patient, but a discharge of tubercular matter mingled with the pus, and benefiting the patient thus far by removing tubercle which might otherwise have been deposited in the lungs and aggravated his condition. The matter contained in the discharge from these anal abscesses or fistulæ, is shown by microscopical examination to be in part tubercular, and unsoftened tubercular matter also lines the bottom of the cavity whence it proceeds. Soon after the establishment of the fistula the chest symptoms often undergo a notable amelioration, while the patient exhibits many other signs of returning health. If this truce be taken advantage of by the administration of suitable remedies, complete recovery even may ensue, the fistula itself, the last trace of the tubercular diathesis, disappearing spontaneously. Such a result is by no means so rare as is generally supposed, and it would be of yet more frequent occurrence if fistulæ were more frequent than they are, and if proper perseverance were observed in the application of remedies and the observance of an azotized diet. Under the influence of fistulæ and of the treatment which they allow of being put into force, the author has known cavities to have become cicatrized; and so far from regarding them with Jobert as aggravating complications which should be at once removed, he regards them as highly salutary, and would recommend their provocation by every possible means when nature does not produce them spontaneously. The success which M. Jobert states that he has obtained in operating in these cases can only be explained by the supposition that he has had to do only with fistulous tracts proceeding from mere phlegmonous abscesses. A phlegmonous abscess, quite independently of tubercle, may become developed at the margin of the anus, and the resulting fistula may be operated upon with even advantage to the patient; but the difficulty is to distinguish such a case from a fistula acting as a means of elimination of tubercular matter, to the great advantage of the pulmonary affection and the general constitution. The decision is sometimes difficult, and always important, as the very life of the patient may be dependent upon it. However, the general conclusion at which M. Thiry arrives is, that the operation for fistula performed at any stage of phthisis only precipitates its fatal termination. He selects two of the cases which have come under his own notice, as illustrative of the advantage derived from respecting the fistula, at both an early and a late stage of the affection; and he adverts to others in which a contrary practice has been followed by the worst results.

VIII. *Question of Removal of Tubercular Testis.* (Bulletins de la Société Anatomique, tome v. pp. 215-217.)

On the occasion of the presentation of a case at the Anatomical Society, M. Cruveilhier observed that in his opinion castration is sometimes too hastily resorted to in tubercular testis. As a general rule, the affection has a tendency to become spontaneously cured, the tubercular masses breaking down and being eliminated. He does not consider that the question of the surgeon's interference is at all definitively decided. M. Houel observed that the majority of surgeons regard the affection as primarily local, and afterwards becoming generalized. Intervention should therefore take place promptly, in order that the extension of the disease may be prevented. M. Potain had seen Malgaigne obtain cures by mere ablation of the degenerate tissue. M. Dussour observed that, as a general rule, he regarded the operation as contraindicated; for, in the first place, it is by no means devoid of danger, while the disease for which it is resorted to is not necessarily fatal; and, secondly, the ablation of the testis does not prevent the invasion of the seminal passages. He has more than once seen individuals, for whom castration has been performed by M. Ricord, return with tubercle in the prostate, the vesicles, or the other testis. It must always be borne in mind, he added, that tubercles may exist in the prostate or the vesicles before manifesting themselves in the testis, and exploration by the rectum should be always resorted to. Enlargement of the prostate, accompanied by urethral discharge, and coinciding with tumour of the testis, is one of the best signs of tubercular sarcocele. In M. Caudmont's opinion, surgical intervention is of no avail in this affection, for in most cases it is preceded by tubercle of the prostate. In general, the first symptoms are those referrible to the neck of the bladder, tumefaction of the epididymis and the testis only appearing at a later period. M. Houel had not found the cases which came under his own notice bear out this view of M. Caudmont, for he had not in these met with tumefaction on rectal exploration. M. Caudmont, however, referred to the number of specimens which had been from time to time laid before the Anatomical Society, plainly showing the lesion far more advanced in the prostate and vesicles than in the testis and epididymis. M. Dufour remarked that there is some difficulty in coming to a decision upon this point, a certain number of cases bearing out M. Houel's statement; and others, and as he believes a larger number, corroborating the opinion advanced by M. Caudmont. There is, in fact, the same difficulty as when, having to do with pulmonary tubercles and a white swelling, we seek to determine which of these two affections has preceded the other.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.
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I. Diseases of the Uterus.

1. *Inversion of the Uterus caused by a Polypus.* By Dr. Abarbanell. (Monatsschr. für Geburtsk, Feb., 1861.)
2. *A Case of Acute Tuberculization of the Puerperal Uterus, and a Case of Tubercle in the Ovaries.* By Rokitansky. (Allg. Wiener Med. Zeitung, 1860, and Mon. f. Geb., Feb., 1861.)
3. *A Case of Twisting-off of the Left Fallopian Tube, with Apparent Reception of an Ovum from the Left Ovary by the Right Tube.* By Rokitansky. (Allg. Wiener Med. Zeitung, 1860, and Mon. f. Geb., Feb., 1861.)

1. Dr. Abarbanell records a case of inversion of the uterus caused by a polypus. A woman aged forty-five had borne four children, the last eight years ago. Since then she had suffered irregularities of menstruation; latterly hæmorrhagic and other discharges had been frequent, and had reduced her to a very anæmic state. Examined, a smooth tumour the size of the fist was felt protruding from the uterus. Fourteen days later, under violent expulsive pains with profuse hæmorrhage, the tumour was driven through the external genitals, and the uterus was completely inverted. The tumour was amputated, leaving only a small layer of the stalk at the fundus, whereupon the uterus quickly replaced itself. The patient recovered. The tumour consisted of connective tissue and numerous muscular fibres.

2. Rokitansky observes that chronic tuberculization of the uterine mucous membrane is sufficiently well known, but that acute tuberculosis of the uterus

has not hitherto been observed. As a proof that it may exist, he relates a case. A woman aged thirty-four had borne eight children, and on the 6th January, 1860, she was delivered of an eighth month child. On the 13th she was admitted at hospital, stating that she had suffered from cough for three months. On the 25th she died. On dissection were found granules, tubercles in the lungs, and in the mucous membrane of the uterus a crowd of small grey and grey-yellowish discrete tubercle-granulations the size of poppy or millet-seeds, causing the mucous membrane to look as if finely corroded. Under this, the uterine substance, even to the thickness of the wall as far as the peritoneum, was studded with these granulations. Grey tubercles were also found in the mucous membrane of the right Fallopian tube.

Tubercular formation in the ovaries is very rare. In the following case it was found in connexion with pulmonary and intestinal phthisis. Both tubes were adherent to their ovaries; their mucous membrane was infiltrated with tubercular matter. The uterine mucous membrane was studded with isolated, very small tubercles. The left ovary contained tubercles from the size of hemp-seed to that of beans, yellow, cheesy, the peripheral ones projecting on the surface. The woman was forty-four years old.

3. Rokitansky's case of apparent reception of the ovum by the opposite Fallopian tube is as follows:—On the 12th April, 1860, he examined the body of a woman aged thirty, who had been delivered on the 11th. The uterus was the size of a child's head; yellowish exudations were found on outer surface. On the left the uterus was covered with whitish pseudo-membranes, which were continued over the broad ligament along the stretched degenerated tube, in knots and bands, as far as the sigmoid colon. The left tube for a space four inches long was a whitish permeable string; at the outer end it was sharply bent, and its canal was lost; it disappeared in the above-described bands of adhesion. In the left ovary, near its outer extremity, was a small corpus luteum, whilst in the right ovary there was no trace of corpus luteum. Rokitansky concludes that the adhesions had been acquired after development of the genital organs. The degeneration of the tube had been caused by the dragging of the pregnant uterus. These adhesions had not arisen during the last pregnancy. It must hence be concluded that the ovum had been taken up from the left ovary by the right Fallopian tube.

II. Labours.

1. *A Case of Puerperal Convulsions.* By Dr. C. Habit. (Zeitschr. f. d. G. d. A. zu Wien, Oct. 1860.)
2. *Complete Inversion of the Uterus; Reduced.* By E. W. Woodson, M.D. (Amer. Journ. of Med. Sc., Oct. 1860.)
3. *Varix of the Umbilical Cord: Hæmorrhage.* By Dr. Nagel. (Annalen d. Charité-Krankenhaüses, Berlin, 1860.)
4. *A Case of Spontaneous Expulsion, after failure to turn.* By Dr. Nagel. (Opus suprà-citatum.)
5. *On the Influence of Tumours of the Genital Organs upon Pregnancy and Labour.* By Dr. C. Habit. (Zeitschr. der Ges. d. Aerzte zu Wien, Oct. 1860.)
6. *A Stalked Fibroïd on Peritoneal Surface of the Uterus; Incarceration in the Pelvis during Pregnancy and Labour; Re-position.* By Spaeth. (Monatsschr. f. Geb., Nov. 1860.)
7. *Thirteen Cases of Cæsarean Section.* By Dr. Winckel. (Monatsschr. f. Geb., Dec. 1860.)
8. *Cæsarean Section, with unfortunate Result for the Mother, in Obliquely Distorted Pelvis and Occlusion of the Uterus.* By Dr. Birnbaum. (Monatss. f chr. Geb., Dec. 1860.)

9. *A Case of Cæsarean Section.* By M. ANDRIEU. (Gaz. Méd. de Paris, No. 38, 1860.)
10. *A Case of Cæsarean Section.* By JAMES EDMUNDS, L.R.C.P.Ed. (Lancet, and Med. T. and Gaz., Jan. 1861.)
11. *Cæsarean Section after Death; Child saved.* By Dr. BONNET. (L'Union Méd., Nov. 1860.)
12. *A Case of Cæsarean Section with Successful Issue.* By Dr. GLISCZYNSKI, of Warschau. (Monatsschr. für Geburtsk. Feb. 1861.)
13. *Forty-fifth Annual Report of the Lying-in Institution of Dresden for* 1859. By Professor GRENSER. (Monatsschr. f. Geburtsk. Feb. 1861.)
14. *A Dynamometrical Adaptation to the Forceps.* By Dr. KRISTELLER. (Monatsschr. f. Geburtsk. March, 1860.)
15. *A Successful Case of Transfusion.* By Professor MARTIN. (Monatsschr. f. Geburtsk. April, 1860.)
16. *On the Most Proper Method of Removing the Placenta.* By Dr. CREDÉ. (Monatsschr. f. Geburtsk. April, 1861.)
17. *On Perforation and Cephalotripsy.* By Dr. SPÖNDLI of Zurich. (Mon. f. Geburtsk. March, 1861.)

1. Dr. Habit's case of convulsions is extracted from his 'Memoir on Tumours of the Female Genital Organs.' A woman, aged thirty-two, in her eighth month of pregnancy, was admitted in a soporous condition. Fourteen days before she had had eclampsia; the fits yielding, she had been discharged. The eclampsia now returned with albuminuria, and ended in death. By Cæsarean section a dead child was extracted. A submucous fibroïd tumour projected freely into the cavity of the uterus. Dissection revealed capillary apoplexy of the meninges, apoplexy of the pons Varolii, œdema of the lungs, hypertrophy of the heart, Bright's disease of kidneys.

2. Dr. Woodson states a case of complete inversion of the womb which he reduced in a manner that deserves to be remembered. A woman, being about four months pregnant, whilst washing was seized with labour pains, and feeling the fœtus protruding, she pulled it from her, bringing the uterus out, and completely inverting it. Five days having elapsed, Dr. Woodson saw her; he found the fundus presenting externally, about the size of a large pear, a portion of placenta almost decomposed still adhering. Attempts to replace by hand failed. Next day he had prepared an instrument similar to Simpson's sound, provided with a ball at the end the size of a half-ounce bullet. This ball was placed against the fundus, and under steady pressure, the womb was replaced with a jerk. The loss of blood was not great, although it had continued from the time the accident had occurred. The patient recovered well.

3. Dr. Nagel relates an unusual case of hæmorrhage from the umbilical cord. A woman was delivered in the Charité Lying-in Hospital of Berlin in May, 1858. During the second stage of labour, a considerable escape of blood from the vagina took place, suggesting the suspicion of premature separation of the placenta. The hæmorrhage was found to have arisen from a ruptured varix of the umbilical cord. The child was born dead, with the clearest signs of anæmia. The cord was twenty-eight inches long, and encircled the neck three times.

4. Dr. Nagel relates a case of spontaneous expulsion of a full-grown fœtus. In December, 1856, a woman who had been in labour twenty-eight hours was brought to hospital; the liquor amnii had long escaped; for forty-eight hours no fœtal movements had been felt. The pelvis was of full size; the child presented in the second shoulder-position; the pains were good. Chloroform was administered to facilitate turning; but all efforts persisted in during two

hours and a half failed in reaching the feet, so strong were the spasmodic contractions of the uterus excited by each endeavour. It was determined to wait; presently, under strong pains, first the right shoulder revolved under the pubes, then the right side, then the breech, and lastly the head. The mother recovered favourably. The child showed signs of commencing putrefaction. [The case is an example amongst many, that chloroform may increase the difficulty of turning, a position which, when stated as the result of repeated experience by the Reporter at a meeting of the Obstetrical Society of London, excited some surprise amongst those who had arrived at an opposite conclusion from *à priori* reasoning.—Rep.]

5. Dr. C. Habit has written an interesting memoir upon the influence of tumours of the genital organs upon gestation and labour. After enumerating cysts of the labia majora and vagina, cysts of the ovaries, fibroïd tumours of the uterus, cauliflower excrescence of the cervix, he says cysts of the ovaries and fibroïd tumours are by far the most frequent.

Case 1. A woman, pregnant about six months for second time, was admitted with vomiting, and acute pains in the abdomen. A clyster was followed by two stools, when suddenly convulsions set in, and death speedily followed. A feeble child was extracted by Cæsarean section. Near the ovum in the uterine cavity was a considerable quantity of fluid blood, and in the left wall of the uterus was a fibrous tumour, the size of a fist. The cause of death was intermeningeal apoplexy. In the abdomen was some fluid and clotted blood.

Case 2 is referred to above.

Case 3. A woman, aged twenty-six, pregnant for the second time, admitted 11th November, 1855, a month before term. The head was felt above the left pubic bone, and could not be moved by pressure. The os uteri was difficult to reach, high and forward over the symphysis. The posterior and upper part of the pelvis was filled by a round, tense, elastic tumour, which it was impossible to push out of the pelvis. The most projecting part was within 2″ of the symphysis. Examination by rectum showed that the tumour did not spring from the pubic bones. The tumour being elastic and the fœtus small, the labour was effected by the natural passages, under violent pains, the child being driven over the tumour. It was born dead. It exhibited effusion of blood in the arachnoïd cavity. Examined eight days later, the uterus was found in fair retrogression, and attached to right and somewhat behind, rose a tumour reaching to the navel. The tumour in the pelvis formed one with the abdominal growth. The patient was discharged well.

Case 4. *Complication with Ovarian Cyst.*—A primipara, aged twenty-eight, was examined by a female pupil, who diagnosed a twin-pregnancy on the grounds of great distension of the abdomen, especially transversely, and a division of the abdomen in two halves by a distinct furrow. Dr. Habit, however, by percussion, diagnosed an ovarian cyst, complicating gestation. A fortnight after admission, being at term, labour came on, and a living child was born. The patient suckled her child, and was discharged. The ovarian tumour remained unaltered.

Case 5. In another case of ovarian cyst, abortion took place under symptoms of collapse ending in death. The uterus and appendages were found covered with exudations. The cavity of the cyst, which was in the left ovary, contained about two quarts of fluid and little-changed blood. Thus there was general peritonitis and hæmorrhage into the cavity of the cyst.

Case 6. A woman, aged twenty-three, was admitted in her fourth month, with symptoms of peritonitis. She had experienced sacral pain and a sensation as if of a foreign body in the pelvis. Above the symphysis was felt a semi-globular, painless, moveable tumour, resembling the pregnant uterus. Internally, above the ostium vaginæ, was a tumour, the size of a fist, smooth, elastic. The cervix uteri was jammed high up against the upper border of the symphysis. Attempts to place the tumour above the pelvic brim failed. The most prominent spot was punctured with an exploring needle, and about two pints of serous fluid escaped. The symptoms were allayed. Some discharge continued from the opening. Five months later she went through her labour normally at term. No trace of the tumour remained. Probably, in this case there had been a single cyst, the result of an oophoritis, the walls of which had collapsed after puncture and degenerated.

6. Dr. Spaeth's case is an interesting example of a mode in which a fibroïd tumour of the uterus may complicate labour. He had ascertained during pregnancy the presence of a tumour filling the pelvis. During labour, the hard tumour so filled the pelvis, that the os uteri could not be reached with the finger. The right arm prolapsed. By great perseverance, the re-position of the tumour above the pelvic brim was effected. Turning was immediately completed, and a living child delivered, the head being extracted by forceps. No hæmorrhage. The patient died on the third day. Dissection showed metrophlebitis, peritonitis, fibrous knots under the uterine peritoneum. The tumour lay under the peritoneum in the middle of the fundus, connected with the uterine substance by a short stem.

7. Dr. Winckel, a physician practising at Gummersbach, near Cologne, gave a summary account to the Berlin Obstetrical Society of thirteen Cæsarean sections performed by himself. He described the condition of the lower classes in his vicinity, and pointed to their uncleanliness, poverty, and lack of animal food as the causes of the great frequency of osteomalacia there. He had thus been obliged during a practice of nineteen years to perform this operation thirteen times. Of these 13 cases, 8 were in consequence of osteomalacia, and 5 of rachitis. In 4 cases, the incision of the abdominal walls only was necessary, the children having been cast into the abdominal cavity through rupture of the uterus; and in 1 case in which the Cæsarean section had been performed in a previous labour, the child was enabled to pass through the old uterine cicatrix. Of his operations, eight, pertaining to six women, ended in recovery. Dr. Winckel attributes this success greatly to his passive method of treatment. Compelled by the exigencies of country practice to leave his patients for long intervals out of sight, his chief attention had been fixed upon securing the greatest possible cleanliness of the wound. The wound was closed by five sutures, and between these by several insect-needles. His internal treatment consisted in an infusion of ipecacuanha with opium. He had hardly ever seen inflammatory affections supervene. The insufficiency of assistants had taught him a practical manipulation in guarding against protrusion of the intestines. When he has opened the uterus, his assistant seizes with one finger the upper angle of the uterine wound, and pulls it into adaptation with the angle of the abdominal wound; when the incision is completed downwards, the assistant performs the same manipulation on the lower angle of the uterine wound with his other hand. The uterus and abdominal wall thus held tightly together, the intestines cannot escape. The operator himself has both hands free. All the operations were performed under chloroform-narcosis.

It will be regretted that the unusual experience enjoyed by one operator has not been detailed with more fulness. Two of Dr. Winckel's cases, however, will be found related with every essential circumstance in the 'Monatsschrift für Geburtskunde' for January, 1861.

8. On the same occasion a communication from Dr. Birnbaum, of Cologne, was read, describing a case of Cæsarean section in great detail. A woman, aged thirty-two, belonging to Niederbolheim, had gone from her fifth to her

ninth year upon crutches; she menstruated with difficulty at the age of seventeen. She became pregnant for the first time at twenty-five. After five days' labour a physician was called. Delivery was accomplished after great difficulty by forceps and blunt hook. Inflammation of the soft passages and abdomen followed. In her second pregnancy the pelvis was examined: the distance between the anterior superior spinous processes was 11·25″. The sacrum was strongly twisted to the left. The vagina was completely closed, forming a blind sac. The promontory was 3·5″ distant from the pubic arch, and depressed strongly forwards and somewhat to the left. The period of her pregnancy could not be defined; it was probably about the seventh month. Electricity was applied, with the view of bringing on labour. The uterine sound was passed through a fistulous opening in the cicatrized os uteri. In eight days pains appeared, and the liquor amnii escaped. It was not considered advisable to open the uterus from below by incisions in the cervix. The abdominal section was performed under chloroform. Free bleeding occurred when the uterus was incised; the placenta bulged forwards immediately. The fœtus was extracted living; it survived. The mother died ninety-eight hours after the operation. On dissection, the uterus was found covered with grumous dark-red fluid, which filled the abdominal cavity and pelvis.

9. M. Andrieu relates another case of Cæsarean section. A woman had borne four children in four years; the first had been delivered dead by forceps; the two next were delivered dead by turning, on account of cross-presentation; and the fourth had been perforated. There was a bony tumour in the region of the sacro-vertebral angle; the conjugate diameter measured a little less than an inch. This was at the end of her fifth pregnancy, and as the child was alive, the Cæsarean section was performed. The wound was closed by the twisted suture, and had united at the end of six weeks. The child did well.

10. A case in which the Cæsarean section was performed at term, the obstacle to delivery being cancer of the cervix uteri, is related by James Edmunds, L.R.C.P. Ed. Both mother and child survived the operation.

11. Dr. Bonnet relates a case of death from apoplexy and post-mortem Cæsarean section, the child being saved. He was called to a woman whom he found insensible, not moving the left side. She was pregnant for the first time; she had complained of headaches, and tingling in the left hand and foot. She soon died. Waiting this event, M. Bonnet prepared to open the abdomen. The placenta was found on the anterior wall; on incising this wall a jet of blood was thrown up. The child was born asphyxiated, but in a quarter of an hour its life was safe.

12. Dr. Glisczynski's case of Cæsarean section refers to a primipara aged twenty-eight. Labour continuing three days, Dr. Glisczynski was called in. The patient had suffered from rickets from three to seven years old. The promontory was only two inches removed from the symphysis. The fœtal heart was heard, and the mother's condition was good. The section was carried out under chloroform. The child was extracted living, and did well. The wound was dressed with lint, bands of adhesive plaster were brought round from the back and were crossed over the wound; the whole was supported by a towel. The wound was nearly closed on the seventeenth day. The child had been put to the breast. At this time symptoms of peritonitis and fever set in. She rallied under nourishing diet and quinine, but relapsed seventeen days later. She ultimately recovered completely. Dr. Glisczynski advocates the use of ice-compresses as preventive of peritonitis.

13. Professor Grenser reports that during 1859 there were 550 deliveries in the Lying-in Institution of Dresden. There were born 558 children; of these, 531 presented by head, 5 by face, 8 by back, 5 by feet, 4 transversely, and 5 presentations were unascertained. Of the operations performed, 16 were forceps-cases, and 4 were cases of turning; 25 children were dead-born, and 28 died shortly after birth; 7 mothers, or more than one per cent., died of puerperal fever. No one of these fatal cases seems to have been connected with operative proceedings.

It deserves to be remarked that 29 puerperæ, including 17 primiparæ, had suffered during childhood from rickets of variable intensity and duration. In 9 only did contraction of the pelvis exercise any marked influence over the labour. Three were delivered by forceps. In 16 cases an uninterrupted recovery took place; the remainder suffered from light forms of perimetritis, endometritis, endocolpitis, and in one instance erysipelas.

14. Dr. Kristeller has submitted to the Berlin Obstetrical Society a contrivance by means of which the exact degree of extractile force employed in using the forceps may be measured. He observes, that the obstetrist, in seeking to describe the extent of the obstacle to delivery, is compelled to draw upon his imagination, and to use vague terms, as "easy, feeble, moderate, difficult, &c." For these expressions his instrument enables us to substitute the figures of a scale. His instrument is somewhat complicated, and not easy to describe without the help of drawings. The dynamometrical apparatus is adapted to the handles. Each handle consists of two parts, one moveable, the other fixed. The fixed part is a strong steel plate, which forms the continuation of the fenestra. The moveable part is a half-cylinder of brass, which is so adapted by its plane surface to the steel-plate that it can ride freely up and down, but in no other direction. Above, the brass half-cylinder is closed by a projection forming a notch in which the fingers of the operator are hooked for power of traction; below, the cylinder ends in the ordinary dilatation for the hand to rest upon. Within the half-cylinder lies a strong steel spiral spring, which presses above against the prominences which support the operator's fingers, and to which the chief part of the extractile force is applied, and below is fixed immovably to a projection from the steel plate. When the operator pulls with his right hand upon the upper prominence, and with his left upon the shafts of the handles, he draws the moveable half-cylinders down, compressing the spiral springs, the elasticity of which serves to measure the force employed. This is indicated by a graduated index adapted to the handle below the lock.

Dr. Kristeller enters with some minuteness into the applications and uses of this instrument. He especially insists upon the advantage it offers as a measure of the hindrance to delivery, enabling us to determine the time when the forceps must be abandoned for the cephalotryptor.

15. Professor Martin has related to the Berlin Obstetrical Society a successful case of transfusion. A primipara aged twenty suffered a fright in her eighth month; symptoms of inflammation of the uterus followed; pains set in with inconsiderable external bleeding, but marked anæmia, with prostration. The os uteri being rigid and undilated, plugging was resorted to. Next morning, a pointing swelling appeared in the scrobiculum, the pulse could hardly be felt, syncope was frequent, the temperature falling, and death seemed imminent.

The median vein of the right arm was exposed by an incision of the skin four or five inches long, a flat trocar was used to perforate the vein, and six to seven ounces of freshly-drawn blood was injected through a warmed glass syringe. The patient complained of no pain, but immediately showed a blush on the cheeks. The plug now removed, the os was found dilated; the fœtus (dead) was extracted by forceps. The uterus being compressed, the placenta was expelled with more than two pounds of black clot. The maternal surface of the placenta showed a compressed part occupying two-thirds, around which the cotyledons rose in a border like a wall. There was some after-hæmorrhage, which was stilled by injecting a solution of sesquichloride of iron. Such a degree of anæmia remained, that a second transfusion was resorted to. About three ounces of fresh blood was thrown into the right basilic vein.

The patient gradually rallied. She had æthereal spirit of chlorated iron and laudanum alternately, and warm milk. She ultimately recovered.

16. Dr. Credé devotes an elaborate paper to prove that the uterus should be made to expel the after-birth by exciting it to contract by means of frictions and compression with the hand. He believes this method is original.

17. Dr. Spöndli examines critically and illustrates by cases the advantages of perforation and cephalotripsy in labour obstructed by disproportion. He disapproves of the cranioclast of Professor Simpson, and concludes that the joint use of perforation and cephalotripsy offers the best method of treatment.

III. The Puerperal State.

On a Phlegmonous Puerperal Erysipelas observed in the Winter 1859–60. By Professor M. Retzius, of Stockholm. (Mon. f. Geb., March, 1861.)

Professor M. Retzius describes an epidemic of puerperal erysipelas which raged in the Lying-in Hospital of Stockholm. The new lying-in hospital was opened in May, 1858. Six months had not passed when several cases of puerperal fever appeared. In the beginning of 1859 the cases were more frequent, increasing until they amounted to 40 per cent. of the lying-in women, with a mortality of 16 per cent. In the summer the health of the place improved, only 3 per cent. of the patients being seized. With the cold months, the sickness again rose to 37 per cent. In the beginning of 1860 the weather was very mild. As many patients were admitted, the crowding was such that no proper ventilation of rooms or bedding could be made. Soon erysipelatous inflammation appeared. In March, several cases of erysipelas phlegmonodes of the upper and lower extremities occurred. The symptoms were as follows: At first, a strong shivering; the ensuing fever showed no disposition to sweating; the patients complained of strong pains in the whole body; the abdomen was little painful, and not swollen; prostration; pulse soft and quick; over the entire surface of the body, sensibility was so exaggerated that the lightest movement caused pain, and the weight of the bed-clothes could not be borne; the arms and legs could not be moved without the greatest distress; the tongue, at first coated, was soon red, dry, and glazed; thirst great; a few hours after the shivering, circumscribed deep-red hard swellings appeared on the extremities, and simultaneously diarrhœa set in. When the phlegmonous swellings had lasted ten or twelve hours, the red colour grew quite dark, and mortification of the skin set in. The affected limbs grew cold, doughy, and insensible; the pains ceased; the pulse grew weaker every moment, and some hours before death could not be felt. The patients sank under sopor. Throughout the disease the lochial secretion was stinking, and so irritating, that the mucous membrane of the vagina was excoriated, without, however, being sphacelated. In a few there was milk in the breasts. Down to the end of March the cases only occurred in the lower story, and in the wards devoted to the instruction of midwives. No puerpera who lay in a room alone with a space of 2000 cubic feet was seized. In the common wards, designed for only three patients, with a space of 1500 cubic feet, on account of the pressure it was necessary to place four persons, so that the space was reduced to 866 cubic feet. Professor Retzius was compelled to close these, and to find other rooms for the instruction of midwives. In these latter no case of serious disease arose. In April, after a short respite, the disease appeared in the department devoted to the instruction of medical men. By rigid seclusion the spread of the epidemic was arrested. During the stay of the children in the institution, no case of roseola happened amongst them. It was, however, made known that later several cases of malignant erysipelas occurred in children, after their reception in the Foundling, whose mothers had died of the disease.

Dissection of the puerperæ, commonly made twenty-four hours after death, showed small quantities of grey-yellow sero-purulent fluid in the peritoneum only twice. The uterus was large and flaccid, its inner surface being covered with a thin layer of purulent stinking fluid, intermixed with coagula; the heart was flaccid and pale; endocardium, purple-red; right ventricle contained a larger, the left a smaller coagulum; strong blood-hypostasis, with œdema of the lungs; liver anæmic, contracted, and soft; spleen larger and softer than natural; kidneys soft and pale, with violet-coloured pyramids. On incision into the diseased extremities, much reddish serum ran from the infiltrated cellular tissue. The muscles, even near the joints, were throughout pulpy, without fatty metamorphosis. In the bloodvessels was no clot. The veins showed no inflammation-changes, and no pus was found in their channels or around them. Only in the spermatic vein were purulent collections discovered.

[The history of this epidemic is an instructive illustration of the manner in which a puerperal epidemic may be produced by crowding lying-in women in a badly constructed hospital. If hospitals be a necessity for lying-in women abroad, could not our brethren exert their influence to substitute *cottage-hospitals*, ensuring the isolation of the patients, for the existing pest-houses? —REP.]

MEDICAL INTELLIGENCE.

Statistical Inquiries as to the Causes of Cretinism.

DR. GUGGENBÜHL, of the Abendberg, in a communication to the Academy of Sciences of Paris (see Comptes Rendus, 1860), has invited that body to interest itself in furthering the accumulation of European statistics upon cretinism, in the same manner that the Academies of Sciences at Vienna and Petersburg have done. These scientific bodies, as it appears, have already adopted a series of questions drawn up by Dr. Guggenbühl, as a uniform basis in requesting at the hands of their respective Governments statistical materials for the use of a commission on the matter in question. Dr. Guggenbühl, in connexion with the questions, which we cannot here quote for want of space, suggests that eudiometric observations should be largely established, repeating his opinion that the predisposing cause of cretinism is a specific malaria.

University of Cambridge.—Degree of Master in Surgery.

The power of granting the Degree of Master in Surgery, in accordance with the Medical Act, has recently been conferred on the University of Cambridge by a statute approved by the Queen in Council; and the Board of Medical Studies in that University is at the present time engaged in drawing up regulations with reference to it. The student is required by the statute to reside nine terms—that is, the major part of each of three years—in the University: this time will be devoted, partly to general study, partly to medical study; and then he may continue his professional studies in Cambridge or elsewhere. He must either have taken a degree in Arts, or have passed certain of the examinations necessary for a degree in Arts, before he can be admitted to the degree in Surgery.

The regulations will be such as to ensure an extended course of professional study; and it is proposed that there shall be two examinations, one in the middle of the period of medical study, and the other at its completion. The details, however, have not yet been published.

The number of medical students in Cambridge has lately been on the increase; and as a legal qualification to practise in surgery as well as in medicine can now be obtained at the University, it is probable that still more will be disposed to avail themselves of the advantages offered. Dr. Humphry and Mr. Lestourgeon have commenced summer courses of lectures on surgery and midwifery, which are intended for students preparing for the Degree of Master in Surgery.

With the view of enabling students who are destined for the medical profession to come to the University at a somewhat earlier period than has been usual, Dr. Humphry, one of the surgeons to Addenbrooke's Hospital, has obtained the recognition of his house as a University Hostel, so that pupils residing with him, and pursuing their medical studies under his superintendence during the period which is usually spent in the house of a medical man, may matriculate in the University, keep terms, pursue their general education to such an extent as seems desirable, obtain degrees, and enjoy all the privileges of University students, just as if they were admitted at one of the colleges.

Respecting the desired connexion between our ancient universities and medical students, we would remind our readers of some observations which were made in a previous number of the Review (April, 1858). The remarks alluded to were *apropos* of a letter to the Provost of Oriel by Mr. Pearson on a scheme for rendering Oxford more accessible to medical students generally.

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1861.

PART FIRST.

Analytical and Critical Reviews.

REVIEW I.

1. *Den Syphilitiske Hjernelidelse. Afhandling for den Medicinske Doktorgrad.* Af VALDEMAR STEENBERG, Tredie Læge ved Sindssyge-anstalten ved Slesvig.—*Kjöbenhavn,* 1860. 8vo, pp. 269.
On the Syphilitic Brain Disease. A Thesis for the Degree of Doctor of Medicine. By VALDEMAR STEENBERG, Third Physician to the Institution for the Insane at Schleswig.—*Copenhagen,* 1860.

2. *Maladies Syphilitiques du Système Nerveux.* Par GUSTAVE LAGNEAU, Fils, Docteur en Médecine, Membre de la Société de Médecine du Département de la Seine, de la Société des Sciences Médicales, Membre Associé de la Société d'Anthropologie, &c.—*Paris,* 1860. 8vo, pp. 531.
Syphilitic Diseases of the Nervous System. By GUSTAVE LAGNEAU, Jun., M.D., &c.

3. *Des Affections Nerveuses Syphilitiques.* Par le Docteur LÉON GROS, Ancien Interne des Hôpitaux de Strasbourg, &c. &c.; et E. LANCEREAUX, Interne Lauréat des Hôpitaux de Paris. Ouvrage couronné par l'Académie Impériale de Médecine (Prix Civrieux, 1859).—*Paris,* 1861. 8vo, pp. 486.
On Syphilitic Nervous Affections. By Dr. LÉON GROS and M. E. LANCEREAUX.

THE almost simultaneous appearance in France and Denmark of three important works upon a class of diseases to which, although they have long been to a certain extent recognised, comparatively little attention has until recently been directed, is in itself a proof that these affections are at length receiving the consideration which their importance demands.

In the writings of Nicol. Massa, who lived about the time of the celebrated epidemic which, at the close of the fifteenth century, ravaged Europe, occurs, observe MM. Léon Gros and Lancereaux, a notice of syphilitic neuralgia, the most ancient with which we are acquainted. In the following century appears Paracelsus, the first to speak of the venereal miasm:

"He admits that once introduced into the system, this venereal miasm combines with all other diseases; that it modifies them, gives them new forms, and, pushing this doctrine to its utmost limits, he affirms that the virus may produce the most varied affections—phthisis, diarrhœa, dropsy, the exanthemata, &c. In a word, he attributes to the venereal taint the property of profoundly modifying the system, and of manifesting its action by often very remote general symptoms." (p. 2.)

On the other hand,

"Fallopius denied that syphilis was a general disease. According to this writer, the disease, local in its commencement, always continues local, and the symptoms which have their seat at a distance from the original seat of the affection do not belong to syphilis. The non-virulists of twenty years ago could not have said more!"

Thierry de Hery (1634) dwelt a little more than his predecessors on syphilitic manifestations connected with the nervous system. He proves from facts that syphilis may be complicated with a train of nervous accidents, such as spasm, epilepsy, &c., and he goes so far as to assert that it may become the source of all diseases.

"Gervais Ucay, who wrote at the close of the seventeenth century, attributes great importance to badly treated syphilis; he says, in fact, it is thence that so many hereditary diseases proceed, scrofulous tumours, old ulcers, attacks of gout and rheumatism.

"Musitano (1771) quotes a large number of diseases which syphilis may produce, and among them he includes asthma, phthisis, dysentery, marasmus; he says, in fine, that there is no disease nor extraordinary symptom which may not flow from this poisoned source." (p. 3.)

Astruc considers syphilis to be capable of producing all diseases—in particular, weight in the head, furuncle, hemicrania, vertigo, convulsions, epilepsy, paralysis, trembling of muscles, hydrocephalus, want of sleep, asthma, orthopnœa, cough, palpitations of the heart, syncope, faintness, inequality and intermission of the pulse, hypochondriasis, emaciation, atrophy, &c.

Sauvages also admits that a number of diseases, and even of febrile diseases, may be produced by the syphilitic virus.

Sanchez (1777) distinguishes from acute syphilis—the only form, he says, described by his predecessors—a variety which he terms chronic, and which manifests itself by a temperament under the influence of which the life of the sufferers is tormented by a number of affections. His theory would seem to have much analogy to that of the masked syphilis of the present day. Among the serious phenomena enumerated as being capable of being produced by syphilis is mania.

"Van Swieten, the great commentator of Boerhaave, the ardent advocate of the treatment of syphilis by corrosive sublimate, asserts that no organ can escape the incessant aggression of the venereal miasm; he has traced it even to the brain. The source of gummy tumours, exostoses, deep-seated pains, the syphilitic virus may, according to him, produce the most serious accidents, apoplexy, epilepsy, blindness, deafness, paralysis, &c." (p. 4.)

"Carrère (1783) treated more particularly of the degenerated forms of syphilis.* According to this author, the degenerated virus may produce phthisis, rheumatism, ophthalmia, cachexy, asthma, dropsy, dysuria, paralysis, epilepsy, apoplexy; moreover, it may be complicated with all other viruses. Carrère divides these diseases into *masked* chronic venereal diseases, depending on the venereal virus *latent* but remaining venereal; and into *degenerated* chronic venereal diseases, kept up by a venereal taint *modified* or *complicated* by another virus." (p. 5.)

Benjamin Bell saw confirmed syphilis produce blindness, amaurosis, deafness, phthisis, asthma, rheumatism, epilepsy, and even mania.

"We may remark in passing," continue MM. Gros and Lancereaux, "that Benjamin Bell is perhaps the first syphilographer who supports his opinions with clinical facts of any value, while the majority of preceding authors either do not enforce their doctrines by the aid of any clinical proof, or they bring forward observations so vague, so devoid of the most essential details, that we cannot, in the present state of science, draw from them any important deduction." (p. 6.)

John Hunter, in his great work on the venereal disease, having, in assigning to the venereal poison, if not as its exclusive seat, at least as its seat of election, certain tissues and organs, thrown doubts on the reality of the remaining manifestations admitted in such great number in his day, was followed by others who absolutely denied the specific nature of the majority of the accidents hitherto attributed to the syphilitic virus. It is sufficient to mention the names of Richond des Brus, Desruelles, Jourdan, and all those whom M. de Castelnau designates by the title of *non-virulists,* to show how far a principle pushed to extremity may lead.

"To sum up," observe MM. Gros and Lancereaux, "between the opinion of the writers of the sixteenth, seventeenth, and eighteenth centuries, who almost all saw in syphilis a true morbid Proteus, capable of assuming the most varied pathological forms, and in the majority of acquired or hereditary chronic affections discovered so many metamorphoses of the venereal disease; who believed that this malady might remain during a very long period latent in the economy, to break forth subsequently under one of the thousand varied forms of the nosology, and who, in consequence of this belief, laid down as a principle the necessity of submitting all patients affected with syphilis, whatever might be the degree or date of their disease, to a full mercurial course; between this opinion, we repeat, and that of the present day, which restricts the venereal disease to a certain number of organs and tissues, on which side lies the truth? No doubt there has been much confusion and exaggeration among the advocates of the first opinion, but resting upon numerous facts, which for the most part we believe to be conclusive, we affirm that in what relates specially to the influence of syphilis upon the nervous system, we must overstep the narrow circle within which the majority of modern syphilographers have endeavoured to confine the history of the disease." (p. 10.)

It is, however, more than possible that much of the diversity of opinion which has from time to time existed on this subject may be due to variations in the type of the disease. That such is the opinion

* Recherches sur les maladies vénériennes chroniques sans signes évidents, c'est-à-dire, masquées, dégénérées ou compliquées.

of the writer whose work we have placed first at the head of this article, is evident from the following passage, though, according to him, it is of late years that a preponderance of syphilitic neuroses has been observed. But the truth probably is, that the syphilitic poison is subject to the law of cycles so evident not only in other diseases, but in all sublunary phenomena.

"The rise and course of syphilis have not at all times been the same; thus, while attention was first directed to this disease, it set in with a violence and a malignity, and ran an acute course, such as have never since been observed; it assumed, in the lapse of years, a gradually milder character. Now, again, it seems most recently to have once more altered its form. Thus, its ancient regularity with respect to the several series of symptoms no longer prevails; on the contrary, we often see so-called tertiary symptoms set in almost immediately after the primary affection, and on the other hand, we frequently observe a secondary lesion suddenly to develop itself in a patient who has long presented tertiary forms. Mercury and iodine have not now, as formerly, the absolute power of overcoming every existing syphilitic phenomenon, for we not unfrequently meet with patients whose symptoms appear to be wholly unaffected by these our most powerful anti-syphilitic remedies. Lastly, syphilis has altered its character also with reference to the organs it attacks; while, on the one hand, it has become so much milder, that it now seldom causes the devastating affections of the bones, which not many years ago were so general, it has, on the other, become far more injurious, inasmuch as it seems much more frequently to attack the internal organs, and especially man's noblest part, the brain." (p. 5.)

Such is the most important portion of the "Confession of Faith" with which Dr. Valdemar Steenberg prefaces his highly practical volume. We shall endeavour to bring before our readers the leading points connected with the pathological anatomy, the symptomatology, the etiology, the diagnosis, the prognosis, and the treatment of the formidable lesion he describes.

The morbid primitive form, which presents itself, more or less distinctly marked, at every post-mortem examination of cases of the disease in question, is sanguineous congestion of a limited portion of the brain. But if the patient dies immediately or in a comparatively short time after having for the first time presented apoplectic symptoms, we find either nothing—as occurs in many other congestions of both external and internal organs—or distinct hyperæmia, or lastly, capillary apoplexy with point-like extravasations of blood in the substance of the brain. If, as is most frequently the case, the cerebral affection runs a more chronic course, we meet with other also multiform phenomena, which, however, all likewise depend upon the sanguineous congestion. This congestion, with the capillary apoplexy depending on it, will frequently recur during the chronic course of the disease, manifesting itself at each period of recurrence by distinct exacerbations, and will, as is usually the case in all congestions, especially affect the part already predisposed to it. Softening of this part thus ensues, caused either solely by the changes in the brain in the seat of the congestion, invisible to the eye, but sufficiently established by their effects (paralysis, death), or by the influence which the extravasated and subsequently metamorphosed blood may have on the surrounding cerebral mass; or, finally, by the obliteration of the ruptured capillaries, and the consequent obstruction to the free afflux of blood for the nutrition of this part of the brain.

The extent of the softening is very various; sometimes not larger than a pea, it may occasionally occupy even an entire hemisphere. It is seldom accurately defined. Occasionally the affected part will increase in size, so that, if it be superficially situated, it will be found more prominent than the corresponding portion of the other hemisphere; most frequently, however, it will be recognised by a collapse and diminution, as if the result of incipient atrophy.

In one patient who had suffered much from syphilis, and had died with apoplectic symptoms, the author, on dissection, found a large coagulum in the basilar artery, together with corresponding thickening of the coats of the vessel, without any other abnormity in the brain. His attention was thus directed to the condition of the vessels both in the brain and in the rest of the body, and from that time he very frequently, though by no means invariably, found a considerable alteration of the vessels, co-existent with the proper cerebral affection, particularly atheromatous deposition, at one time in the larger, at another in the smaller ramifications. He is hence inclined to believe that a great proportion of the syphilitic affections of the brain are secondary to a primary lesion of the arteries, consequently that it is not in the brain the syphilis is localized, but in its arteries, and that the affection of the brain is merely the result of the obstruction to the supply of blood, that is, of impeded nutrition; and he compares cerebral softening the result of syphilis, with the uncomplicated softening in the aged, adding that there is no disease which so rapidly converts the young and strong individual into an old man as the syphilitic dyscrasia. That hereditary syphilis gives the infant the look of an old man, is well known; but a similar change occasionally takes place in a remarkable manner in adults.

In none has the author found atheromatous degeneration so considerable as in syphilitic patients, and he adds the remark, that after the splenic artery, it is the vessels of the brain which are specially predisposed to such depositions. He has, in fact, sometimes found the cerebral vessels affected, while all the others were healthy. It is, moreover, well known that the organs of circulation are frequently the seat of syphilitic localizations; thus, we are acquainted with syphilitic pericarditis and endocarditis, and with a simple and gummy interstitial syphilitic myocarditis.*

Several authors describe such vascular lesions in patients who have died of syphilitic diseases of the brain; thus Gildemeester and Hoyack speak of them.† Virchow frequently mentions them,‡ and even says§ that syphilitic paralyses may arise not merely from direct affections of the nerves, but also mediately from interruption of the circulation of the blood.

Hence the author deduces the following hypothesis:

* Virchow's Archiv, Band xv. Heft 3, p. 288. † Nederl. Weekblad, Jan. 1854, p. 23.
‡ Virchow's Archiv, Band xv. Heft 3, p. 324. § Loc. cit. p. 291.

"The syphilitic dyscrasia has a tendency to induce an atheromatous lesion of the coats of the arteries, and the vessels of the brain are especially disposed thereto. If it be the minuter ramifications which are the seat of this lesion, a moment at last occurs when the passage of the blood becomes impossible, partly in consequence of thickening of the coats of the vessels, partly of the coagula of the blood deposited in the affected parts. By reason of this stoppage of the blood congestion ensues, with rupture of several small vessels already rendered brittle by the atheroma, producing capillary apoplexy. If it be the larger branches which have become atheromatous, a similar pathological process will be initiated, but it will not attain its full development, because a large portion of the brain will be deprived of its normal nutrition, and death will first ensue." (p. 16.)

But it is not Dr. Steenberg's opinion that this lesion of the vessels is the sole, or perhaps even the most frequent, cause of the cerebral anomalies which have been alluded to. He admits that in some cases where the vessels have been found affected the lesion may have been secondary, depending on another condition; for example, pressure on the vessels, in consequence of exudation in the meninges; and this is the view entertained by Gildemeester and Hoyack.

But if we reflect on the above-mentioned pathological changes, and especially on the most common, and certainly the most essential, softening, we shall soon be convinced that none of them have any decided syphilitic stamp; that if we met with such a patient first on the dissecting table, we could not positively say whether his cerebral affection was of syphilitic origin or not; on the contrary, precisely similar lesions are often found in persons who have never been tainted with the syphilitic poison. We shall therefore proceed to examine another affection which is scarcely ever met with except in syphilitic individuals, namely, the so-called gummata or gummy tumours.

By these we understand, as is well known, small, solid, pale tumours, generally of the size of a pea, which may be found in the most various parts of the body, particularly in the skin and subcutaneous connective-tissue, where they are usually larger, and may even attain the size of a nut. They are also frequently found under the mucous membrane, and it is their ulceration which produces the frequently considerable destruction in the throat. Some of the affections of the bones are due to these; they are met with in the muscular connective-tissue, in the liver, the testicles, the lungs; and Lebert asserts that the pulmonary abscesses which occur in syphilitic infants are precisely these gummata which have passed into suppuration; lastly, they have frequently been met with in the brain, and have been described by Bonet, by Ricord (who calls them syphilitic tubercles of the brain), by Cullerier, Lallemand, &c. According to Lebert,* they consist of a rather solid elastic tissue, in which are found, in a finely granular intercellular substance, a number of roundish bodies, neither distinctly cells nor cell-nuclei, resembling tuberculous corpuscles without being identical with them, their similarity depending on this, that the intercellular substance rapidly becomes tough and dense, so that the cellular elements can no longer be fully developed. They may pass into sup-

* Handbuch der praktischen Medicin, p. 371.

puration, and we then find, instead of the bodies just described, a granular detritus with a number of distinct pus-corpuscles, and both destroyed and uninjured connective-tissue.

The sinuses of the dura mater and the other vessels of the meninges are frequently quite empty and collapsed; sometimes they are distended with blood; the Pacchionian granulations are frequently highly developed.

The pathological phenomena which the author has most frequently observed in the bones in connexion with syphilitic disease of the brain, have been the characteristic syphilitic necroses or cicatrices. These usually occupy the forehead or the anterior part of the vertex; once only did Dr. Steenberg see the cribriform plate of the ethmoïd bone destroyed. In general they were not deep, but had merely removed larger or smaller portions of the outer lamina of the bone. In those fortunately rare cases where parts of the skull occupying its entire thickness had been destroyed, the functions of the brain were normally discharged. The cicatrices after these lesions of the bone all presented the characteristic marks assigned by Virchow—viz., a want of productivity in the centre and excessive productivity at the circumference.

The author appends a table, which we copy, of the changes in the brain and its investing parts, observed in 37 post-mortem examinations, of which 29 are from the General Hospital at Copenhagen, while 8 are borrowed from foreign journals. It is to be observed that most of the patients presented several abnormities, and that the disease was in general not confined to a single anatomical part of the brain. No abnormity was found in 6 cases;* softening of a portion of the brain existed in 26.

The seat of the softening was: in the left hemisphere, seven times; in the right hemisphere, seven times; twice in the cerebellum; ten times in the left corpus striatum; four times in the right; in the left thalamus, twice; in the left side of the corpus callosum and fornix, once; in the right lenticular ganglion and ciliary plexus, once; in both crura cerebri, five times; in the right crus, once; in the pons Varolii, once. Gummy tumours were met with five times. Atheromata in the arteries of the brain† six times; thromboses of the same, three times; meningeal apoplexy occurred five times; adhesion of the membranes of the brain, four times; osteophytes on the inner surface of the cranium, in three cases; syphilitic ulcers (or cicatrices) of the cranium, in five cases.

The condition of the spinal cord was examined only in 13 cases; in 3 there was well-marked softening; the softening usually occupied so much as would correspond to two or three dorsal vertebræ. One of these patients had never presented proper cerebral symptoms, but after having been for several years syphilitic, suffered from epilepsy, from which he was, however, free during the last year of his life; on

* One of these patients died of small-pox six months after having suffered from apoplectic symptoms, from which he had partly recovered.

† Sometimes in other arteries, without those of the brain being affected.

the other hand, he laboured under incomplete paralysis of motion and sensation in the lower extremities and in the abdomen, very troublesome dyspnœa, and cough without expectoration, and without the lungs or heart presenting any abnormity; moreover, he suffered from considerable debility, had a cachectic appearance, and exhibited a number of tertiary affections. His brain was quite healthy, but the spinal cord was softened at about the second and third dorsal vertebræ. In the second patient, who had likewise suffered to a great degree from epileptiform attacks, it was the cervical portion of the cord which was softened, and the brain was at the same time attacked. The third patient, in whom the softening existed likewise in the cervical portion, had laboured under considerable necrosis of the cervical vertebræ, and in her, too, the brain also was involved.

The lungs were in general in their normal condition; only three times did the author find them tuberculous. Pneumonia is, however, rather frequent among these patients, and not a few of them die of that disease.

In 4 cases the heart was in a state of fatty degeneration, with atrophied walls; twice the valves were atheromatous.

In 3 cases the thyroïd gland was hypertrophied.

But the disease which most frequently occurs in old syphilitic patients, and which seems to stand in the closest causal connexion with syphilis, is Bright's dysæmia; in 7 cases the kidneys, and in 5 the spleen, presented distinct Bright's degeneration.

In not a few cases were the kidneys unusually rich in small cysts. The suprarenal capsules were always normal.

An organ which also suffers very frequently in syphilitic dyscrasia is the liver: Dittrich* and Gubler† were the first to give an accurate description of this condition, and to point out its causal connexion with syphilis. According to Virchow‡ we have both a syphilitic perihepatitis and a simple and gummy interstitial hepatitis. Perihepatitis certainly never occurs alone, but is in general partial, surrounding the parts which are the seat of the interstitial hepatitis. The author adds an account of this affection, borrowed from Bamberger,§ referring for a more detailed description to Virchow's paper, in his 'Archiv,' just quoted. Syphilitic disease of the liver usually belongs to the later symptoms; the author, however, quotes an example of its early occurrence, which he remembers to have seen in the clinique of Councillor-of-State Fenger.

The pancreas has almost always been in its normal state; only in 2 patients, who suffered in a high degree from Bright's disease, was it very large, hard, and fragile.

Neither did the intestinal canal present any abnormity dependent on syphilis, though it may of course suffer at the same time from other accidental acute or chronic affections; thus, the author saw it once exhibit marks of catarrhal inflammation in a patient dead of variola;

* Prager Vierteljahrsschrift, Bände vi. und vii.

† Mémoire de la Société de Biologie, tome iv. 1852. ‡ Loc. cit. p. 267.

§ Virchow's Pathologie, Band vi. 1, p. 560.

in 3 cases tuberculous ulcers were present, in 1 there was a cancerous tumour around the pylorus.

"Only in one single patient did syphilitic orchitis seem formerly to have existed, in other respects the testicles in all were perfectly healthy; with this Professor Hassing's experience completely agrees, that in Denmark syphilis very rarely attacks the testicles; if I do not mistake, in four years that he has presided over the syphilitic department of the General Hospital, only two persons have laboured under such an affection, and yet during that period about 1500 syphilitic patients were admitted. It is well known how frequent such affections are in other countries—for instance, in France; this is one of many examples of the fact, that syphilis occurs variously modified in different countries." (p. 34.)

In two cases the ovaries contained numerous cysts. Cicatrices of chancres were frequently observed upon the genitals.

The glands in the groins, elbow-joints, neck, &c., were often swollen and hard.

Considered in reference to its *symptomatology*, syphilitic brain disease presents itself under two forms; the first, the centripetal paralysis, being characterized by the gradual advance of peripheric nervous symptoms. In addition, we have signs which point more distinctly to the brain as the seat of the disease, for example, vertigo, intolerance of disturbance or of noise, impairment of memory. The paralysis extends to the muscles of the bladder and rectum, producing involuntary evacuations. But while unilateral paralysis of the face is almost constant in the second form, we scarcely ever observe it in this more chronic variety. On the other hand, the muscles which govern the motions of the eye, and those which co-operate in the articulation of words, are frequently implicated in the first form. Thus paralysis of one of the muscles of the eye, particularly of the external rectus, is often an early and even long a solitary symptom, soon perceived by the patient himself, on account of the strabismus and double vision it produces. A change of voice, more gradual in the first, more rapid and complete in the second form, is a very frequent attendant on both varieties. Connected with speech we have three modifications of lesion: 1. Weakness of the muscles, producing a thick and stammering articulation. 2. Disinclination to give answers, which are as short as possible, and resemble those of a drunken man. 3. The strange substitution not merely of whole words, but even of complete sentences, for what the patient intends to say. Another symptom is what Landry has called "paralysis of the feeling of muscular activity," or the morbid condition in which a patient can regularly perform every movement so long as he can follow it with the eye, but the moment he closes his eyes, or it becomes dark, even if this take place when he has half completed so simple a movement as bending the arm, he cannot continue it. Duchenne, who, the author believes, was the first to direct attention to this point, has termed the function which is here lost, "muscular consciousness." Professor Schroeder van der Kolk has shown "that throughout the body, the sensitive branches of a mixed nerve run to the part of the skin which is moved by the muscles receiving motor filaments from the same nerve trunk," or, in other words, that "a spinal nerve gives its motor branches to the muscles as instruments of motion, and its sensitive branches to the part moved."* Paralysis of these sensitive filaments, the motor branches remaining intact, would evidently produce the lesion in question.

The author alludes to cases where mistakes similar to those of speech above mentioned are made in writing. There is something very singular in the condition of the brain under such circumstances; thus we have ourselves seen a gentleman labouring under paralysis, capable of writing correctly, but unable to read in consequence of miscalling the letters.

The second, or apoplectic, is the principal form of this disease; it is of much more frequent occurrence, is much more destructive to the patient's bodily and psychical welfare, and is also that which constitutes the termination of the first described variety.

These syphilitic affections of the brain may manifest themselves, whether only a few months or many years have elapsed since the individual was first tainted with the poison, whether the previous symptoms were violent or so weak that they were scarcely observed, whether they advanced in an uninterrupted series from organ to organ, until at length the brain became the seat of the disease, or whether years have passed away, during which the poison has lain in a deceptive slumber.

The author concludes his chapter on the symptomatology of the disease with the following supplemental table of the relative frequency of the several forms of paralysis: In 26 patients, the limbs of the right side were paralysed; in 21, those of the left; in 1, the right arm and left leg were paralysed; in 2, the left side of the face and the right extremities; in 4, the paralysis shifted from one limb to another; in 3, the left arm alone was paralysed; in 3, the right leg alone; in 1, the left leg alone; in 20, both lower extremities were paralysed; in 34, the evacuation of urine was abnormal; in 18, the fæces were passed involuntarily.

As to the *etiology* of the disease—is the name which is given to it correct? Some deny that the cerebral disease in question has any connexion with syphilis, and attribute its development to the use of mercury. Others admit its dependence on syphilis, but only so far as to recognise in syphilitic patients a tendency to cerebral affections, while they deny our right to denominate such a form of disease "the syphilitic brain disease," inasmuch as it differs in no respect from cerebral affections depending on any other cause. The most convincing proof lies, however, in the result of treatment: "In the only cases of recovery," observes the author, "which I have myself witnessed, or have found on record, success has been due especially to anti-syphilitic treatment;" and a little lower down he adds, "there is scarcely any other chronic disease against which the physician can proceed so actively and with such good effect as against syphilis. Our power over chronic diseases of the brain, how great is it?" As to the first objection, that the disease is due not to syphilis but to mercury, the author shows that of

* On the Minute Structure and Functions of the Spinal Cord. London. New Sydenham Society. 1859, p. 6, and p. 7 note.

89 patients treated in the General Hospital, 48 used mercury; respecting 16 the journals give no information in this respect; and of 25, it is ascertained that they never used mercury, and were never exposed to the action of that metal before they presented distinct signs of an affection of the nervous system.

"If we now consider that, with scarcely an exception, all the physicians in Denmark treat the secondary cases which occur in their practice with mercury, that the knowledge of syphilitic symptoms and their danger is generally diffused throughout the middle classes in Copenhagen, to which most of the patients belonged; that these patients could with ease, and for the most part without payment, procure admission to hospital, it is most probable that the number of syphilitic patients who have been treated in Denmark with mercury, so far exceeds that of those who have not taken the drug, that the figures I have given do not merely show satisfactorily that the cerebral affection was not due to mercury—for this a single case where mercury had not been employed would be sufficient—but also that it is much more probable that the syphilitic patient who has been treated with mercury shall remain free from this cerebral affection, than he with whom this medicine has not been employed."

Hence Dr. Steenberg infers that "syphilis is the principal cause of this cerebral affection; and that if a patient has never been syphilitic, he will never be attacked by such a disease."

In this proposition the author goes, we think, a little too far; while we freely admit that the numerous cases he has so ably detailed, and of which he has given at the end of his volume so very clear and useful a summary, are quite sufficient to prove that a large number, perhaps the great majority of such cases, are connected with, as their principal cause, the poison of syphilis, latent or otherwise, in the system, we see nothing in their origin, symptoms, course, or pathological results, sufficiently characteristic to distinguish them from cases which not unfrequently occur where there can be no suspicion of the existence of such a taint. But in forming our opinion as to the presence or absence of the syphilitic poison, we must not forget how "very rarely it is expelled from the system it has once affected;" and that Ricord has shown "that syphilis may be slumbering in the constitution even for forty years without exhibiting the least trace of its presence, and may then suddenly break out in all its power."

In 89 cases investigated by Dr. Steenberg, 17 patients had suffered from primary, secondary, and tertiary syphilis; 48 from primary and secondary, not from tertiary; 6 from primary and tertiary, not from secondary; 3 from primary, not from secondary or tertiary; 14 from tertiary, their having laboured under primary and secondary symptoms being unknown. In one patient Dr. Steenberg was unable to prove that any syphilitic symptom had pre-existed, but both her previous mode of life, the course of the disease, and the pathological changes found upon dissection in the brain, appeared to justify her being included in this category.

Since the existence of the primary and secondary symptoms there had elapsed in one patient thirty-five years; in 6 patients, between thirty and twenty years; in 11, between nineteen and ten years; in 22, between nine and two years; in 16, "several" years; in 10, one year or some months; in 8, the secondary symptoms were still present.

Of the 14 patients who denied having had primary or secondary syphilis, 8 could give no information at all as to the time when the then present tertiary symptoms had appeared; 1 had had them for eleven years; 4 for about three years; 1 only for some months.

In the cases borrowed from foreign journals, primary, secondary, and tertiary syphilis had existed in 6 patients; primary and secondary, not tertiary, in 11; primary and tertiary, not secondary, in 3; tertiary, not primary or secondary, in 2; congenital syphilis in 2; in 1 case the history was quite defective.

Since the existence of primary and secondary syphilis there had elapsed: in 6 patients, from twelve to six years; in 5, from five to two; in 5, one year or some months; in 7, no time was stated.

In the two cases where only tertiary symptoms are mentioned, cicatrices alone after these remained, likewise without any time being given.

The author gives a list of the special constitutional symptoms which either coexisted with or preceded the cerebral affection; the conclusion he draws from a consideration of this catalogue is, that nearly all the forms of syphilitic disease may be followed by nervous affections.

Syphilis is the essential cause of this cerebral disease, but as every one who has once been syphilitic does not necessarily become paralysed in mind and body, Dr. Steenberg proceeds in the next place to inquire on what this difference depends? This important question must, for the present, be left in part unanswered. The author, however, points out that it does not depend on difference in the virus, for he has known several married couples where the husband has contaminated the wife, or *vice versâ*, so that both were infected with the same kind of pus, and yet the brain of one has become diseased, while the other has escaped:

"The patient, however, is never at a loss upon this subject, the malignity of his syphilis is due to the negligence or ignorance of his former medical attendant; the latter recognised the character of his disease either too early or too late; he gave him either too much or too little mercury, or, at least, he did not keep him long enough in bed, or he allowed him to eat something which was not fit for him; in a word, the patient is so willing to ascribe all the blame to his physician, that it is only surprising that he does not also give him the credit of having got him his primary chancre."

Seventy-three patients were of the male and 41 of the female sex; but the preponderance of males is simply the result of the fact that they are in general more exposed to the contagion of syphilis than females are.

With respect to age, 1 patient was sixty-nine; 17 were between fifty-nine and fifty; 25 between forty-nine and forty; 39 between thirty-nine and thirty; 21 between twenty-nine and twenty; 2 were nineteen; 1 was two; and 1 was one year old. In 7 cases the patients' ages were not stated.

Age would, therefore, seem to have *per se* no influence upon the disease; the foregoing would show only that the latter occurs some years after the individual has been contaminated; it is between twenty and forty years of age that people generally expose themselves to contagion, and accordingly we see that somewhat later the disease is at its maximum.

Neither does the patient's occupation seem to exercise any decided causal influence. From an enumeration made by the author, it would appear that all the classes which usually resort to hospitals are represented in about the same proportion; prostitutes alone predominate, which is a natural result of the fact that almost all of that class at one time or another become syphilitic.

In reference to *diagnosis,* Dr. Steenberg observes:

"When a patient, who has not yet passed the age of virility, who does not present any evidence of any affection of the heart or arteries, who has not suffered from any considerable injury of the head, or from a disease capable of developing embolism or thrombosis of one of the cerebral vessels, is suddenly attacked with an apoplectiform seizure, his malady may undoubtedly be diagnosed as a syphilitic encephalopathy, if the symptoms of syphilis co-exist or have recently gone before; nay, even if many years have elapsed since the syphilitic dyscrasia last manifested itself."

In like manner, if the affection does not assume the apoplectic form, but occurs, under similar circumstances, more insidiously as a centripetal neurosis of a particular part, there can be no doubt that the symptoms are due to a syphilitic lesion of the nervous system. The author adduces examples to show at what extremes of youth and old age syphilis may be acquired, a part of his subject into which it is unnecessary for us at present to enter.

The cerebral disease which occurs most frequently, and at the same time presents the greatest similarity to the syphilitic encephalopathy, is true apoplexy; the signs of congestion of the head are, however, usually more prominent in the latter. If the patient does not die immediately after the attack, which fatal result very seldom happens in the syphilitic seizure, the absence of symptoms due to the pressure of extravasated blood, particularly of long-continued and well marked loss of consciousness and general paralysis, will enable us to decide with tolerable accuracy that no effusion of blood has taken place in the brain.

The syphilitic brain disease is, or at least ends by becoming an encephalomalacia; the distinction between it and the non-syphilitic disease must be etiologically deduced.

The author enters at some length into the consideration of the diagnosis between this disease and saturnine paralysis and saturnine brain disease, and also between it and general paralysis.

The *prognosis* of the disease is in general unfavourable. The length of time after which a relapse may occur, creates a difficulty in arriving at any very accurate conclusions on this point. Dr. Steenberg observes:

"I have reckoned all those patients as cured, whose symptoms had completely ceased at the time of their discharge, and of whom I subsequently lost sight; and all those improved, in whom the results of the attack—for example, diminished power of movement in one of the extremities—were not, indeed, entirely removed, but did not present any indication that the morbid process was progressive. It will be seen that in the cases borrowed from abroad, the prognosis is much more favourable than in my own; this is due especially to the fact, that I have taken most of these cases from French authors, and of these it is generally true that they have the opportunity of observing a patient during the course only of a part of the disease, and subsequently lose sight of him, and besides, Frenchmen are particularly disposed to regard the results of their treatment with the most sanguine eye; if the patient does not die in their hands, they write with great readiness, 'Cured,' upon his card of dismissal. In the cases published by German, and partly also by English writers, I have in general found the report either accompanied by the account of the post-mortem examination, or terminating with the only too often true remark, that the treatment was unavailing. Perhaps the prognosis is really worse in proportion to the coldness of the patient's residence, as we know that syphilis is much more obstinate with us than in the southern countries of Europe." (p. 221.)

Dr. Steenberg gives the following as the results of treatment at the General Hospital: "Cured, 7 males, 6 females; improved, 9 males, 6 females; uncured, 20 males, 10 females; died, 16 males, 15 females." Of those treated abroad there appear: "Cured, 10 males, 2 females; improved, 2 males; uncured, 4 males; died, 5 males, 2 females."

Among the patients who died in hospital, the periods which elapsed between the commencement of the disease and its fatal termination were as follows:

"In 2 cases, from two to three days; in 2, from sixteen to twenty days; in 7, from one to three months; in 7, from four to eleven months; in 8, from one to four years; in 1, over six years. In 2 instances no information could be obtained as to the commencement of the disease. The average duration of the malady would therefore appear to be about one year."

As to treatment: "Causâ sublatâ, tollitur effectus;" syphilis is the cause of the disease, could we therefore completely remove the former, or repel it to organs where it might lie concealed without causing direct injury, we should also be able to remove the cerebral affection. But unfortunately, as syphilis has in other respects in the course of time assumed another character, it now frequently exhibits a much greater power of resistance against the remedies which formerly acted powerfully and rapidly, and this is especially true of the syphilitic disease at present under consideration.

But if a patient comes under treatment while the cerebral affection is still in its earliest stage, the employment of anti-syphilitic remedies will not only be fully justified, but we shall most frequently obtain a satisfactory result from their use; still we must not forget that the course of the disease is intermitting, at first even perfectly intermitting, and we must not ascribe exclusively to the remedies employed the improvement which is perhaps only the consequence of the nature of the disease. Whether we shall employ mercury or iodine must depend partly upon the character of the other coexistent or preceding syphilitic symptoms, partly on the earlier treatment, mercury being indicated if this remedy has not before been employed, or if it has not been given in sufficient quantity, or for a sufficient time; in the opposite case iodine is applicable, and its use may, as is well known, be repeated as often as may be necessary.

It is almost a matter of indifference which of the various preparations of mercury we adopt; but the author believes experience to be in favour of the rule to treat the earlier symptoms with calomel, and the later, particularly if calomel has been before employed, with sublimate. The ordinary mode of exhibiting calomel at the General Hospital is to give a pill of one grain morning and evening until the symptoms disappear. Dr. Steenberg quotes the employment of mercurial inunctions to the head after the removal of the epidermis by a blister, by Dr. Read of Belfast,* as having been attended with considerable success, inasmuch as of three patients with well-marked cerebral symptoms, two were completely cured, and the third was much benefited.

The author alludes to a contra-indication to the use of iodine:

"In cases where a remission of the nervous symptoms occurs simultaneously with the formation of a tertiary sore, the use of iodine ought not to be continued until the ulcer is completely healed, because I believe that I have seen instances of such an ulcer having acted as a natural issue in removing the tendency to the localization of the dyscrasia from the brain."

The filthy and immoral process of syphilization "has also been tried in this disease, but without any particular success. Gjör, however, quotes a case of recovery after syphilization had been continued for eight months."†

When the disease is further advanced, and no other syphilitic symptoms are present, the specific treatment will be no longer indicated. Our object now should be to strengthen the system, and to raise the sunken nervous energy. Nourishing diet, open air, the avoidance of every violent mental or bodily exertion, or psychical emotion, together with quina or iron, are best adapted to fulfil the first indication; while for the second we should have recourse to those means which act more immediately upon the nerves—viz., strychnia, valerian, arnica, issues, blisters, moxas, electricity, baths (cold and warm, Russian, with or without the douche), frictions with counter-irritants, either along the spine or on the paralysed parts. Aperients will, of course, often be required. Opium, camphor, musk, &c., will, it is needless to say, also be occasionally indicated.

The extent to which we have drawn from Dr. Steenberg's volume, and the regularity with which we have abstracted something from every chapter, will be the best proof of our opinion of its value. We have only to add that his work abounds in evidences of the practical good sense and straightforward truthfulness so characteristic of our brethren in Sweden, Norway, and Denmark.

The work of Dr. Lagneau, Fils, is not, like that of his Danish *confrère*, confined to lesions of the brain. It embraces a much wider field, including the syphilitic pathology of the entire nervous system, with its anatomy, symptomatology, diagnosis, prognosis, and treatment.

* Dublin Quarterly Journal of Medical Science, vol. xiii. p. 53.

† Norsk Magazin for Lægevidenskaben, Bind xi.

Before speaking of the encephalon, the lesions of the cranium are considered under the heads of suppurative osteitis of the vault of the skull, both external and internal, suppurative osteitis of the face and of the base of the skull, and syphilitic cranial tumours or exostoses.

Internal suppurative syphilitic osteitis is, according to M. Bedel, of rarer occurrence than the external variety, by reason of the preference of caries for affecting superficial bones and those in which the spongy tissue predominates. The author, however, suggests that the lesion may commence in the internal table of the skull more frequently than the data furnished exclusively by necroscopic examination would lead us to suppose, as the latter most frequently takes place at a period of the disease too advanced to enable us to determine what part of the bone was first attacked, for often both tables are then altered, and perforations more or less extensive have occurred. Occasionally there has been found on the internal surface of the bone "a flabby, grey, very fœtid, non-diffluent substance, resembling a thick pap, which it is impossible to remove completely with the forceps and spatula." In other instances, concrete membraniform matters have been met with, which might either be the results of a purulent effusion, or might seem destined to protect the encephalon, and by their ulterior cartilaginous transformation to supply the place of the destroyed portion of bone. Suppurative osteitis of the internal surface of the cranium appears rather to determine the molecular destruction of the bones—that is to say, caries—than to produce fragmentary mortification of the same, or necrosis.

The author next considers the pathological anatomy of the lesions of the meninges and encephalon, under the heads of alterations of the texture of the meninges, tumours of the meninges and of the encephalon, vegetations, and alterations of the structure of the encephalon.

In treating of the symptomatology of encephalic syphilis, Dr. Lagneau first speaks of the symptoms presented by the cerebral nerves; headache is of very frequent occurrence, and in degree is usually intense. Its peculiar character is to increase towards evening, to become very violent during the night, and to diminish in the morning, continuing slighter all day. The nocturnal exacerbation is in itself sufficient to attract the attention of the practitioner to the probability of a syphilitic complication. In some exceptional cases the headache seems to have predominated during the day.

Independently of want of sleep caused by osteocopic, rheumatic, or other pains, by pruritus, &c., there occasionally exists, in cases of long-standing syphilis, a species of insomnia comparable to that which, according to Georget, manifests itself at the commencement of all cerebral irritations, sometimes announcing their approach long before. Sigmund has of late years studied the insomnia of chronic syphilis, and more recently M. Bouchut has described, under the name of *nevrosisme*, a particular nervous state which, supervening upon various general morbid conditions, recals to mind the hyper-excitability described by Benjamin Bell.

M. Yoaren sums up the characters of syphilitic headache as consisting in,—1, its violence; 2, a more or less prolonged duration; 3, the nocturnal recurrence or exacerbation.

The general nervous symptoms more especially proper to encephalic syphilis consist in alterations of intelligence, of sensibility, and of motility.

Mental Alienation.—Of 450 cases, Esquirol refers 9 to syphilis. Frequently the intellectual faculties, instead of being more or less perverted, are rather abolished.

The alterations to which the general sensibility is liable, under the influence of syphilis, are of two kinds, producing either its exaltation or its abolition. The author is not aware that exaggeration of the general sensibility, determined by encephalic syphilis, has been observed; but the simultaneous existence of pains in regions paralysed in consequence of a syphilitic affection of the nervous centres has frequently been witnessed. Pain is thus sometimes manifested, while the general sensibility is notably diminished. Bayle's patient, for example, experienced pains and numbness in the legs, the sensibility of which was greatly lessened. Cases of this kind, as M. Landry has recently shown,

> "Find their explanation in the fact that the pain due to an organic lesion is referred by the patient to the peripheric parts to which the nervous filaments are distributed, instead of being felt in the seat of the organic lesion, which may at the same time interrupt the continuity of the nerves, and fully account for the peripheric anæsthesia."

The alterations of motility are likewise of two kinds, according as this faculty is abolished or perverted. To the former class belong the various forms of paralysis of movement, to the latter the several varieties of convulsions.

Differential Diagnosis of Encephalic Syphilis and other Analogous Affections.—The elements of this diagnosis are usually derived from the local or general nervous or other symptoms, and from the patient's antecedents. The efficacy of a treatment regarded as specific has also been held by many to be a proof of the syphilitic nature of the disease they may have been called upon to treat. Among the local symptoms which, by the fact of their coexistence with nervous accidents suffice to reveal the syphilitic nature of the encephalic affection, are caries, necrosis of the cranium or face, often inducing the destruction of the os frontis, of the bones of the nose, of the ethmoïd, &c.; profound organic lesions, which at the same time that they manifest themselves externally with characters decidedly syphilitic, act within the skull or a part of the encephalon. To these lesions we must add the several tumours developed on the external surface of the cranium, such as gummy tumours, periostoses, exostoses, &c.

The paralytic form of encephalic syphilis is frequently of a progressive type, which MM. Sandras and Lucas Championnière regard as characteristic; the convulsive or epileptic form usually appears at a period subsequent to puberty, contrary to what is ordinarily observed in non-syphilitic cases of epilepsy. Besides the diagnostic importance connected with the late appearance of the malady, Cullerier is of opinion that in an individual who has previously had syphilis, when no vivid impression or moral affection has preceded the first attack, we should be justified in adopting an anti-venereal line of treatment.

Syphilitic chorea, too, has appeared at a more advanced age than that at which ordinary chorea generally manifests itself.

The *prognosis* in encephalic syphilis is usually very unfavourable, for if anti-syphilitic treatment be not employed, or if it be adopted too late, death is frequently the result. Of 147 cases enumerated by the author, 57, or nearly two-fifths, proved fatal.

"It is well to remark that the nervous accidents observed in the course of this affection, although involving a prognosis as formidable as those not attributable to the same cause, if the progress of the disease be not opposed, may nevertheless be considered by the practitioner as less serious; for under the influence of timely anti-syphilitic treatment, they are more easily curable than the latter, against which too frequently the most varied, most energetic, and best-directed medication is often powerless." (p. 173.)

The *treatment* adopted in syphilitic affections of the nervous system is most frequently exclusively medical; occasionally, however, these diseases require the intervention of surgical aid.

The medical treatment consists chiefly in the administration of mercury, iodine, &c. Surgically, the trepan has been employed, but the author properly observes, that

"Before deciding on having recourse to this means, we should remember that by the efforts of nature, sometimes spontaneously, often under the influence of medical anti-syphilitic treatment, considerable osseous fragments are exfoliated, as Biett records of the entire frontal bone; and that M. Petrequin mentions a similar curious example relating, if not to the entire thickness of this bone, at least to the whole of its external table; we must also bear in mind that in like manner large portions of bone are insensibly eliminated through the destructive effects of a vast caries, a remarkable instance of which is presented in the case of the woman treated by MM. Cazenave and Dufour."

Surgical intervention is required also for the extraction of more or less considerable, more or less moveable fragments, the exfoliation of which is tardy. Such interference is, however, justifiable only when the gravity of the symptoms renders it impossible to await the spontaneous exfoliation of the necrosed bones.

The author, in concluding his observations on the pathological anatomy of the optic nerves, remarks that

"If we may say with Marjolin, 'that it does not appear to be demonstrated that the venereal virus is capable of acting directly upon the nervous system of the eye, so as to extinguish the sensibility of this organ;' which is not surprising, for no microscopical investigation has been made, so far as I am aware, to throw light upon this point of textural pathological anatomy; we may also see from the foregoing that syphilitic organic lesions capable of directly or indirectly affecting the vision are numerous and varied. I may add, moreover, that the recent work of Von Graefe on syphilitic affections of the eyes reveals various lesions involving either the optic nerve itself, or the constituent parts of the eye. Besides choroiditis, of which M. Laroyenne has recently reported a case, the Berlin ophthalmologist describes a diffuse exudation in the retina, an atrophy of the optic nerve and of its papilla, with diminution of the calibre of the central vessels; circumscribed abscesses or partial softenings developed in the central course of the optic nerve, &c." (p. 223.)

In their intracranial course, the *auditory* nerves may be affected by all the tumours, inflammations, and effusions which may occur at the base of the skull.

Occasionally, caries, instead of determining a free communication with the exterior, confines itself to opening the cavity of the tympanum, and so establishing a communication, through the medium of the Eustachian tube, between the mastoïd cells and the pharynx, recognisable during life by means of injections. Sometimes osteitis of the temporal bone appears to induce its hypertrophy. "Paralysis of the auditory nerve," remarks M. Bedel, "often commences with osteitis or caries of the mastoïd process, extending, by proximity, to the petrous bone. M. Ménière quotes several cases of the kind, some of which were remarkable for considerable thickening of the perichondrium of the ear. In some cases, too, the temporal bone of one side appeared thickened and hypertrophied." M. Courty also mentions a man affected with caries of the petrous bone, with deafness, due to a syphilitic cause.

Obturation of the Eustachian tube is, however, according to the author, the lesion which appears to be the most common cause of syphilitic deafness.

The syphilitic affections of the cranial nerves of the eighth pair furnish but few facts worthy of notice.

"Occasionally an alteration of taste coincides with profound syphilitic ulcerations of the pharynx and mouth, and with considerable submaxillary glandular congestions, as if the gustatory nerves were indirectly affected by the extension of the inflammation accompanying the ulcerations, or by the pressure of the inflamed glands. Cullerier's patient presented this coincidence of an alteration of taste and of serious lesions in the bones and in the soft parts of the buccal, pharyngeal, and nasal cavities. It is evident that organic lesions analogous to those observed by M. Davasse in the patient Pierre Verdier, may also produce serious alterations of the general or gustatory sensibility of the tongue; for in this patient the tonsil, the anterior palatine arch, the insertions of the levatores palati at the base of the skull, the corresponding portion of the styloïd muscles, a portion of the petro-pharyngean aponeurosis, and of the right lateral wall of the pharynx, no longer existed.

"In some cases the abolition of taste, as in the epileptic and amaurotic patient of M. Rul-Ogez, and in the hemiplegic patient of M. Briquet, seems to depend upon an intracranial lesion. The same is occasionally true of the general sensibility of the tongue; the hemiplegic woman under the care of Lallemand and M. Verdier experienced tingling and numbness in one half of the tongue, as well as in several other regions." (p. 319.)

Probably under the influence of the divisions of the eighth pair, spasms have been observed in syphilitic patients, exhibiting themselves in the pharynx, in the larynx, or in the muscles of the back of the neck, resisting the most varied treatment, and yielding only to medicines regarded as anti-syphilitic. Some instances of this kind appear to be merely the first epileptic manifestation of encephalic syphilis, analogous to those described by Marshall Hall under the denominations of laryngismus, odaxesmus, sphagiasmus.

"Fits of suffocation may also be the result of syphilitic lesions of the trachea, whether ulcerative or cicatricial. M. Moissenet recently read before the Société de Médecine des Hôpitaux a very curious case of syphilitic cicatricial constriction of the inferior part of the trachea and of the left bronchus, producing almost every night fits of suffocation closely resembling those of stridulous laryngitis." (p. 321.)

The remaining sections of Dr. Lagneau's work are devoted to the consideration of syphilitic affections of the hypoglossal nerves, of the spinal nerves of the neck, of the upper extremities and trunk, of neuralgic affections of the genitals, and of syphilitic affections of the sciatic and splanchnic nerves.

We commenced this article with a brief abstract of the very interesting history of the literature of syphilitic neuroses given by MM. Léon Gros and Lancereaux in their opening chapter; we cannot better close our notice of this class of diseases, or more profitably sum up the whole subject, than by reproducing some of the "conclusions" with which they terminate their volume.

"Nervous affections," observe these writers, "may be developed at any period of constitutional syphilis.

"These affections bear sometimes separately, sometimes simultaneously, upon the three great functions of the nervous system: sensation, motion, and intelligence." [Might they not have added a fourth—nutrition?]

"Very varied in their symptomatic forms, they may simulate the majority of the neuroses and affections symptomatic of an alteration of the nervous centres and cords.

"Syphilitic nervous affections are *direct* or *indirect*.

"*Direct nervous affections* may exist without organic lesion, appreciable to our methods of investigation. Most frequently, however, they depend on a material lesion of the nervous system, the latter being, equally with the other organic systems, liable to attacks of the syphilitic virus.

"The *indirect nervous affections* are symptomatic of syphilitic lesions situated in the organs or tissues adjoining the nervous system.

"Syphilitic nervous affections without appreciable material lesion are divisible into—neuroses of sensation, of movement, and of intelligence.

"Neuroses of sensation include *rheumatalgia, headache, neuralgia, anæsthesia,* and *paralysis of the organs of sense.*

"Neuroses of movement comprise: *general convulsions* capable of simulating epilepsy and eclampsia; *partial convulsions* capable of simulating hemichorea, &c. They comprise, also, *general* or *partial paralyses.*

"The neuroses of the intellect are allied to hypomania or monomania (*syphilophobia*).

"*Rheumatalgia* often marks the commencement of constitutional syphilis; it may occupy all the regions of the body, especially the head, the neighbourhood of the joints, and the muscular masses.

"It is less constantly nocturnal than *osteocopic pains.*

"These latter are most frequently located in the head; they constitute a tertiary phenomenon *par excellence.*

"The *insomnia,* frequent in children affected with hereditary syphilis, may depend upon the existence of this kind of pains.

"The most ordinary varieties of *syphilitic neuralgia* are: trifacial neuralgia, gastralgia, and sciatica. They most commonly manifest themselves in the course and towards the decline of the secondary period. When they supervene at a later period, they are in general referrible to an organic lesion.

"Anæsthesia does not appear to exist as a sole nervous manifestation due to the syphilitic diathesis.

"Paralysis of the organs of sense very rarely exists without appreciable lesion.

"The general convulsions which simulate epilepsy recur in fits, preceded by vertigo and accompanied by loss of consciousness. They are distinguished from epilepsy: 1. In that the attacks are preceded by headache during a longer or shorter period. 2. By the appearance of the first attack in adult age. 3. By the absence of the ordinary causes of epilepsy. 4. By the prompt and permanent cure effected by well directed specific treatment.

"The nervous affections without appreciable lesion may supervene at any period of syphilis.

"The material alterations, depending on the action of the syphilitic virus on the nervous system, consist sometimes in a simple disturbance of the circulation (congestion, anæmia); sometimes in inflammation or softening of the nervous tissue (meningitis, encephalitis, ramollissement, retinitis); sometimes in the formation, in the substance of this tissue, of plastic deposits, which compress it and disturb its functions (indurations, *gommes*, exudations on the membranes of the eye).

"The nervous centres, and preferentially the more vascular portions of these organs, are the more special seat of inflammation and softening.

"The syphilitic nervous affections depending on a deposit of plastic matter disseminated or agglomerated in the nervous substance, in general appear very slowly, five or six, and even twenty years after the primary accident. They belong to the quarternary period of syphilis, or visceral syphilis.

"The nervous affections symptomatic of syphilitic alterations of the neighbouring tissues appear sometimes in the secondary, but most frequently in the tertiary period.

"Syphilitic nervous affections are not unusual phenomena, metamorphoses of syphilis. They belong to this disease in the same manner as all other manifestations usually recognised as syphilitic.

"The accessory causes which appear to favour the localization of syphilis in the direction of the nervous system, are all those which act in over-exciting or in depressing the nervous force (nervous temperament, excesses of all kinds, mental fatigue, moral causes, &c.).

"Syphilitic nervous affections manifest themselves by symptoms almost identical with those of the majority of nervous affections. This absence of all pathognomonic character seems to be one of the causes why they have hitherto been unrecognised and confounded.

"Their diagnosis is, however, almost always possible. It rests—1. On the antecedent or simultaneous existence of one or more accidents belonging to the syphilitic diathesis. 2. On the appearance of the affection at an age different from that at which it is usually developed. 3. On the absence of the ordinary causes of nervous affections. 4. On the absence of any sign indicating that the nervous affection ought to be referred to another cause. 5. On the regularity of the appearance of the nervous affection at a given period of the general disease. 6. On the successive appearance of numerous and various nervous phenomena, giving to the affection, in this respect, certainly, a special character. 7. On the inefficacy of all medicines usually considered beneficial in nervous affections. 8. On the favourable result of specific treatment. 9. On the habitual relapses when the treatment is not long continued. The combination of several of these characters will produce, in the great majority of cases, an almost absolute certainty.

"The prognosis in syphilitic nervous diseases varies according to the general state of the patient, the absence or presence of appreciable material lesions of the nervous system, according to the part of this system which is the seat of the lesion, &c.

"The affections without appreciable lesion usually get well notwithstanding the apparent gravity which the symptoms may present.

"Of all the affections with lesion of the nervous substance, the severest are those dependent on inflammatory action.

"Affections of the exudative type, and those which are symptomatic of lesions of the osseous and fibrous tissues, in themselves less severe, become most frequently fatal in consequence of inflammatory complications.

"Relapses, so frequent in all affections with lesions, present each time a greater severity.

"The treatment of syphilitic nervous affections is that of any other manifestation of the same diathesis.

"Preparations of mercury and iodine should be employed, separately or simultaneously, according to the period of the disease, the severity of the affection, and the general state of the patient.

"Mercury appears to be more specially indicated in the congestive and inflammatory forms, iodide of potassium in the exudative or plastic variety.

"The efficacy of the specific treatment will often be enhanced by the employment of adjuvants fulfilling a special indication. Such are antiphlogistics in case of the presence of acute inflammatory symptoms; tonics, chalybeates, and preparations of sulphur in chloro-anæmia and cachexy; electricity in paralysis with or without atrophy."

In one respect, the three books whose titles stand at the head of this article strongly resemble each other—namely, in the large number of cases on which the statements they contain are based. This is as it should be. A subject so important as that of which they treat, which although by some writers early recognised, has not, during a long series of years, attracted the attention it deserved, and may to a certain extent be considered in the light of a new subject, requires the support of an extensive induction. In these works, therefore, may be found the most reliable as well as the most recent and the fullest information we possess respecting the syphilitic diseases of the nervous system.

Review II.

El Siglo Medico. Periodico de Medicina, Cirugia y Farmacia, Consagrado a los Intereses Morales, cientificos y professionales de las Clases Medicas. Se publia todos los Domingos à Madrid.

The Medical Age. Periodical of Medicine, Surgery, and Pharmacy; Consecrated to the Moral, Scientific, and Professional Interests of the Medical Classes. Published every Sunday in Madrid. 16 pp. large 8vo.

Our last number of the Review contained the concluding part of an interesting article upon the History of Medicine in Spain, in which but slight allusion was made to the present condition of medical practice, or the various modern appliances for teaching the medical art now existing in the Iberian Peninsula. Taking the Spanish medical periodical mentioned above as our guide, and fortified by observations made during two recent visits to Spain, our purpose now is to place our readers in possession of such information as may help them to form a just idea of the advance which medicine and medical education have there made of late, and of the several professional resources now enjoyed by that country, concluding with a few remarks upon the climate of various parts occasionally recommended for invalids.

Speaking generally, it would appear that Spanish medical writers may be at present specifically, yet very correctly classed into four categories—viz., organists, vitalists, chemists, and, last but least, homœopathists. The sect first-named is decidedly the most numerous, arising chiefly from the great influence now exercised by French medical literature upon Spanish practitioners, and the scientific education Government has of late enforced at the several national universities. The vitalists usually reckon among their ranks as proselytes many who consider Hippocrates the great apostle of medicine, and under his auspices enthusiastically promulgate the doctrines which they generally have adopted. On the other hand, those designated the chemical party are, as yet, neither large in number nor influential, and a native authority when alluding to such persons, condemns their productions as being more remarkable for imagination than sound judgment. Besides, having only recently taken up any marked position in the Spanish medical republic, their influence is not extensive. Farther, the chief partisans of this school being young professors full of enthusiasm for scientific progress, but often deficient in that experience which is essentially necessary to enable practitioners to appreciate the practical value of luminous theories, their labours often fascinate the understanding without really conveying useful instruction to such votaries. The last-named class, the homœopathists, constitute as yet a very insignificant body, and comprise only ninety-four practitioners of that class throughout Spain, with one hospital at Madrid; and then it is rare for any fanatical disciple of the German apostle, Hahnemann, to become in practice so bold as to assume any serious responsibility by treating an acute or dangerous malady.

Although Spaniards for several centuries often acted as leaders in the march of civilization, throughout recent years they have lagged very much behind other European nations, not only in reference to arts and sciences, but especially respecting medicine, notwithstanding it was much cultivated during the Moorish domination by authors still considered of eminence. Indeed, it ought ever to be remembered in honour of the many Arabian sovereigns who ruled Spain during upwards of seven centuries, that they always patronized science and learning; while one of their kings—viz., Gehwar, whose capital was Cordoba, not only banished quacks, charlatans, and ignorant empirics then attempting to cure disease, however incompetent, and without being first educated for such duties, but he likewise established, about 1035 A.D., a medical college of wise men in that city, who were empowered to examine and license candidates desirous of practising medicine, or of procuring appointments at hospitals and public establishments.

In fact, this Moorish king founded a College of Physicians, which was one of the oldest institutions of that kind in Europe ever created by royal authority; the first university instituted in Spain dating, however, about a century and a half later, viz. 1199, when Alonzo

VIII. established that of Palencia; Salamanca being founded in 1243 by Alonzo IX., his successor.

But even long prior to the period now mentioned, Cordoba, justly considered the Athens of Spain, had become greatly distinguished above every other Spanish city for the eminent medical practitioners usually resident within its precincts, and to procure whose advice patients frequently flocked thither from distant provinces. Thus Don Sancho el Gordo, king of Navarre, sent his nephew, Don Sancho, who then reigned in Leon, to Cordoba, in order to get cured of the dropsy with which he was afflicted, and where the treatment pursued proved successful. It ought, however, to be added, that this event occurred earlier than the time of Ghewar, being about 930 A.D.—that is, when the luxurious Abderrahman was sovereign, who always retained at his court some Arabian physicians of high repute. But not only to procure good medical advice for the relief of diseases under which they suffered did strangers often frequent Cordoba; students likewise came there from distant lands to obtain practical knowledge in their profession at this Academy, which they considered the most celebrated medical institution throughout Europe.

Among other important advantages which Cordoba possessed, we may mention that, so early as the tenth century, clinical wards for illustrating diseases by cases and their treatment were attached to this school, where appropriate teachers imparted to medical alumni useful information. Besides that feature, there was also a most magnificent library, containing upwards of 250,000 volumes; while numerous valuable works by Cordobese physicians and professors are still extant, which some modern writers might advantageously peruse, not alone with profit to themselves, but with benefit also to their patients.

Although many medical luminaries of this learned locality were Saracens, whose tenets as true Mahomedans made them adverse to touch any dead body, nevertheless among individuals of their persuasion anatomy was much cultivated at Cordoba, but more especially by persons of the Jewish community, who entertained no such objection. Indeed, it may be stated that, at an early period of its fame, several writers of this school described the structure of the lungs, heart, and various other internal organs; and also gave exact details of the osseous system and its appendages. They were likewise cognizant of the arteries, nerves, &c., and knew their functions; while it also was taught by professors that the medulla oblongata constituted a centre from whence all voluntary movements departed, the nerves being simply their conductors. In short, many learned Cordobese physicians of those bygone times appear to have been more accomplished than they often get credit for in modern estimation. As one example of the scientific acquirements of teachers who flourished in those remote periods, it has been asserted—and this in the minds of some with apparent reason—that the circulation of the blood was actually known at Cordoba many centuries before Harvey settled the question. In support of this assertion modern Spanish authorities not only say that the difference betwixt arterial and venous blood was pointed out by prominent Israelitish medical authors of the above period, but they also state that Aberroes, an eminent Cordobese physician, in his work entitled 'El Colliget,' which appeared during the twelfth century, makes the following curious and interesting remark: "Arteriæ quæ portant sanguinem a corde et ramificatæ sunt per totum corpus ad ferendum rem ipsam." Afterwards, Aberroes adds, should this not seem sufficiently convincing, "Manifestum est de anatomia, quæ multæ arteriæ a corde mittuntur ad cerebrum;" the fact being then clearly ascertained that arteries transmitted blood from the heart to the brain, as elsewhere. Whatever modern anatomists and sceptical critics may think of these quotations, they at least seem of sufficient importance to merit special mention when here alluding to the ancient and during many centuries justly celebrated medical and scientific seminary of Cordoba, which, as authorities state, was remarkable even so early as the days of Aristotle for its learned men; while it has been also asserted by Spanish historians—for instance, the erudite Estevan de Garibay—that Aristotle himself was a Cordobese. But be that as it may, there is no doubt two other distinguished philosophers of antiquity—namely, Seneca and Lucan—were both natives of this ancient Moorish metropolis.

Notwithstanding the great reputation as a seat of learning, particularly in reference to medicine, which Cordoba had virtually attained (then popularly called the "Spanish Nurse of Science"), it has now become like a barren waste in so far as regards the promotion of knowledge, seeing that all former glories and great repute are now mere matters of memory. Instead of containing nearly one million inhabitants, the population has dwindled down at present to scarcely an eighteenth of that number. Its valuable library and far-famed medical school exist no longer; while the celebrated manufactories of leather, for which this locality was so well known throughout mediæval Europe, have also disappeared. The only relic existing of ancient glory (although reputed remarkable for its very old still resident nobility, of whom some even date their origin to Gothic families who lived prior to the Saracenic invasion,) being, as we can state from recent personal inspection, the truly magnificent Mosque, since converted into a Christian cathedral, actually built nearly twelve centuries ago by the Moslem sovereigns of that period, and containing many hundred splendid columns. This unique temple yet remains in excellent preservation, and will be ever admired as one of the most remarkable architectural structures throughout Europe. Without specially enumerating every destructive influence which mainly caused the subsequent decadence of the Cordobese Academy, a severe blow was inflicted upon its prosperity when Moors and Jews were ruthlessly expelled the Peninsula subsequent to the conquest of Granada in 1492, which terminated Arabian domination. Many hundred thousands of industrious persons, of whom numbers were likewise the most accomplished portion among its native population, were then banished to other countries for religious reasons. The sadly disastrous persecutions perpetrated against these proscribed races, after being continued by successive governments, to the great detriment of national prosperity, and inimical to all diffusion of knowledge, became further materially augmented, but at a later period, by Pope Benedict, who published a bull ordering that henceforward "no Jew should practise medicine, be an apothecary, or act as notary throughout Spain." From these tyrannous laws being still in force, all Israel's children continue ostracized, while none of that creed are able to exercise any profession, or lawfully reside within the Spanish dominions, even at the present moment.

Although the progress virtually made in medical science south of the Pyrenees has been rather slow during many recent years, particularly while other regions of Europe have effected considerable advances in the right path, Spain has nevertheless more lately shown considerable desire to improve the old routine system she had hitherto long pursued. For example, it is now made imperative that every Spanish medical student who shall be henceforth licensed, either as physician or surgeon, must receive a better and more extended education than heretofore. Among the most important changes in that respect recently enacted by government authority, and especially connected with our profession, those relating to the university curricula of candidates for medical degrees, and some new laws at present regulating the practice of pharmacy, ought to be mentioned.

According to an Act of the Spanish Legislature passed during the reign of her present Majesty, and promulgated subsequently by royal ordonnance, no institution of the realm now grants the degree of doctor in medicine or surgery, unless by the central University of Madrid, which was only instituted about twenty-five years ago, viz., on removing the ancient Alma Mater of Alcala de Henares to the metropolis. In order to obtain either of the above academic titles, students must study during not less than seven years in some Spanish University, one of which, however, it is imperative to have spent in attending medical lectures at the Faculty in Madrid, and that prior to graduation. As none but such graduates in medicine or surgery can now fill the office of physician or surgeon to hospitals, or obtain various other public appointments, it follows that the new central University possesses a real controlling power respecting medical education. In this way, therefore, all underselling of qualifications to practise is prevented; while any Dutch-auction mode of proceeding for obtaining professional distinction by means of degrees so improperly acquired becomes effectually avoided.

Besides the Metropolitan University, there are likewise six others having each a medical faculty attached. These are Barcelona, Granada, Santiago, Sevilla, Valencia, and Valladolid; the formerly far-famed city of Salamanca, which was said at one time to have contained about 17,000 students of all ranks, being now without any superior medical appendage. At the seven Universities above named, seventy-three public professors teach the various branches of medical science; while the average number of students rarely exceeds 1200 at the utmost. In addition to the several faculties of medicine already designated, four Universities—viz., Barcelona, Granada, Madrid, and Santiago—likewise possess schools of pharmacy, comprising eleven professors, and usually about 570 attendants. From these data it appears that the aggregate medical alumni throughout Spain averages under 1800 individuals; whereas the total amount of law students generally ranges more than double the above number, 3742 being actually registered during 1858 as belonging that category. But such result is easily explained, since there are many more offices in Spain open to law graduates than to the medical profession; while the latter body is likewise inferior to the ecclesiastical, seeing the clergy possess, among other attributions, no less than sixty-one bishoprics and ten archbishoprics, of which the See of Toledo, whose incumbent is Cardinal Primate of all the Spains, yields, it is said, about 100,000*l.* of yearly revenue.

As in every other continental country, Spanish physicians and surgeons never dispense medicines or engage in pharmacy. Should, however, any person so licensed assume the latter calling, by existing laws affecting pharmaceans, he would *forfeit* his professional privileges, while those keeping shops for supplying drugs prescribed by the former classes—"no pueden ejercer simultaneamente la medicina ni la cirugia"—cannot exercise simultaneously medicine or surgery. On that point there exists no equivocation. Further, pharmaceans cannot sell a secret remedy, nor any special or specific preparation whose composition is unknown. Should it ever happen that the prescription of an authorized medical practitioner orders strong medicines, especially if poisonous, in extraordinary or unusual quantity, before compounding such doses the pharmacean must first communicate with the prescriber to verify his recipe, lest the party may have made a mistake. Various important regulations that might be judiciously imitated in Great Britain could be here quoted from the amended "Ordenanzas" respecting the practice of pharmacy, which received the royal assent in April of last year, and hence are now in operation; but as doing so on this occasion would be incompatible with the chief task at present undertaken, it will therefore suffice to observe, that among the different reforms recently introduced into Spain, those affecting education and its Universities must prove equally beneficial to the people, as they are really creditable to recent Spanish governments.

Another class of practitioners connected with, or strictly considered very subordinate to the medical profession in Spain, but who nevertheless are still numerous in that country, has likewise been placed under more stringent regulations by Government than heretofore—namely, the fraternity whereof the far-famed Figaro, of operatic celebrity, was a member. Thus, the Minister of Public Instruction issued in June of last year additional new rules, "Sobre el ejercicio de la Barberia," which strictly refer to persons applying plasters, bandages, or any simple local remedy usually included in what is called minor surgery. This recent code also regulates the practice of parties lawfully privileged to bleed their fellow-creatures, to apply leeches or cupping-glasses, to pierce the ears of young ladies or *gentlemen*, to vaccinate, to put topical irritants on the skin, or to employ cauteries. Lastly, the regulations then issued likewise include those practising as dentists or chiropodists. In short, no person is allowed to hold himself out anywhere as qualified for one or other of the above-named manual employments, without having been first examined and licensed accordingly, by a special commission consisting of three professors, two of whom are members of the medical faculty. Prior, however, to being admitted to examination, candidates for such licences must have previously attended during two years at some public hospital having at least sixty beds, in order to obtain some practical knowledge of the particular calling which the party proposes to exercise.

That practitioners of the description just designated constitute a considerable body throughout Spain, becomes manifest by the large sign-boards which travellers frequently recognise over open shops, not only in every town, but even villages, intimating to passengers that the occupant within is a licensed "Sangrador," and so forth. It is likewise worth mentioning, as indicating how common the custom of blood-letting yet continues to prevail among Spaniards, but often without medical advice and sanction, that flaming pictures on wood representing a votary being bled at the arm, from the foot, or in other parts of the body, and containing various paraphernalia, are fixed upon house fronts to attract passing customers, much after the same manner which publicans and gin-palace keepers now adopt in England to enveigle enslaved victims of intoxication. Frequently likewise, a gay parti-coloured barber's pole, with the brass basin which was mistaken by Don Quixote for Mambrino's helmet, decorate the domiciles of these inferior operators, who are nevertheless legally empowered to follow their respective specialities. The numerous places where leeches are sold also strike a stranger's attention, more especially of foreigners, when visiting Spain. Such shops, and those of barbers, therefore unmistakeably prove that the old practice of Dr. Sangrado, as portrayed by the author of 'Gil Blas,' still seems very popular in this country, notwithstanding it is often attended by much mischief among a vegetable-feeding if not half-starved population. In these respects, the still prevalent propensity of employing venesection among such people much resembles the practice also common in Italy, where medical men at present continue to bleed their patients in the cure of diseases which English physicians would assuredly treat very differently.

The existence of barber-surgeons having continued during many ages throughout Spain, it has proved rather difficult to prevent some individuals from acting in both capacities, after they had obtained the proper licence to practice purely as surgeons ; and even although there existed a law of the several colleges of surgeons, issued in 1804, strictly prohibiting any such licentiates from exercising the inferior handicraft of "Barberia." To prevent this unprofessional abuse being continued henceforward (for, as the Council of Health said, in their report recently made to Government, it degraded the whole medical body when any of its members keeps a barber's "tienda"—shop open for custom,) it was enacted by a royal ordinance, having the Queen's sign-manual attached last October, and transmitted by the Minister of the Interior to all provincial governors, that in future should a pure medical practitioner so far demean himself as to follow the above inferior calling or manual occupation, he must not be allowed to fill an official appointment connected with his profession, or be ever employed in governmental service, either by the Acaldes or any other public authority; the reasons assigned for this proceeding of Government being to reduce the evil within the smallest limits possible; and further, ultimately to extinguish it. We are thus particular in now mentioning, although perhaps some may say too minutely, these illustrations of late medical legislation effected by official authority in Spain, with the intention chiefly of demonstrating that, even this country which heretofore has been sometimes accused by foreigners, and apparently with justice, of lagging much behind other continental nations in the modern race towards advanced knowledge and civilization, is really now making marked progress. Indeed, the onward march of improvement which Iberia has lately begun to follow, is alike creditable to the ruling powers, and must eventually prove beneficial to its people generally.

The Madrid University scholastic year usually commences during the first week in October. This ceremony is rather an imposing "Funcion;" and having been witnessed very recently by the present writer, it is thought that perhaps some short account of the manner in which they manage these affairs in Spain may not seem altogether uninteresting to readers, while it will also indicate the present Government's desire to patronize educational institutions, and further show that the well-being of students, whether medical or otherwise, is not overlooked by public authorities. On the occasion mentioned, a large and fashionable auditory was assembled in the magnificent Public Hall of the University, classically designated "El Paraninfo." Numerous ladies were present, who all ranged themselves on one side of the apartment, no gentleman being allowed to approach the benches which these fair visitors occupied. But whether this arrangement was made purposely, lest their attention might be distracted by objects less important than those they doubtless came to witness, it is very difficult to define. However, whatever was the real motive for such unsocial proceedings, in order that none might weary until the chief *dramatis personæ* entered, a military band of music in an adjoining gallery played patriotic tunes, which made any preliminary delay pass pleasantly.

The meeting was presided over by his Excellency the Marques de Corvera, Minister of "Fomento," that is, of education, commerce, and public works; the President of the Council of State, El Señor Martinez de la Rosa; the Marques de la Vega, Governor of Madrid; and the Marques de San Gregorio, Rector of the University, being seated right and left of the noble chairman. Besides these high official personages who individually honoured this ceremony with their presence, the reporter likewise noticed among others the Marquesses de Volgorniana and Molins, Count Cortina, a considerable gathering of professors, members of the Council of Public Iinstruction, as also students, with their numerous friends, both male and female. Altogether the day's proceeding seemed very imposing; while a large assemblage of doctors belonging to the several faculties, arrayed in academic gowns and caps, having the distinctive colours of their respective sections, thereby rendered the effect still more brilliant, especially to the eyes of a foreigner.

Regarding these variously coloured costumes of the different doctors, it may not be out of place here to specify that when a wearer's satin cape and silk bonnet were white he was doctor of divinity; the green colour indicated graduates in common law; crimson, those in civil law; blue, arts and philosophy; whereas yellow characterized doctors of medicine. Occasionally some splendidly caparisoned magnate wore his stylish cape and head gear of two colours, as, for instance, blue and green, thus showing he was doctor both of law and philosophy. Others also sported blue and yellow, thereby signifying that the party in such cases had become doctor as well of philosophy as medicine.

Having taken his seat, the President in due form declared that the University session was now commenced. Afterwards one of the Professors delivered from an elevated pulpit a written but eloquent oration, regarding the influence universities had virtually exerted during particular epochs on the civilization and government of the Spanish Peninsula. Subsequently diplomas were distributed to those students who had obtained such academic honours; silver medals were next presented to various distinguished pupils, while several in gold were also given to alumni of higher merit. These important and varied duties being finished, the brilliant ceremony then terminated, after apparently imparting much satisfaction to many interested spectators.

Of course, in every large Spanish city public hospitals are open for the reception of the sick or those suffering from accidents, whose system of management resembles, generally speaking, that followed in France. At University towns these institutions usually have distinct clinical wards for the instruction of medical students, which form part of such establishments. Among many charitable receptacles of that description, the General Hospital at Sevill—" El Hospital del Sangre" (Hospital of Blood)—merits special notice, being exteriorly one of the finest buildings ornamenting that highly interesting ancient Moorish metropolis. One singular feature of this institution nevertheless deserves notice—viz., its lower tier of windows are provided with temporary shutters, which may be applied when inundations of the locality occur. At those times patients can only occupy the upper floor; while during most hot seasons the lower story is preferred, although such an arrangement must be insalubrious.

The General Hospital of Madrid is larger than any similar establishment in England, and contains a medical, surgical, and midwifery clinical section for the instruction of students in these respective departments. The total number of beds at this institution usually amounts to 1526, although sometimes, when an epidemic prevails in the Spanish capital, even 2000 patients have been accommodated. There are fifteen attending medical officers attached to the several divisions. Being an old building (founded in September, 1594), and much too extensive, as also badly adapted for introducing modern sanitary improvements, the municipality intend to construct two or three smaller hospitals in different urban situations, which would prove more convenient to the public than the present locality, lying near the gate of Atocha. By way of giving some idea to English readers respecting the movement which annually takes place amongst the sick persons treated at all the Madrid civil hospitals, it may be here stated that the total number of patients received during 1860 into these establishments amounted to 12,357, of whom 9021 were medical, and 3336 surgical cases. The cured amounted to 10,584, comprising 7900 medical, and 2684 surgical inmates; while the aggregate deaths reported were 1897, whereof 1701 occurred in the medical wards, and 196 in the surgical department. When these results are calculated separately, it appears that 18·85 per cent. on the admissions, or 1 in $5\frac{1}{3}$ of persons labouring under medical diseases, died; whereas, only 5·87 per cent., or 1 in 17, surgical cases admitted proved fatal; hence, just one-third the mortality noted in the former category.

Adjoining the General Hospital is the College of San Carlo. Here the medical school of Madrid has been established, comprising lecture-rooms, anatomical theatre, library, and all other necessary appendages; the number of pupils in attendance during session amounting generally to about five hundred. Clinical teaching being a prominent feature in the professional education pursued at this establishment, it may be interesting to refer briefly, as an apt illustration, to the system adopted by the Professor of Practical Medicine upon a late occasion, when the narrator of what he then saw happened to be an auditor. The special ward of this physician contained only selected examples of different diseases which could be made instructive to attending students. For each patient two pupils were appointed, whose official duty was, in the first instance, to write a full account of the case under consideration, afterwards to report daily the symptoms, or any variation in their character, and to see that the treatment ordered by the professor was strictly pursued. In short, both were made responsible clinical clerks, acting according to the directions of their superior, who specially examined them in the presence of other students regarding the malady then treated, its character, the effects of remedies employed, and so forth; while he would likewise ask some other pupil about the prognosis and etiology, or state his own opinions thereon. Should it appear that the party so questioned was not able to give satisfactory replies, or expressed erroneous views, and if the superintending pupil had not performed his function to the professor's satisfaction, he was reproved for omissions, at the same time that all present received useful instruction. In this way bystanders benefited by the teacher's remarks respecting the cases which then formed subjects of special observation.

Subsequently, professor and pupils adjourned to an adjacent clinical lecture theatre, where the former made additional practical allusions to the different maladies previously passed under review. Altogether, in reference to clinical instruction, the mode pursued at this institution appeared equal, if not superior to that adopted at various celebrated medical institutions which we have actually visited in different European countries.

One regulation in regard to patients selected for admission into the clinical wards at this school also merits mention—namely, that no sick person can be transferred from the General Hospital wards to any such special department, without first obtaining the party's own free consent, and if a minor, that of the parents or guardian; while in all these instances, permissions so granted must be entered in the former establishment's official register. Besides the several clinical sections now alluded to, although rather succinctly, this public charity has divisions appropriated for syphilitic diseases, affections of the eyes, and a maternity. Therefore students have thus ample opportunities of seeing and studying all the usual forms of maladies to which humanity is liable, without wandering from one institution to another, in order to obtain practical knowledge which shall be useful when subsequently engaged in the active duties of their profession.

Analogous arrangements prevail not only at the General Hospitals of University cities, such as Granada and Barcelona, but likewise even at similar institutions in provincial capitals having no medical faculty, as, for example, at Malaga and Zaragoza. From these statements it may be perceived that the question which has recently occupied philanthropic and professional attention in London—viz., the establishment of special receptacles for treating patients labouring under particular diseases, does not seem as yet much extended to the Peninsula, since its public hospitals indiscriminately receive all sick inmates, whatever may be their complaint, with very few exceptions, unless when an epidemic like yellow fever, plague, or cholera prevails.

Before, however, taking leave of Spanish hospitals, it may be added, as perhaps interesting to readers who may not be acquainted with those charities, that the wards of many were often too crowded by patients, while ventilation was frequently defective. Sometimes four rows of beds, even exceeding one hundred altogether, were contained in one apartment, which had the windows so low down as to range nearly on a level with the floor, besides a high vaulted roof, from whence contaminated effluvia could scarcely escape; so the whole inclosure thus becomes confined, and consequently injurious to health, as patients were thereby constrained to breathe an atmosphere often neither pure nor wholesome. At Madrid, several medical wards appeared unexceptionable; while it ought likewise to be stated that, attached to the institution at Zaragoza, a new airy apartment has been recently constructed in the lunatic section, which might well be adopted as a model for imitation elsewhere.

Connected with the medical administration of the Madrid Central Hospital, considerable discussion has lately taken place respecting gratuitous advice and medicine being given to applicants, it is said, too often indiscriminately. In fact, the same question which recently occupied much attention, and very justly, considering it may be frequently abused, among English practitioners, has likewise formed the subject of anxious inquiry at Madrid. According to regulations enacted by the provincial Junta of Benevolence, and lately adopted by the Central Hospital medical officers, it is arranged that one professor shall attend every afternoon, but two on Wednesdays and Saturdays, in order to give advice gratuitously in their respective departments to poor persons, who may also receive medicines without any payment, leeches excepted. Patients, however, not actually paupers, are obliged *abonarse*, viz., to purchase from the Hospital Director a ticket that costs 20 reals, or 4*s.* 3*d.* sterling, prior to having any verbal consultation with the medical officer in attendance. But if the party desires written directions respecting his malady and its treatment, then 100 reals, or 1*l.* 1*s.* 3*d.*, is the sum charged. The money thus collected in either case is exclusively appropriated for the benefit of poor sick inmates treated at this benevolent establishment.

On the other hand, should any stranger not known to reside in the capital apply for eleemosynary advice, according to these new rules, before being admitted, some written statement of the applicant's case, accompanied by a certificate from an alcalde or a parish priest of the place from whence such patient came, must be produced, so as to avoid deception, which contingency doubtless, both here as well as elsewhere, cannot always be prevented. As this new system was only inaugurated during the early part of last year, sufficient experience has not yet been obtained to decide respecting its actual working, or effects produced upon the public and profession; although observers already assert that individuals the reverse of necessitous, by assuming the guise of outward poverty, have taken advantage of these gratuitous consultations established at the Madrid General Hospital, which thereby, in their opinion, prove detrimental to private practitioners. But this is not surprising, being analogous to the injuries known to follow similar abuses of public benevolence which have been also noticed in other large cities besides the one now brought under observation.

Notwithstanding that strong reproaches may be uttered against Spain, that she is frequently behind other European nations in adopting ameliorations found useful elsewhere, and has likewise shown on various occasions an unwillingness to follow the wake of people more civilized, according to modern opinions, still, although these accusations appear no doubt true in some respects, yet in many instances they are erroneous and unmerited. Without referring to matters not virtually professional, enough has been already stated in previous paragraphs to show that much has been already accomplished towards improving medical education, and also to advance the civil or social status of practitioners throughout her Spanish Majesty's dominions. Besides which, the honours conferred upon distinguished members of the profession by the Crown and successive Governments, especially in modern times, sufficiently demonstrate the high estimation eminent physicians and surgeons have occasionally acquired in that country. In truth, the treatment is often quite different from what almost invariably obtains in Great Britain.

For example, not only have eminent medical practitioners in Spain been decorated by their Sovereign with the insignia of ancient and honourable orders of knighthood, to which statesmen and scions of nobility also belonged, but some physicians have become noblemen by creation, their title being further made hereditary. In addition to such marks of royal favour, it is not uncommon to find individuals, members of the medical profession, elected by their fellow-citizens representatives in the legislature, as also called upon to fill other responsible appointments, which of course raised the parties so chosen, *pro tanto*, higher in public estimation.

Facts being always more convincing arguments than unsupported assertion, we would refer, among other proofs of the statements just enunciated regarding the elevated social position which medical men sometimes attain in Spain, to a book now lying on our table and recently published by Dr. Rubio, one of the Queen's Physicians-in-Ordinary. Besides being Deputy to the Cortes, he is also Grand Cross of the distinguished Spanish Order of Charles III., a Royal Counsellor of Public Instruction, Member of the Madrid Academy of Sciences, and has likewise received other public honours which are much valued by his countrymen and fellow-compeers. Without, however, quoting any other analogous instance, it will suffice as illustrations to mention that some years ago one of the reigning Queen's personal physicians—Dr. Costello—was created "Marques de la Salud," which title is still enjoyed by his descendant; while her Majesty's present chief physician, Dr. Corral, now Rector of the Central University, after having been ennobled by the title of "Marques de San Gregorio," has very lately been named a Grand Cross of the Royal Order of Charles III., which dignity being limited to sixty members of that class, therefore becomes more valued. This order has been instituted about a century, solely as a fraternity indicative of merit, and from comprising chiefly noblemen, either foreign or native, such distinction is consequently more significant. That was so in the case of Dr. Rubio, who, with the medical Marques already designated, constitute two among the threescore Grand Crosses of such brotherhood, whereof another is now the English Field-Marshal Viscount Combermere.

But Spain is not the only Continental country where eminent medical men receive high civil honours from their Sovereign. During the first French empire such cases happened occasionally; and recently an Italian King, besides making several distinguished physicians senators, has also named others to prominent offices in the State, which are commonly filled by nobles or civilians. Respecting royal patronage exercised towards members of the faculty, this fact may likewise be stated as a further instance of its application—viz., Dr. Ekströmer, formerly physician to St. Seraphim's Hospital in Stockholm, was created a Peer by the late King of Sweden, and sits in the House of Nobles of that kingdom. However, although the two distinguished physicians now named are aristocrats, and also hereditary legislators in their respective fatherlands, the public still derive much benefit through the professional labours of both these noblemen, since the Swedish Peer is President of the College of Health and Director-General of Hospitals, while the Spanish "Hidalgo" continues to practise medicine. Indeed, it may be mentioned, that when the latter accompanied the present Queen of Spain during a visit to the Balearic Islands last autumn, her Majesty having been accidentally wounded by a tent-pole falling on her head, the medical Marques not only dressed the part injured, but bled his royal patient, to obviate the accession of any serious consequences.

Before taking leave of Spanish medical institutions, and other professional questions succinctly adverted to in previous discursive pages, some brief remarks regarding lunatic asylums, as also recent legislation towards their improved extension, cannot seem irrelevant to various matters which have been discussed in the present article. Influenced by that idea, we would here add, that among several charitable establishments of the above description, the ancient "Hospital de los Locos," of Granada, at least deserves some notice, more particularly as this receptacle is one of the oldest existing buildings for insane patients throughout Europe, still used for its original purpose, being actually constructed by order of Queen Isabella, now upwards of 350 years ago, or soon after the Moslem expulsion. Considering the period when this asylum was built, it is highly creditable to the royal foundress, and when first opened for insane patients must have been greatly in advance of the prevailing spirit of that age, as also of many popular notions regarding lunatics or their proper management. The number of inmates when we visited this establishment was about 215, more than two-thirds—152—being male residents. While alluding to this benevolent institution of Isabella, it cannot seem out of place specially to notice an edict of her husband, King Ferdinand, which exerted an important influence towards promoting anatomical science and pathology—namely, his Royal Ordonnance, in 1488, permitting the medical officers attached to the hospital of "Santa Maria de Grana," at Zaragoza, to open and anatomize every patient's body dying therein, without incurring any penalty; and also that no person should hinder these examinations, "so pena de mil sueldos"—viz., a fine of 1000 pennies, or about four guineas, by no means an inconsiderable sum in those days. Such conduct on Ferdinand's part deserves high commendation, especially in an age otherwise characterized by gross ignorance.

Other public insane asylums have likewise been long established in many of the large provincial capitals: for instance, at Toledo, Zaragoza, Barcelona, and Valladolid, which last contains usually about 250 patients; besides similar institutions elsewhere. Some of these establishments for the insane are altogether separate from any other charitable institution where patients labouring under physical diseases are treated, although frequently the lunatic wards only form a special department of an ordinary hospital. Speaking generally, if these insane asylums, or rather special sections, be contrasted with analogous receptacles of France or England, in many respects they are much inferior. Nevertheless, remembering how isolated Spain has remained from other European countries during the current century, and also the severe national trials, political and otherwise, which she has undergone, foreigners must not criticize too severely, or express much astonishment should Spaniards appear to have remained lately behind more northern peoples in the treatment of lunacy, and still less forget that they set, more than four centuries ago, a good example for others to follow, who however showed, for a long period of time, little inclination to imitate ancient Spanish physicians.

In proof of that opinion, and as highly creditable to Spain, it ought to be stated, that while most civilized European nations did really little or nothing towards introducing a more humane mode of treating insane patients, an asylum for their accommodation and safe keeping was opened at Valencia in 1409, through the benevolent efforts of Friar Jofre Gilaberto; another at Zaragoza, in 1425; one at Valladolid, in 1436; a fourth the same year, at Sevilla; and a fifth, in 1483, at Toledo, called "El Nuncio," the latter being founded by Friar Ortiz, a canon of the metropolitan cathedral, as also Papal Legate, according to an inscription which was subsequently placed under his portrait, and preserved in the existing, but much more modern building, which had, when we were at Toledo, 84 inmates. The above-named several institutions consequently show that Spaniards of bygone times were far in advance of most adjacent kingdoms, however different the case may be at present.

Indubitably, till within some few years past, not much progress has been actually effected in matters connected with lunatic asylums; but the public authorities of Spain seem now alive to the subject, and admit the great necessity which really exists for making important reforms. After having sent medical commissions to France and other countries, in order to acquire practical information regarding asylums, besides observing modern systems of treatment therein pursued, the Spanish Government resolved very lately to erect six new public institutions for insane patients, according to the most approved principles. None are to contain more than 500 lunatics, both sexes included. Each establishment must have one directing physician, two assistant-physicians, and a pharmacien. By the royal decree dated 28th July, 1859, an asylum was ordered to be commenced in the vicinity of Madrid, which should serve as a model for other provinces to follow; while at Barcelona and Zaragoza plans are at present in course of preparation for the erection of such institutions, at both of which places not less than 30,000*l.* will be expended on each of the new structures.

When the six proposed establishments are completed, they will become great boons to suffering humanity, seeing that the ratio of mental diseases lately ascertained to exist in Spain ranges about one lunatic to every 1667 inhabitants; among whom it may be further mentioned as interesting, that Spanish men much oftener lose their senses than women, contrary to what is observed in England, where females oftener become victims. This greater prevalence of lunacy in male Spaniards is aptly illustrated by a recent report, which says that 6851 ascertained demented patients comprised 4060, or 60 per cent., of the sex just specified. In addition to which peculiarity, it may be also stated as curious, that epilepsy seems more frequently met with in this part of Europe than throughout regions further northward—as, for example, Scotland, Scandinavia, or Northern Germany. In these countries mental alienation is much rifer if contrasted with Spain, where the ratio of epileptic cases ranges comparatively greater. Indeed, it may be fairly enunciated as an axiom, that lunacy prevails oftener in cold countries than epilepsy, whilst the latter augments as an observer proceeds southward; whereas the reverse obtains respecting insanity, which becomes less frequent. Hence Norway and Sweden on one hand, and Spain on the other, occupy in reference to both these maladies quite opposite extremes.

Notwithstanding the mineral springs of Spain are now little spoken of elsewhere throughout Europe, and by no means enjoy the reputation as remedies in disease which many celebrated French and German spas have for a long period attained, still the Spanish peninsula possesses not only numerous valuable medicinal waters, even known to the ancient Romans; but they are exceedingly common, and are of various kinds and chemical composition. Believing some brief remarks on a few of the most frequented watering-places may prove interesting to English readers, we would here mention that, according to recent investigations on this subject, there are at present 705 different localities having salubrious springs, while the aggregate fountains recognised in the above number really amount to 1187 altogether. Such facts at once show the richness of Spain in thermal sources.

But however numerous, these 705 separate spots so supplied are very differently distributed throughout the several Spanish provinces. For example, the greatest number exist in Guipuzcoa, which lies close to the western Pyrenees; and although of very limited extent, this district contains more mineral springs than any other section of the Peninsula, seeing it has fifty-five different places of resort of that description for invalids, enjoying more or less repute. Next in point of number follow Malaga and Ciudad Real, which, on the authority of recent authors, have each thirty-one mineral fountains; while thirty-nine are reported to exist in Granada and its vicinity. At Pontevedra, a town of Gallicia, there are twenty-six; Navarre has twenty-five; Santander, twenty-four; Orense, twenty-three; Leon, twenty-two; Barcelona, also twenty-two; and Zaragoza, twenty-one; all being distinct, and many forming separate establishments, often supplied by several wells in the same locality. These statements, which now only should be considered as specimens and to illustrate the question under discussion, indicate as well the great frequency of medicinal spas in Spain, as likewise that northern provinces adjoining the Pyrenean mountains thus seem more abundantly supplied than some other districts of the country. Indubitably, various central localities are also prolific in similar products. For instance, Guadalajara, in Old

Castile, has twenty-one sources; Salamanca, thirteen; while Madrid and its immediate vicinity is reported to possess about the same number as the place last named. It hence appears that votaries of Hygea need not travel far in almost any province of Spain, without finding some appropriate spot where a temple to the above-named goddess of health might not be justly erected for the use of worshippers.

Among the seven hundred and upwards of different places reported as medicinal springs, of course many are still neither much frequented by invalids nor have acquired any decided reputation in the cure of disease. Nevertheless, numbers are held in high esteem, and at several the concourse of visitors in search of health or for amusement at the prescribed season when fashionable people congregate, is occasionally considerable. Being so numerous, it may be anticipated the mineral springs of Spain are very various in their salutary and medicinal effects upon the human constitution. Speaking generally, most of these wells may be divided into six varieties—viz. : 1. Sulphurous, of which there are both warm and cold examples. 2. Acidulous, comprising three kinds; first, those containing carbonic acid without any admixture of iron, or some with that metal, and lastly, others having sulphuric acid joined either to iron or copper. 3. Ferruginous waters, whereof three species have been enumerated. 4. Saline springs, which are both cold and warm. 5. Alkaline. And 6. Nitrogenous, which are sometimes salines combined with nitrogen, or sulphurous, having such gas also in combination. From these data it may be perceived that numerous Spanish spas are not only often met with, but seem very varied in their chemical composition; more so, perhaps, than has been heretofore generally believed among non-professional English readers, or even recognised by medical authorities of foreign countries.

Here to attempt giving any account beyond an outline of the most frequented medicinal springs which now enjoy public favour among Spaniards in the alleviation of disease, would be impossible within the limits of our present article. We will only therefore allude succinctly to a few Spanish thermal establishments, and principally to those held in highest repute during recent years, or where the largest number of strangers usually assemble. Guided by these "criteria," the baths of Ledesma, situated about twelve miles distant from the city of Salamanca, in a wooded dell on the banks of the river Tormes, first merit attention. The waters of this locality are warm and sulphurous, and date from the earliest period of Roman history, being even celebrated after the second Punic war for their curative virtues, and then named "Bletisa." The Emperor Commodus patronized these springs when Tribune, and in consequence he built costly fabrics for public convenience. Indeed, some remains of the sumptuous buildings he constructed may be still observed. The Ledesma water is used in baths, in vapour, or taken internally. Its temperature on issuing from the fountain ranges usually about 122° F., but it is generally cooled down to 86° when drunk by invalids. The maladies for which this water is found beneficial, are paralysis, rheumatic complaints, skin affections, painters' or metallic colics, indolent ulcers, old wounds, and scrofulous diseases. Towards curing these maladies the baths of Ledesma have become a favourite resort; and more visitors have there assembled of late for the recovery of their impaired health, than at any other analogous public place throughout the Peninsula.

Carratraca, also known to the Romans, is situated about thirty-one miles inland from Velez-Malaga, and ranks next in reference to the number of visitors which frequent this bath during ordinary seasons. The water there supplied, like Ledesma, is sulphurous, but cool, the temperature being seldom, if ever, above 65° F. It is used in baths, douche, as a lotion, or as enemata, and also taken internally. So employed, the Carratraca waters have attained very high reputation, especially in cutaneous affections. They are likewise much employed against neuralgia, chronic rheumatism, scrofulous complaints and nervous attacks, but particularly when occurring in females, or young persons endued with excitable constitutions, and in many cases are reported to prove very efficacious.

Among Spanish saline springs, Alhama, situated in the province of Granada, and about twenty-one miles north of Malaga, is considered to possess some of the most powerful mineral waters of that description throughout the kingdom. The baths so named are exceedingly ancient, being designated by the Romans "Antigi Juliensis;" and it is stated, that even while the Saracens long ruled paramount in this district, they had much repute. The adjacent scenery is beautiful and imposing, and tradition asserts that, after Granada became conquered by the Christians in 1492, whereby Moorish domination entirely ceased, it was in reference to this favourite place of the luxurious Spanish Mahomedans that they uttered their popular plaintive wail commencing with the words "Ay-de-mi Alhama"—Alas! for my Alhama;—to which Lord Byron alludes in one of his poetical effusions. The fountain supplying the medicinal water under consideration is very prolific, and gives out eight thousand five hundred gallons per hour on an average. This remedy is employed as baths and douches, and is also drunk, being chiefly administered in muscular pains, paralysis, scrofulous affections, gastric complaints, abdominal congestions, and nervous maladies. Such was the great reputation of these baths in former centuries that, historians relate, sometimes upwards of ten thousand visitors came to Alhama during one season to try their effects: whereas, the number of persons who have recently frequented this saline spring for similar purposes rarely reaches one-tenth of that amount. Nevertheless its reputation towards relieving the various maladies mentioned above continues to attract invalids. The water of this fountain in temperature ranges 113° F., and the periods for using it extend either from the 1st of April to the middle of June, or during the months of September and October, both these seasons having been so regulated by Government.

Another much frequented locality also deserves notice—namely, the baths of Trillo, both on account of proximity to the Spanish metropolis, as likewise the elevated classes of society who there assemble, rendering it thereby a very fashionable resort for idle company during the prescribed season, which extends from Midsummer-day to the 20th of September. This favourite place is also called the baths of Charles III., in consequence of that sovereign having promoted considerable improvements, its chief fountain being national property; while the establishment is more complete in general organization than any other analogous one throughout Spain. Trillo is situated about sixty miles north-east of Madrid, where the rivers Tagus and Cifuentes join, and on the left bank of the former. These baths occupy a pleasant valley, surrounded by moderately-elevated hills, which has been named, on account of its salubrious influence, "*Valle de los fuentes saludables*," the Vale of wholesome fountains.

In composition, the waters here resemble those of Alhama; that is, they are saline and warm. In temperature, King Charles's fountain is 84° Fah.; although the Princess's bath ranges a little higher. Possessing numerous separate sources, which partially differ in reference to their chemical ingredients and warmth, although not to any great extent, the administration of the Trillo waters may be varied or combined by a judicious physician, according to the constitution of patients, or the particular malady under treatment. Rheumatism and paralysis constitute a large proportion of the diseases which derive most benefit from the baths under consideration. Syphilitic affections, and the results of the abuse of mercury, likewise are remedied, while articular swellings often become materially alleviated, if not cured entirely, by their employment. In scrofulous maladies the Trillo waters are also said to act efficaciously, besides being useful in old ulcers and cutaneous eruptions. The remedy is drunk internally, used as baths, either general or topical, in douches, aspersions, ablutions, and also for gargles, according to circumstances, or the patient's malady.

The acidulous baths of Alhama in Arragon, which also contain steel, the hot wells of Pontevedra in Gallicia, and those of Montbuy near Barcelona, which latter were known to the Romans, and whose temperature is considerable, are sometimes also much frequented. Indeed, the Montbuy Wells have the warmest mineral water in Spain; one, that "del Leon," showing 158° Fah., as it issues from the fountain. The baths "del Higado," of the liver; "del Estomago," of the stomach; and that "de los Herpas," of herpes, at Penticosa, in Arragon, enjoy likewise much repute, as might be even guessed by the rather odd designations above specified. They are saline, and also contain nitrogen. Hæmoptysis, hæmatemesis, pulmonary catarrhs, chronic pneumonia, asthma, and protracted affections of the liver or kidneys, are reported to be often benefited by the waters which these places supply, if used judiciously. The baths of Esparraguera in Catalonia, and about twenty-one miles distant from Barcelona, also hold high favour in public estimation. The waters of this place are nitrogenous and sulphurous, having the warmth of 72° Fah. on an average. They enjoy much reputation in phthisis, hæmoptysis, asthma, and chronic catarrh; especially if accompanied by much physical debility. As a remedy in cutaneous affections, these springs have always been very popular; indeed at present the fame of Esparraguera has become much enhanced from being selected by Queen Isabella, who resided here some years ago, with, it is said, much benefit to her Majesty's health. The waters are employed both in baths and taken as an internal remedy.

In addition to the medicinal springs just mentioned, many others which stand well in public estimation might be also enumerated, as beneficial agents in curing or alleviating disease. But that is superfluous; whilst further to attempt such a task would be incompatible with the object proposed in the present brief allusion to localities heretofore little known to British medical practitioners, and still less to the public generally. Various establishments, especially those of most repute, have resident physicians or medical directors appointed by Government, and sometimes by the local magistrates. These direct how the waters should be administered, and otherwise superintend medical arrangements, at the places to which they are nominated, on much the same plan as in France. According to recent reports, the number of such medical officers amounted to about eighty; some of whom, however, enjoying at the same time two separate appointments. The salary allowed to most usually amounts to 8000 reals, or 84*l.* during the season, with a residence likewise; while all visitors, excepting recognised paupers, pay 10 reals, 2*s.* 2*d.* each, before obtaining the director's permission either to drink the water or to employ bathing. In short, the system generally established essentially resembles that pursued beyond the Pyrenees, which has been taken as a model by Spanish administrators, when regulating the governmental bathing establishments of their own country.

Besides such professional appointments at various medicinal springs, we would also mention, as the fact may seem worth stating to English readers, that there are, in addition to the above, numerous public medical and surgical offices throughout Spain, both in towns and country, which can alone be filled by licensed practitioners—as, for instance, the physician or surgeon appointed in particular districts to whom reference is made in certain defined cases by the public authorities, who distribute this patronage. In many of these situations the salary ranges from six to ten thousand reals per annum, in addition to collateral advantages. Among other appointments of that kind, the following may be here quoted as an illustration—namely, the office of physician-surgeon, lately vacant, to the town of Cantalojos and its suburb, Villacadima, in the province of Guadalajara, that is, where the baths of Trillo, already described in a previous page, are situated. According to a late advertisement, the salary there allowed is seven thousand reals per annum (about 74*l.* 8*s.* 6*d.*), a house free of all taxes, one cartload of firewood in the ratio of every 170 inhabitants which the place contains, and fifty bushels of wheat, to be delivered during the month of September. In other localities the patrons nominate both a physician and surgeon, having their respective attributes, but who enjoy each separate salaries. The above example will serve to convey an idea respecting the duties and professional position of these local officers, whose institution are alike beneficial to the public interests and advantageous to the profession. Before quitting the subject of these provincial officials, if only as a matter of curiosity, and likewise from being characteristic of Spanish manners, it may be added that in many places there exists also a functionary called the "sangrador barbero" (bloodletting barber), who is regularly appointed by the constituted authorities, and as such receives either a salary or perquisites. For example, the office of "sangrador barbero" to the town of Quintanilla, in the province of Burgos, was lately vacant; and although the allowance was only fifty bushels of wheat, without any money salary, nevertheless candidates were not wanting who wished to be successful even in this object of their ambition.

The hydrological literature of Spain in reference to its mineral waters is extensive, and dates backwards during many centuries, the earliest original document which treats that subject and is still extant, being a MS. written in 719 A.D., by a Benedictine monk named Hauberto, who specially describes the baths of Lacedon known as the ancient "Thermida." Some Spanish-Arabian physicians also wrote upon the same subject, among whom may be named Agmer-Ben-Abdala, of Toledo. This Arabian writer composed a treatise, in 1054, on the medicinal waters of Salambir, which is preserved in the Prince de Anglona's library, but translated into Spanish, and published at Madrid in 1761 by Dr. Frangeschi. Numerous other authors, especially during the last and present century, and amounting to about 270 altogether, have ably treated this question, which proves the marked attention it has always received in Spain from the medical profession. Therefore, however imperfectly the Peninsular medicinal springs and their virtues in the cure of disease may be known beyond the Pyrenees, the above statement respecting the large collection of treatises which have been written, especially during the current century, by native authorities, furnish good evidence that the several matters they discussed were amply and in many instances thoroughly investigated.

Much curious and interesting information might be quoted from various learned publications which that list comprises; but as such an extensive undertaking would be wholly beyond the scope proposed in this review, only some general reference can be here made to the principal maladies for whose relief recourse is usually had to different medicinal springs and baths of the Peninsula. Thus, it appears by published statistical returns, that, for three seasons ending in 1851, among 73,955 visitors frequenting particular localities named therein, 23,114, or nearly one-third, comprised gouty, arthritic, and rheumatic patients; 14,531, or about a fifth, laboured under cutaneous affections, being chiefly herpetic; 3968 were paralytic cases; 2549 suffered from the effects of syphilis; 2433 were classed as chronic gastro-intestinal complaints; while the remaining 27,360 comprehended not only persons who in some respects were invalids, but many going there solely for the sake of amusement or recreation.

Previous to taking leave of Spanish medicinal springs, some of which have been shown in former paragraphs to be not only of much repute but also valuably sanative, it seems not out of place to mention that two influences have materially contributed to bring them into notice and to promote their subsequent development: first, the conquest and long possession of the Peninsula by the Romans, who were always great patrons of bathing, while they erected various splendid buildings for its promotion; and secondly, the Saracenic domination, which ruled in most parts of Spain during upwards of seven centuries. The latter people being, religiously and otherwise, strenuous advocates of ablution, the baths and mineral springs of their conquered country hence became much patronized by that extraordinary, and in many respects civilized nation. After the Saracens were expelled, many of the previously celebrated places of resort for their baths fell into neglect. Now, however, these are again attracting public notice; and as various Spanish Governments during recent years took, and still evince, an interest respecting such matters, considerable ameliorations have been accomplished. Whenever greater facilities of transport shall exist, by reason of the several railroads now in progress of completion throughout some central districts of Spain, and moreover, when the beneficial effects of its medicinal waters get better known to foreigners, as also more extensively to natives, there appear well-founded prospects that various localities not yet sufficiently appreciated elsewhere, but wherein a residence is confidently stated often to improve physical health, will become quite as attractive to visitors as many analogous places which have acquired for ages high renown both in Germany and adjacent French provinces north of the Pyrenees.

Considerable discussion having lately taken place regarding the relative salubrity of different climates, and the advantages which some imagine may be attained by migrating to particular southern regions reputed to act favourably upon the physical frame of parties, especially if coming from localities enjoying less repute, a few general remarks respecting Madrid, as also of one or two other places which are occasionally recommended as residences for invalids, it is confidently believed, will not appear either out of place or unsuited to the present critique. First, then, we would observe in reference to the Spanish metropolis, more particularly as it is yet but little known to English medical practitioners, and far seldomer visited by British travellers than many other Continental capitals, that, compared with most European cities of a similar category, Madrid is by no means salubrious, since its average rate of mortality usually ranges much higher, with few exceptions, than other places possessing large and crowded populations. During most winter seasons, acute inflammatory diseases of the lungs, popularly denominated "pulmonia," rage exceedingly common, and often prove fatal to thousands of the Madrilenian people. The icy northern blasts which at that period blow down from the Guadarrama mountains, then usually covered with snow, contribute greatly to this mortality, while in summer excessive heat, an unusually dry, keen, and frequently varying atmosphere, in regard to its temperature, also produce much unhealthiness. Febrile diseases, rheumatism, and bowel-complaints are likewise often prevalent under these circumstances.

Perhaps, however, the most remarkable peculiarity characterizing disease in Madrid is the marked frequency of intermittent fevers during dry, scorching weather, and when the entire district has a hot, burning sun above, but is frequently enveloped in clouds of dust below, while utterly devoid of moisture everywhere. This prevalence of agues, which often assume a tertian type, becomes more singular, seeing the above-named maladies are generally considered as associated with malaria, stagnant marshes, decaying vegetable matter, and also peculiar to moist atmospheres. So far from such being the case in the arid Castilian plains surrounding Madrid, trees are there almost wanting, bare rocks often taking their place, while an aspect of desolation and unfruitfulness meets the eye outside its precincts in nearly every direction. Indeed, this entire district looks like the barren sandy wastes of Africa. No announcement is, however, more commonly made in the weekly reports of public health at Madrid, than that "muchas fiebres intermittentes de typo cotidiano y tertiano" are often exceedingly prevalent among Madriléños, both during summer and autumn, besides being not infrequently observed at other seasons. The great singularity now described appears so contrary to occurrences noted elsewhere, and under very different circumstances, that we think further inquiry seems necessary before being fully able to give any satisfactory explanation of these phenomena.

Among other features which the climate of Madrid portrays, is the marked difference of temperature that frequently prevails between day and night-time, and even on opposite sides of the same street, during sunshine especially. This fact must never be overlooked. Again, the river Manzanares often gets covered with ice in winter, which disappears after sunrise; while in summer, should the Solano, a south-east wind, blow through its thoroughfares, the city then becomes almost a furnace, the thermometer rising frequently to 100° F., or even higher, but falling again at night, sometimes to the extent of 40° or upwards, which alternation therefore exceeds treble the amount of analogous diurnal changes usually noticed in London. Knowing these characteristics of its climate, observers need not wonder if Madrid proves unhealthy, or at the custom which natives adopt of wrapping themselves in cloaks both during cold and hot weather; since "embozandose en las capas" becomes alike beneficial as prudent at all seasons. Therefore the Madrilenian atmosphere is most treacherous, and should due care be not taken, exposure to its action may produce hurtful consequences. This injurious influence being well known, has given birth to the popular Castilian proverb, "El aire de Madrid es tan sotil, que mata á un hombre, y no apaga á un candil:"—"The air of Madrid is so subtle that it kills a man and does not extinguish a candle." Besides, if contrasted with numerous other continental cities, Madrid will be found to occupy a more elevated situation, and consequently is much colder during winter than many a northern metropolis, but often hotter in summer, its site being 2412 feet, or about three-sevenths of a mile, above the sea-level. That the Spanish capital is hence an insalubrious residence cannot after these statements be now questioned.

During recent years public attention has been much directed to various foreign climates as salutary retreats for invalids. Among other countries, Spain has attracted some attention in that respect; Valencia and Malaga being especially subjects of remark by medical authorities. Upon Valencia eulogiums are often lavished in reference to its reputed salubrity; indeed, some writers even vaunt that this southern Spanish city is far superior to many celebrated French and Italian localities, for persons labouring under pectoral diseases. They likewise assert, as conclusive evidence, that the air of this district feels deliciously soft, and so dry that even salt undergoes very little or no change, unlike that which occurs in many more moist European climates. Frost is here almost unknown, while during summer pleasant marine breezes temper the excessive heat frequently prevalent elsewhere; hence enthusiastic partisans affirm that it proves admirably adapted to consumptive patients. But these laudations are not always founded on extensive practical observation, being frequently dictated by the gratified physical feelings of healthy travellers in quest of novelty or of the picturesque, especially if coming from more cold northern regions, who thereby conclude, erroneously, that because a climate seems agreeable to their animal sensations, it must of necessity be salubrious. This doctrine is obviously fallacious, and so much the reverse of reality, as scarcely to require any refutation, being besides unsupported by experience.

Irrespective, however, of these and other objectionable considerations, as there sometimes exists great difficulty in here obtaining comfortable accommodation for invalids during winter, particularly if visitors are numerous, that circumstance alone constitutes a serious drawback. Further, the air they breathe being often characterized, as already stated, by an unusual dryness, and aggravated through prevalent land winds, patients suffering from irritating coughs cannot consequently derive benefit. Moreover, as native inhabitants seem not exempted from severe affections of the respiratory organs, and consumption is also no uncommon malady among Valencian residents, while catarrhs occur rifely during summer, besides frequently terminating in disease of the lungs, Valencia does not appear a place really desirable for selection by persons suffering from pectoral maladies. Again, the greatly extending cultivation of rice, even close to the city itself, does not improve either its salubrity or that of the celebrated "Huerta" (garden), which the surrounding district, from being the most fertile plain in Spain, and almost unequalled throughout Europe for its productiveness, is called. Nay, the malarious emanations from such grounds, augmented by extensive irrigation, particularly during the hot weather which here prevails many months, and also when certain winds blow over these humid swamps, all become influences exceedingly prejudicial to the physical health of even robust individuals. Besides such cogent objections, it may be added that the pleasant evening zephyrs, always praised by ignorant strangers, and which even residents consider so enjoyable after sunset, sometimes prove most treacherous; while competent medical authorities affirm, that since the fashionable promenade called "La Glorietta" has become on summer nights the accustomed resort of Valencian male and female pedestrians, consumptive cases have much augmented in number.

Fever, rheumatism, and nervous maladies are likewise common in this district. Therefore, before deciding the question now mooted, invalids, or visitors in search of a pleasant yet healthy climate, ere choosing Valencia, should pause, at least until they have obtained further reliable information respecting the applicability of its climate to the particular malady wherewith they are afflicted. Great doubts have been expressed on this point by a recent medical traveller, who remarks, when speaking of Valencia, from all the evidence he personally or otherwise could obtain, while making a holiday tour in Spain: "There prevails much exaggeration in the florid accounts so often given regarding the healthiness of this locality." Without first obtaining additional facts on the subject, impartial critics should therefore hesitate before believing that this Spanish city really constitutes the elysium which it is often reported to be by foreigners.

Somewhat analogous observations may equally apply to the second district also mentioned in a previous paragraph—viz., Malaga, which of late has been likewise frequently lauded on account of the assumed curative influence which its climate is reported to exert upon pulmonary disease. Some writers of repute even believe that this seaport is superior to numerous southern localities enjoying at present much popular favour as salubrious retreats for invalid constitutions. Notwithstanding the high encomiums expressed by various medical authorities, grave doubts are still entertained by others respecting the advantages which Malaga virtually possesses as a salutary residence, especially for consumptive victims. Pulmonary maladies commonly prevail throughout this city and its vicinity, as is proved by the following conclusive fact lately published—viz., that out of 1469 deaths officially reported to have occurred during nine consecutive years, among patients treated in the General Hospital, 595 fatal cases, or upwards of forty per cent. of the total mortality, were caused by pectoral complaints.

Seeing, therefore, that severe affections of that description so often originate, and terminate fatally, in this southern region of Europe, it cannot surely become eminently sanative to persons migrating thitherward from much colder countries, particularly invalids having the germs of thoracic disease already manifested in their bodily frames. The winters may be mild compared with situations farther north; but it is both an instructive and curious fact, that here the largest amount of sickness invariably prevails during December and January, while in summer the dust, great heat, and often long-continued dryness of the atmosphere—always injurious to phthisical sufferers—then get nearly unbearable. According to observations made on the spot by competent judges, the Malaga climate has been much overpraised, and in the opinion of judges entitled to speak, cannot be recommended like Hyères in France, Pisa in Italy, or even some places along the south coast of Ireland and Devonshire.

The narrow tortuous streets and often unpleasantly odoriferous alleys of this ancient Moorish city, now teeming with a crowded population, who occupy filthy and badly-ventilated residences, usually render it far from being healthy. But further, as the upper ranks of society mostly live in palaces from which sunshine and the outer atmosphere are too frequently excluded, many of the houses will be deemed undesirable dwellings in an Englishman's estimation. The Alameda forms, however, an exception, and in winter becomes really an agreeable promenade. Here there are excellent apartments in the large structures occupying each side, while visitors will find numerous pretty "quintas" (villas) on various heights, commanding beautiful sea and land prospects over the immediate neighbourhood. In these modern dwellings on the Alameda, or at adjacent quintas, foreign invalids usually reside during the winter seasons.

Elsewhere throughout Malaga many of its streets are rather lanes, generally full of dust in summer, but often muddy in winter, or, as a recent traveller has quaintly said, places where "dirt, dust, and donkeys often reign supreme;" while, being very confined, should it happen that two porters with raisin boxes on their backs, or opposite lines of loaded quadrupeds, unluckily meet therein—which contingencies frequently occur—then strangers will experience the often recurring inconveniences of living in such localities. The want of drives for recreation in the open air during afternoons or fine evenings when the weather is propitious, forms likewise a great drawback to sick Malagnenian residents or visitors having no occupation. The river Guadalmedina, which flows near the city, being generally quite dry throughout summer, its bed is often then used as a promenade; but as thick clouds of dust are prevalent at such times, and are frequently also driven about by gusts of wind blowing down from adjacent mountains, invalids must always carefully avoid these annoyances. Matters would be very different were public authorities to imitate an obsequious Madrid magistracy, who, it is reported, were so compliant as actually to water the dried-up course of the Manzanares stream when a late King of Spain travelled by that route towards the metropolis on a summer evening, lest any dust might occasion discomfort to his Majesty, or to accompanying courtiers. But the aqueous element being a scarce article at Malaga during hot seasons, and even purchased in small quantities like wine, which abounds in this prolific grape-growing district, it seems very improbable that the present suggestion will be actually adopted, however refreshing the effect would prove to promenaders when breathing a hot dusty atmosphere.

Irrespective of the objections now curtly stated in reference to Malaga being selected as a residence for parties in search of health, a very remarkable influence which its air is said under certain circumstances to produce upon native residents merits special mention. The feature here alluded to is the following:—When the *terral* or north-east wind rushes down towards Malaga, from the mountain gorge called *Boca dell Asno*—Donkey's Mouth—and which blows sometimes with great violence, being always cold in winter, but burning hot during summer, such blasts often produce so great an excitability and restlessness in the human system, that quarrels, wounds, and even suicides consequently prevail in larger numbers, if contrasted with other seasons. In fact, this very peculiar action of the *terral* upon Malagnenian organisms is so well known and recognised, that the district judges are said to modify their sentences upon criminals in consideration of its prevalence, and hence similar to a proceeding in legal tribunals which, as Sir Woodbine Parish has stated, likewise obtains at Buenos Ayres, in South America. In that city, when *el viento norte* comes from over the great Pampas plain southward, it frequently produces so much nervous excitation, and even temporary derangement of the moral faculties, as to cause crime and bloodshed among the populace. Indeed, this singular action upon the vital system of natives is reported to be so very marked, that advocates even plead in court the prevalence of a north-west wind as some extenuation of their clients' delinquencies.

Occasionally other situations in Spain are recommended as salubrious retreats for invalids; among which Cadiz, Sevilla, Granada, and Ronda may be mentioned, seeing that these places have each acquired some reputation in that respect. The locality first named was at one time believed to possess a salutary climate for consumptive cases, but that opinion is erroneous, while phthisis occurs frequently among its natives. This sea-surrounded city is no doubt the cleanest throughout Spain, and in some of its features forms an agreeable residence. But the water is bad, and during a horrid *solano* wind it truly becomes detestable, as that searching blast then finds out whatever may be wrong in the nervous system, especially of strangers, who feel most uncomfortable, or much the same as when the *sirocco* prevails in Southern Italy. The *solano* likewise produces somewhat analogous effects upon the human frame which were specified when speaking of Malaga, quarrels and bloodshed being more frequent at such periods than under opposite circumstances; while writers have also said that Caditan courts of justice occasionally make allowances for its irritating influence upon criminals brought up for judgment. Moreover, this climate is very subject to considerable variations from its exposed situation, and the air becomes also often damp; besides, too, the *levante* (east wind), and also that from the north-east, make it by no means an agreeable sojourn under such circumstances. Altogether, such a marine locality cannot be much recommended for valetudinarians.

On the other hand, Sevilla is much superior as a residence for persons of impaired health during most winter months, since at that period its temperature feels usually mild, frost and snow being very rare events. Nevertheless, consumptive invalids should always carefully eschew Sevillan night air; and when the Levant wind exists, they must remain at home with closed windows and doors, in order to counteract if possible any irritating injurious influence upon most human constitutions. One remarkable characteristic of the climate of this district must not be overlooked—namely, great calmness; in this respect Sevilla bears considerable resemblance to that of Pau, in Southern

France. Aerial disturbances are there exceptions, and for days, or even weeks consecutively, any wind is scarcely perceptible. During summer, however, this Andalusian capital must be avoided, as the heat often gets almost intolerable, and its climate at that season feels really African. The air is, further, then frequently very dry, besides being of high temperature; respecting which features we can speak authoritatively from personal experience, having ourselves visited Sevilla during a recent autumn, when the heat became most oppressive, and locomotion in its confined streets, and even promenades, was rendered the very reverse of agreeable, more especially as not a drop of rain had fallen for many weeks whereby to lay dust, or modify the constant burning sunshine. Late in the autumn, and again during spring, much wet weather, however, often prevails, from whence inundations of the river Guadalquiver supervene, which produce fever, as also other epidemics. At such seasons this ancient city becomes unhealthy, and consequently a residence within its precincts should be shunned by invalids.

The far-famed and interesting Moorish metropolis, Granada, has occasionally been also recommended by writers as an eligible retreat in cases of impaired health. This opinion may be well founded for persons requiring a bracing atmosphere, but in pectoral maladies the case is quite otherwise. During winter, as the adjacent Sierra Nevada mountain range, which in several peaks exceeds the height of twelve thousand feet, is also then always covered with snow, from whence cold blasts of wind frequently rush down into the city lying at its base, the weather hence becomes exceedingly ungenial, as also injurious to health; while in spring, serious inundations take place from the snow melting rapidly. Similar events often occur in autumn, as, for instance, only last season, when the neighbouring town of Santa Fé was nearly destroyed by rivers being overflowed. Although during summer the scorching heat of an Andalusian sun becomes often moderated by cool mountain breezes, at night the temperature sometimes falls considerably, as we personally experienced when visiting Granada during the month of September, the weather being then unusually dry, from little rain having fallen for many weeks; and thus dust, not mud, became one of a Spanish traveller's grievances. At that period most evenings really felt chilly; and often after sunset, in consequence of the Sierra remaining still capped with snow, the nights were cold, as also ungenial. Through these and other inimical influences, this locality is not usually deemed a health-restoring residence for invalids, unless, perhaps, during spring, at which season the climate is reported to be often pleasant, but at other periods of the year it cannot boast of any superiority. Nevertheless, to antiquarians and enthusiastic admirers of the picturesque, this celebrated capital, although now dwindled down from half a million of inhabitants to less than one-tenth of that number, will often appear an agreeable sojourn to foreigners, especially to Englishmen who admire Moorish architecture.

While almost every inhabited spot of Southern Spain seems during most summer months as if within the tropics, one town forms an agreeable exception—namely, picturesque Ronda, which occupies an elevated position near a mountain range called the Serrania. This retreat, really cool at a time when the whole adjacent country feels almost like an oven, is situated about twenty-five miles inland from the Mediterranean. It lies on the banks of the Guadalvin river, has many romantic prospects on every side, as it is three thousand two hundred and seventy-nine feet above the sea level, and is designated the Tivoli of Andalusia, on account of its various local beauties. This town being a pleasant residence during the hot season, becomes therefore much frequented at such periods. Besides the advantages just mentioned, Ronda is considered salubrious, and the natives are reported to be remarkable for longevity, as indicated by their common saying, "Los hombres á ochenta son pollones:"—The octogenarians are chickens.

Reasoning upon the facts now narrated, as likewise various opinions expressed by reliable authorities, and further, from our own personal knowledge, we would conclude these brief remarks respecting several localities in Spain which have been of late occasionally recommended as salutary residences for invalids, by stating that (notwithstanding some special objections which medical advisers might reasonably urge in reference to several localities now named), should strangers intend to pass an entire year in the southern Spanish provinces, whether for health or pleasure, we would advise them to spend the winter months in Sevilla, spring in Granada, summer in Ronda, and autumn at the baths of Alhama or Carratraca. Following the plan now sketched, visitors will enjoy, we believe, the largest amount of gratification, and suffer the fewest inconveniences compatible with any Peninsular peregrination.

Again, we must not forget to comment upon the exceeding prevalence of fever among the inhabitants of Spain; such maladies, historians say, having been at all periods common in Spain, and especially during ancient times, when epidemics very often devastated the entire peninsula. Throughout the Saracenic domination pestilences were exceedingly rife, and subsequently fevers, which proved very fatal, were also of frequent occurrence. For example, during 1347 and two consecutive years, Tabardilla—spotted fever—in fact, plague, ravaged the whole kingdom to such an extraordinary extent that its population actually fell thereby to one-third. Again, in 1649, more than 200,000 persons died from fevers, especially in Sevilla and its vicinity; while reliable authorities report that various localities, more particularly in some southern provinces, rarely remained free from such maladies, whether putrid, intermittent, or contagious. This has proved often the case, not only in the city just now named, but likewise at Malaga, which is said to have actually lost half its population by fever during the first three years of the seventeenth century. Moreover, 40,000 residents there died by the same cause in 1637; further, during 1648, 1678, and 1741, an analogous epidemic malady devastated this district. While thus adverting to the frequency of fevers in this southern part of Europe, it may be perhaps interesting to English practitioners to mention, with reference to their treatment, that an eminent Jewish physician of

Toledo, named Ben-Albalid Albinsi, who wrote about the thirteenth century, then recommended the affusion of cold water as an efficacious remedy in adynamic fever, and hence anticipated Dr. Currie by 650 years at the least.

In conclusion, we would now state that not only fever, but other diseases have frequently prevailed epidemically throughout the Spanish peninsula during former and recent periods. For instance, diphtheria, which has lately attracted considerable attention, was common many ages past in Spain, the name it acquired being "Garotillo," "Morbus suffocemo," or "Esquinancias gangrenosas," and was said by medical authors to have been originally propagated from Astracan through Europe. The disease prevailed extensively both in Italy and Spain during 1530, as also in the latter country about 1596, having commenced at Granada, from whence it spread over the peninsula, and proved then very fatal everywhere. Early in the seventeenth century the complaint again became epidemic, particularly at Madrid and adjacent provinces; while in 1613 it was so rife and mortal in many districts of the country, that this year was designated historically, "El año de los Garotillos." Besides other epidemic visitations, whether fever, garotillo, or maladies which have been often noticed during more recent periods, cholera has also prevailed several times in Spain since its recent appearance in other parts of Europe. Among places which have lately suffered from this scourge, and where its victims were numerous, Valencia, Murcia, and Malaga, may be specially mentioned. Indeed, only last summer the city last named became severely attacked by cholera, which in a brief period caused the death of thousands, among whom may be recorded a distinguished English physician and member of Parliament, who was visiting Malaga at that unhealthy season.

Review III.

1. *Toynbee on Diseases of the Ear.—London*, 1860. pp. 422.
2. *Die Anatomie des Ohres.* Von Dr. A. F. von Tröltsch.—*Würzburg*, 1861. pp. 105.

The Anatomy of the Ear. By Dr. A. F. von Tröltsch.

3. *Wilde's Aural Surgery.—London and Dublin*, 1853. pp. 496.
4. *Die Ohrenheilkunde in den Jahren* 1851–1855. Ein Nachtrag zu der Erkenntniss und Heilung der Ohrenkrankheiten von Dr. Wilhelm Kramer, Königl. Preuss. Sanitätsrathe.—*Berlin*, 1856. pp. 136.

Aural Surgery in the Years 1851–5. By Dr. W. Kramer.

5. *Taubstummheit, Ohr- und Gehoerkrankheiten.* Von F. L. Meissner. —*Leipzig and Heidelberg.* 1856. pp. 387.

Dumbness, Diseases of the Ears and Hearing. By F. L. Meissner.

Nearly two hundred years have elapsed since Heurnius Mercurialis published his prelections 'De Oculorum et Aurium Affectionibus,' and yet in two English works, whose titles head this chapter, we find the authors remarking how readily medical men affirm "that they know nothing about diseases of the organ of hearing."

This is but too easily explained by the fact that, except in one or two medical schools, no special attention is given to the subject; true, the anatomist lectures on the structure of the ear, but the student finds it almost impossible to reconcile his chipped and incomprehensible dissection of the "petrous portion of the temporal" with a teacher's descriptions and diagrams. An examiner who, five years ago, would have inquired very minutely into a candidate's knowledge of the ear would have been considered as little better than a Herod, and his questions would probably have been followed by a massacre of innocent hopes terrible to contemplate.

Yet why should this be? how much of human happiness depends on the integrity of what is contained in that little nut of hardest bone; to the child, whether he is not only to hear but speak; to the adult, whether he is to pass through busy life with "the exciting scene silent as a picture;" to the aged, whether the last years of life are to be deprived of rational enjoyment.

While we cannot deny the truth of Messrs. Toynbee and Wilde's somewhat humiliating statement, we at the same time feel grateful to them for it, and to all others who have endeavoured to bring this important class of maladies under the control of man.

Classifying aural diseases is not such an easy task as would appear at first sight. A simple anatomical arrangement suggests itself to every educated surgeon, and such an one was adopted by Du Verney and Mr. Wharton Jones; but, as Mr. Wilde observes, "as it is not possible to limit disease, especially of an inflammatory character, to any particular structure, so is it impossible to carry the anatomical division beyond a certain extent." Nevertheless, though he pays Lincke the high compliment of considering him "the best modern classifier," he adopts the anatomical method as far as he can. Lincke's classification may be shortly stated as a division into

I. Inflammations, simple and specific.
II. Affections caused by solution of continuity.
III. Affections caused by cohesion of parts.

Mr. Toynbee follows the anatomical method, beginning with the auricle. This appendage probably serves to collect the sonorous vibrations, and though not an essential, must surely be considered an important part of the auditory apparatus. Nevertheless many, as Itard, say it is of little or no use; and Richerand says it "may be removed without injury further than a little hardness of hearing for the first day or two." On similar evidence it would be no less strange to affirm that the external auditory meatus is of no use, because we every now and then meet with cases where the external ear is imperforate, and yet a considerable amount of hearing power remains. The writer knows of a girl who does the duties of housemaid in a public institution, and has a very useful amount of hearing, without either auricles or apparent meatus, and yet he would hardly affirm that the ear-hole would not be an important addition to her hearing apparatus.

The auricle is co-heir with other parts of the body to many ills of the flesh, and in addition has been subjected to maltreatment from the earliest ages. The Hebrew bondsman who declined to leave his master when his years of servitude had expired, had "an awl thrust through his ear unto the door, and he shall be thy servant for ever: and also unto thy maid servant shalt thou do likewise." This would probably serve the double purpose of testing the sincerity of his wish to stay and marking the owner's property. The elegant vestige of barbarism, the wearing of earrings, is occasionally followed by keloïd growths of the lobule. Mr. Wilde figures an ear with a tumour between the helix and concha, which he says felt like a hydrocele, and was full of a glairy fluid, resembling the contents of a ranula or enlarged bursa, contained in a smooth polished sac. This author states that "Morbid growths are by no means uncommon; steatomatous and sebaceous bodies form on the concha," and thus differs from Mr. Toynbee, who says that the "*only* encysted tumour found on the auricle is a collection of blood between the cartilage and the skin," which was thought at one time to be peculiar to the insane, and on the origin of which Dr. Thurnam seems to throw considerable light by the remark that it is less common now than in the days when insane patients were more harshly dealt with. Indeed, one is somewhat inclined to class it with the cases Mr. Wilde alludes to as having been common in the days of faction fights in Ireland, where the morbid appearances might be traced to "blackthorn sticks."

Cancer fixes its steady gripe upon the auricle, though fortunately, as Walshe remarks, it is rarely the seat of primary disease, though it may spread from the cheek or lip. Nor do records exist showing in what manner the affection originates, when primary in this situation. Dr. Kramer describes the early stages of scirrhous degeneration of the auricle as similar to those of erysipelas. In the case operated upon by Dr. Fischer, the patient suffered when eight years old, and the disease gradually progressed till he was twenty.

So far, aural diseases are simple enough, but as we pass further in, they become more complicated, and the student soon finds that unless he is familiar with its anatomy, he is like one entering a new country, with but a few partially understood names to guide him through dark and winding ways.

No further than a line or two from the orifice are the ceruminous glands secreting a band of wax "half an inch in length and half a line thick," of such consistence as will enable it to retain its position and catch the particles of dust which are floating in the atmosphere. Slowly secreted in health, it is gradually expelled by the motions of the lower jaw. But sometimes these minute glands strive to imitate the vices of their betters, and secrete too much or too little, or not at all, or what they secrete may be too soft or too hard. Liquid cerumen runs incontinently from the ear, but hard wax remains in the meatus, and more and more accumulates; the person endeavours to remove it, and only drives it further in, with additions of hair or epithelial scales, which serve, like the straw in the Israelitish bricks, to insure the cohesion of the mass.

This pellet of wax may now be compared to an opaque lens in the eye: no light can get through the one, no sound through the other, and none past it, should the dermis be tumefied. Not only will it now cause deafness, but by pressure it may excite absorption of the bony walls of the ear-passage, and by forcing the membrana tympani inwards, induce the peculiar cerebral symptoms supposed to be caused by undue pressure of the stapes against the contents of the labyrinth. One would imagine that nothing could be easier than to remove this mass of wax, and perhaps ordinary surgeons may think so. Mr. Wilde recommends a spatula or forceps for the purpose, thinking "the mere projection of warm water from a syringe" scarcely efficient by itself; whereas Mr. Toynbee says "the only judicious mode" is by the use of the syringe. When it is very hard, both these gentlemen advise softening it with oil or an alkaline solution. To be sure it may be merely an instance of the universal ignorance of aural surgery which these writers deplore, but we somewhat dislike this dogmatizing upon manœuvres which must be simple enough, even in the most (comparatively) complicated cases. We all know that one man can extemporize an instrument which he will make do what he pleases, while another may be too awkward even to use a stream of water with impunity to his patient. It is safer to be cut for a strangulated hernia by one man, than to have the bowel returned by the taxis of another. Specialists, as they are called, should be especially careful to avoid the

> "Turgid style,
> That gives an inch th' importance of a mile;
> Casts of manure a wagon load around,
> To raise a simple daisy from the ground;
> Creates a whirlwind from the earth to draw
> A goose's feather;"

or even a "large compact mass of cerumen" from the ear.

At the same time, we quite agree with the authors in considering it occasionally very difficult to remove small round foreign bodies from the meatus. And it is scarcely possible to attach too much importance to discharges from the ear, for they are but too commonly followed by what Mr. Wilde calls "that disgusting disease, otorrhœa;" or the inflammation becomes periosteal, and spreading to neighbouring structures, produces fatal consequences: in fact, so prone is this diffusion to occur, that Mr. Wilde confesses to being forced away by it from the anatomical system of classification. Indeed, the violent pain and fever accompanying inflammatory attacks of the meatus and outer layer of the membrana tympani, so frequently followed by rupture of the latter, show, in his opinion, that the mucous membrane on both sides of the membranous diaphragm which separates the two portions of the auditory meatus is generally involved.

Allowing for the differences which exist between cases, it would seem that for the acute inflammatory attacks purgation, fomentation, and local depletion are chiefly to be relied on; and in the more chronic forms tonics, astringent washes, and counter-irritants—in short, the ordinary principles of surgery suggest all that is known with regard to the treatment of these aural inflammations.

The sudden stopping of a discharge from the ear is popularly looked upon with great suspicion, especially when it has been arrested by local applications; but "there can be no doubt," says Mr. Toynbee, "that the cessation of the discharge is one of the *effects* and not one of the causes of the inflammation . . . . . the symptoms that follow are not dependent upon the cessation of the discharge [for *it* does not always diminish], but upon inflammation caused by the irritant." We see in other parts of the body that an amount of inflammation may be set up by some local application with which purulent formation seems incompatible. But it all comes to this,—beware how you tamper with an external auditory meatus from which flows a discharge, for it contains all the elements of great danger to your patient, and you may set them loose upon him when you least expect it. Mr. Toynbee advises rather to treat symptoms, and to remove by attention to the general health every obstacle to cure, and then, though the aural surgeon see the disease make very slow progress towards *amelioration* (*sic*), he must be content that its "advance towards the bone and the brain is averted." The reader is here inclined to open his eyes—Allah, bismillah! your servant has eaten dirt; he has crept through layer after layer of epidermis, dermis, &c.; and he is now only taught how to make his patient *no worse.*

Again, the discharge is frequently only a symptom of more internal disease, and perhaps of some irritation, in the tympanum; or, in children, it may be more superficial than you wot of, merely the dermis secreting a watery fluid which floats off the "epidermoïd cells;" or some little sequestrum trying to escape through the fibrous meatus; or there may be some "raspberry cellular polypi," or granulations exuding pus, which little growths, Mr. Toynbee says, sometimes arise from obstruction of the Eustachian tube. Of how this should cause them to grow, we would like some explanation.

Other growths are apt to form in the meatus, the most curious being the slow-growing exostosis, which very much resembles the excrescence on a beech-tree, and gradually fills up the passage, causing deafness. The temporal bone seems, above all others, addicted to these bony outgrowths; Schmalz, Otto, and many others, record instances; Kramer tells of one which hung like a stalactite from the roof of the meatus. They seem to be somewhat under the influence of iodine locally applied.

The membrana tympani, the driest membrane of the body, has absorbed the attention of many who saw different beauties in it, attributed to it different functions, and demonstrated in its gossamer thickness different layers of very problematical fibres. Where are now the radiated muscular fibres of Sir Everard Home? where Sir Astley Cooper's perforation of the tympanum? Instead of the one, we have a "bilaminated fibrous layer," and instead of making holes we stop them up. But all agree "that it is the agent whereby vibrations are conveyed from the meatus externus to the tympanum, and in conjunction with the muscles and the bones of the tympanum acts as the analogue of the iris."

Should a hole large in proportion to the area of the membrane occur, then it is evident that the vibrations of the air in the tympanum would no longer be confined within that cavity, and in consequence, would strike more feebly against the membrane of the fenestra rotunda. With this state of matters of course must coexist a diminution of the hearing power, and the obvious indication is to keep the air in the tympanum; we may do so by inserting a little plug of moistened wool, as suggested by Mr. Yearsley in 1848, or by Mr. Toynbee's elegant little caoutchouc diaphragms.* These gentlemen differ as to the *modus operandi*, and Mr. Wilde says "that we still require a feasible solution for it;" but we think that Mr. Toynbee's is a very satisfactory one. Given a drum with a hole in it, to restore its resonance. Answer: "Stop up the hole, or put a new skin on." However it acts, all these gentlemen, and no doubt many besides, have treated cases successfully with the plug or diaphragm. Mr. Toynbee says he has treated hundreds beneficially; they could hear voices across a room, and one heard "the voices of boys at a *distance of between one and two fields.*" There is something so pleasant in the thought of the habitually unhearing man being roused in his moody walk by the lads' merry voices ringing in the quiet summer gnatiferous English evening air, that we forgive him who measures "hearing distances" by inches, the vague estimate of "one or *two* fields."

Mr. Toynbee gives a minute account of the articulation of the stapes, describing its being hung, as it were, upon ligamentous fibres which pass from within towards its tympanic surface. Dr. A. Tröltsch, on the other hand, denies that it has further connexion with the vestibule than what it obtains from the periosteum of the latter spreading across its base. From his own dissections, the writer would be inclined to agree with Mr. Toynbee, but no joint (if we may call it such) in the body seems more liable to change of form than this; indeed, we rarely find the margins of the fenestra ovalis alike in two subjects. In some, one side, or perhaps both, are expanded as a lip over the aperture, preventing any motion except upwards and downwards; in others, the base of the stapes seems to have become, as it were, one with the fenestral margin, and cannot be displaced even by force applied from the vestibule. This ankylosis of the stapes, which Mr. Toynbee tells us is such a common and irremediable form of deafness, is termed by Tröltsch, calcification of the fibrous structures at its base. It seems to be a change consequent on age or the rheumatic diathesis.

There are some points in the anatomy of the ear which we regret have not been more fully discussed, one is the chorda tympani; the German anatomist we have just quoted describes the results of some experiments

* See No. 24 of this Review, page 460, for a notice of Mr. Toynbee's observations on his artificial membrana tympani.

with galvanism upon this nerve, by which he finds that the stimulus directly applied to it produced a peculiar sensation on the tongue.

What *is* the chorda tympani? Mr. Wilde adheres to the theory that it is the vidian branch of Meckel's ganglion, though we never could see any good proof for this statement, but would refer the reader to Professor Luigi Calori's paper on this nerve, in the 'Transactions of the Royal Society of Bologna,'* for a clear and philosophical description of this mysterious nerve.

We confess to having expected in such a recently published work as Mr. Toynbee's, that there would be some more on the subject of the cochlea than we find. It is, indeed, most probable that Dr. Thomas Young was very near the truth when he called it a "micrometer of sound," for there seems little doubt that the rods of Corti form with the terminations of the cochlear nerves so many acoustic arrangements by which musical sounds are appreciated. We may grant, then, this function without confounding it with what Goodsir terms "the æsthetic perception of the sensations produced by the instrumentality of the cochlea,† its nerves, and the sentient centre, which is a psychical function and a result of the pre-established harmony between the mental and corporeal elements of the animal constitution on the one hand, and external nature on the other."

Mr. Wilde says that "the musical ear in many instances remains unimpaired, though general hearing is much affected;" and Mr. Toynbee has shown in one very interesting case, that the dormant powers of an ear may be roused to usefulness by the stimulus of sound. It is possible that in many deaf ears there may be some part left in which its integrity may be reached by sounds, or that class of sounds appropriate to itself.

What every student who takes up these volumes must remark, is the constant repetition of certain remedies, the syringe, the blister, and ear trumpets. Unto this last too many cases come.

It gives one a melancholy idea of therapeutics in general, when we see no greater practical result follow such good and vigorous work; but if these writers had done nothing more than convince their professional brethren of their ignorance, of the possibility of and necessity for careful diagnosis of aural maladies, we should owe them a deep debt of gratitude, but more especially as they have and will induce many to follow up their investigations in the same spirit of patient inquiry, and send more physiological Livingstones to explore the half discovered country, and the great river of living waters of knowledge—

"Along whose margin
Grows the wondrous tree
Whose leaves shall heal the nations."

* Sessione delli 18th Nov. 1852.

† Edinburgh Monthly Journal, 1854.

Review IV.

1. *An Account of the Life, Lectures, and Writings of William Cullen, M.D., Professor of the Practice of Physic in the University of Edinburgh.* By John Thomson, M.D., F.R.S. L. & E., Professor of Medicine and General Pathology in the University of Edinburgh. In 2 vols. Vol. I. first published in 1832, now re-issued along with the second volume, and having prefixed to it a Biographical Notice of the Author. Vol. II. commenced by Dr John Thomson and Dr. William Thomson, and concluded by David Craigie, M.D., F.R.C.P.E., F.R.S.E., &c. &c.—*Edinburgh and London.* 1859. pp. 1524.
2. *The Works of William Cullen, M.D., Professor of the Practice of Physic in the University of Edinburgh; containing his Physiology, Nosology, and First Lines of the Practice of Physic, with numerous Extracts from his Manuscript Papers, and from his Treatise on the Materia Medica.* Edited by John Thomson, M.D., F.R.SS. L. & E., &c. &c. In 2 vols. 8vo.—*Edinburgh.* 1827.

We hope that when, a year and a half ago, the bulky volumes which stand first of those prefixed to this article were given to the world, the editors did not delude themselves with the idea that their perusal from beginning to end was at all probable. Surely the commonest charity would forbid the imposition of such a task upon any but the most idle. It grieves us to state that a more injudicious compilation, or a more oppressive attempt at professional bookmaking, it has never been our lot to meet with. And the life and labours of such a masterly and perspicuous teacher as was Cullen have no business to be made a clog upon any library table, when we might have had a pleasant and readable narrative of that illustrious physician. It would appear that in the year 1832 Dr. John Thomson, of Edinburgh, published that first volume of the 'Life of Cullen' with which most of us are familiar, giving assurance to the public that this instalment of biography should in due time be followed by a supplementary issue. This assurance, however, was not fulfilled until years after the death of the elder Thomson. The work, indeed, was in progress, but received a temporary interruption by the removal from Edinburgh to Glasgow of the author's eldest son, who had greatly assisted him in the compilation. The subsequent decline of the father's health, his death in 1846, the son's increasing professional duties in his new sphere, followed also by death in 1852, have thus, until recently, prevented the fulfilment of a design which both Dr. John and Dr. William Thomson had deeply at heart, and for the accomplishment of which they laboured diligently. A younger brother and son (Dr. Allen Thomson) now appears upon the scene, and, charging himself with editorial superintendence, consigns a huge amount of historic labour to Dr. Craigie, contenting himself with a graceful dedication to Sir Benjamin Brodie, a history of family antecedents, and a general arrangement of this second volume. So that the work, in its complete and bi-volumed form, which has been thus launched upon the crowded and restless sea of literature, is something more than a biography of Cullen. It is, in fact, a biography of that great physician; but it is also a biography of Cullen's two biographers. In short, eighty-four pages are devoted to a sketch of Dr. John Thomson, eight pages to a notice of Dr. William Thomson, and the remaining fourteen hundred and thirty-two to matters which bear directly or indirectly upon the author of the 'Nosology' himself. We are not at all disposed to question the "rare learning in the general history of medicine" which Dr. Allen Thomson attributes to his friend Dr. Craigie; but we cannot help thinking that this very learning has been the cause of much irrelevant disquisition and of much intolerable prolixity. The amount of space, indeed, which is devoted to the conflicting opinions of those who may not have thought with Cullen is quite extraordinary; and the reader is lost in bewilderment at views and theories which might have been "buried in the night of oblivion," inasmuch as they are not only not at all essential, but absolutely detrimental, to the elucidation of Cullen's career. To take one instance: in the second volume alone, two hundred and eighty-four pages (besides nine pages in the Appendix) are devoted to an elaborate disquisition of "Brunonianism," and to a history of the Italian medical doctrines springing therefrom. Who on earth but an unhappy "reviewer," to whom such things are a necessity, can afford time or patience for so minute an analysis of the "new, perfect, and unalterable system" of Dr. John Brown? Even the editors themselves (we speak plurally) seem startled at the nature of the country over which they have dragged us, and lead us back again, after a protracted diversion, to the nominal subject of this memoir, in the following language: "We have at length traversed the rugged and thorny regions of controversy and misrepresentation; of rival conflicting and disputed claims; of alleged discoveries, reforms, and improvements in medical theory and practice." (Vol. ii. p. 506.) It is to be wished that Drs. Allen Thomson and Craigie had been under the pressure of a more condensed atmosphere than they appear to have breathed during the construction of this biography, and that they had been more mindful of what he whose theories they expound has written: "My business is not so much to explain how this and that happens, as to examine what is truly matter of fact; my anxiety is not so much to find out *how* it happens, as to find out *what* happens."*

From a paucity of biographic materials, which the editors themselves deplore, and which makes the great bulk of this work more inexcusable, we shall endeavour to furnish our readers with a compendious narrative of a physician who, without having ever manifested a high order of intellectual faculty, is yet more associated in our minds with the medical progress of the last century than any other individual.

William Cullen was the second of a family of seven sons and two

* Works, vol. i. p. 442.

daughters, and was born at Hamilton, on the 15th of April, 1710. At the grammar-school of that town, and afterwards in the University of Glasgow, he received his education, and cultivated those qualities by which he was ever afterwards distinguished—great quickness of apprehension, and a very remarkable memory. In the great Scottish centre of industrial commerce he likewise initiated his medical career, being apprenticed to a general practitioner or apothecary of the name of Paisley. Finishing his studies there towards the end of 1729, the young Cullen went to London, with a view of obtaining a situation in which he might enjoy opportunities of acquiring a practical knowledge of his profession. It is not narrated (though the subject is suggestive of speculation in reference to that puzzle of puzzles, a modern 'Bradshaw') what time was occupied in the transit from one city to the other. The fact, however, is on record that Mr. Paisley's pupil did not remain long in London, for he was almost immediately appointed surgeon to a merchant vessel trading to the Spanish colonies of the West Indies, whose captain was a Scotchman and a relation of his own. During the voyage thither, and there, he had the advantage of observing the diseases peculiar to those latitudes, and the effect of climate upon the human constitution. In the beginning of 1732 young Cullen returned to Scotland, to find himself the eldest surviving son, charged with the duties of arranging the affairs of his father (who had been dead some years), and providing for the education and settlement of the younger branches of the family. Fortunately for both parties, his late commander (Captain Cleland) had returned also to his patrimonial estate of Auchinlee, in the neighbourhood of Hamilton, and he invited William Cullen to reside with him and superintend his son, who was afflicted with a lingering disorder. Here the young medicus had abundant opportunities of study, of which he fully availed himself. Two years later, succeeding to a legacy by the death of a relative, he again crossed the Border, and resided with a dissenting minister in Northumberland, where he gave himself up to philosophy and general literature. The winter sessions of 1734–5 and 1735–6 were spent in Edinburgh; but Cullen did not graduate in medicine until five years later, at the University of Glasgow. Those years were occupied in general practice in his native town, where the afterwards celebrated Dr. William Hunter (whose theological tendencies were diverted by Cullen) resided with him for awhile as friend and pupil. It is curious that after this relationship between them ceased, these two great men never met but once again, though they continued to the last to maintain an intimate and familiar correspondence. In the autumn of 1744, Dr. Cullen, having been twice elected a borough magistrate, and once married, relinquished his general practice in Hamilton, and settled as a physician in Glasgow. The following extract from a letter of William Hunter to Cullen, in 1745, will be read with interest:

"Well, how does the animal economy appear to you, now that you have examined it, as one may say, with precision? I have good reason to put the question to you, because, in my little attempts that way, since I began to think for myself, Nature, where I am best disposed to mark her, beams so strong upon me, that I am lost in wonder, and count it sacrilege to measure her meanest feature by my largest conception. Ay, ay, the time will come when our pert philosophers will blush to find that they have talked with as little real knowledge, and as peremptorily of the animal powers, as the country miller who balances the powers of Europe." (Vol. i. p. 21.)

It would seem that at this period there were University Professorships to which little or no duties were attached, save the innocent one of receiving fees. Such a state of things but little accorded with the active and enterprising genius of William Cullen. He made arrangements, therefore, to remedy this abuse, and agreed with Dr. Johnstone, Professor of Physic in the Glasgow University, to deliver a course of lectures during the winter of 1746 on the Theory and Practice of Medicine. In the session of 1747, the University authorities having fitted up a laboratory, he also lectured on Materia Medica and Botany, with Dr. Hamilton's assistant. In a few years Dr. Johnstone resigned his sinecure professorship, and Dr. Cullen succeeded him, inaugurating the wise change of delivering his lectures in a living rather than in a dead language. This circumstance, of course, like all innovations upon established customs, gave rise to opposition, and to insinuations of the most unfounded character as to the new Professor's imperfect acquaintance with the Latin tongue.

Among Dr. Cullen's pupils at this period was the afterwards celebrated Dr. Joseph Black, whose "emulous spirit" and acute perceptive faculties were at once recognised by the preceptor, and encouraged in the student. Cullen's own studies and tastes seem to have taken a chemical bias during his association with Black; and he subsequently displays in his correspondence a timid but noble delicacy lest his own progress (for the teachers of chemistry were but then learning it as they taught) should be greater than that of his pupil, and his own discoveries anticipate those of one whose genius he had had such peculiar pleasure in fostering. When Black was afterwards studying in Edinburgh, he frequently communicated literally with Cullen; and the following extract from one of the former's letters illustrates the point above alluded to:

"I received your packet of chemistry, which rejoiced me extremely. A new experiment gives me a new life; but I wonder at the reserve and ceremony you use with respect to me. Did I learn chemistry from you only to be a bar to your inquiries? The subject is not so limited as to be easily exhausted, and your experiments will only advance me so much farther on." (Vol. i. p. 49.)

There can be little doubt that the data from which Black subsequently elaborated his theory of Latent Heat were in a measure furnished by Dr. Cullen's lectures, and from his Essay on Evaporation.

But our Professor's chemical studies were much too comprehensive to be limited only to their bearing upon the science of medicine. His researches, indeed, gave a strong impetus to agricultural and industrial science; and he introduced a few lectures on Agriculture into his chemical course. Does not his correspondence with Lord Kames, upon the subject of manures and the general cultivation of land, show that he was the Liebig of his day?

"Do not forget," says his Lordship in a letter to Cullen, bearing date, Edinburgh, Dec. 26th, 1752, "your letter upon husbandry; having been entertained with no new theory now for a long while, I am sinking into a mere practical farmer. I have not a single new thing at present, except one experiment I am making to convert moss into dung by endeavouring to rot it in a dunghill by mixing it with fresh horse-dung."* "I no sooner get a moment's leisure," he says again, "than I think of my old project for you about agriculture [publishing a work on that subject]. I would not be surprised if you were offered 200*l.* for the copy."

A manuscript composition, entitled 'Reflections on the Principles of Agriculture,' was afterwards found among Cullen's papers, and shows that he had made some progress in the course indicated by his noble friend. There was also a manuscript essay, 'On the Construction and Operation of the Plough,' which had, in all probability, been read before the Philosophical Society in the Glasgow College, and which an intelligent farmer who proposed the substitution of iron for wood, spoke of as the first attempt "to account for the structure of that machine upon mathematical principles." An essay by Dr. Cullen, entitled 'Remarks on Bleaching,' was presented to the Board of Trustees for the Encouragement of Fisheries, Arts, and Manufactures, in Scotland; and it is mentioned in the records of that Institution for June, 1755, that "three suits of table-linen had been given to Dr. William Cullen for his ingenious observations on the art of bleaching."

It should be observed that a few days after Dr. Cullen's admission to the Professoriate in the University of Glasgow, Dr. Adam Smith was appointed by the Faculty to the Chair of Logic; and that on the death of Dr. Craigie, in 1751, the author of the 'Wealth of Nations' was translated to Dr. Craigie's chair—that of Moral Philosophy. By this removal the Professorship of Logic became vacant, and tempted three candidates—two of them of no mean reputation—into the field. They were David Hume, Edmund Burke, and Mr. Clow. The latter gentleman proved successful; but the contest was fraught with unusual interest to Cullen, who warmly supported the cause of the historian, and laid thereby the foundation of an enduring friendship. The following letter, addressed to Dr. Cullen by Hume, after his defeat, is far more interesting than a disquisition on "Brunonianism," and we make no apology for presenting it to our readers:—

"Sir,—The part which you have acted in the late project for my election into your College, gave me so much pleasure, that I should do myself the greatest violence did I not take every opportunity of expressing my most lively sense of it. We have failed, and are thereby deprived of great opportunities of cultivating that friendship which had so happily commenced by your zeal for my interests; but I hope other opportunities will offer, and I assure you that nothing will give me greater pleasure than an intimacy with a person of your merit. You must even allow me to count upon the same privilege of friendship as if I had enjoyed the happiness of a longer correspondence and familiarity with you. For as it is a common observation that the conferring favours on another is the surest way of attaching us to him, I must by this

* See Prize Essays and Transactions of the Highland Society of Scotland, vol. ii. p. 130.

rule consider you as a person to whom my interests can never become altogether indifferent. Whatever the reverend gentlemen may say of my religion, I hope I have as much morality as to retain a grateful sentiment of your favours, and as much sense as to know whose friendship will give greatest honours and advantage to me.

"I am, Sir, your most obedient and most humble servant,

"DAVID HUME.

"Edinburgh, 21st Jan. 1752."

In the autumn of 1755, an opportunity occurred of carrying out a project which had long been urged upon Cullen by his friends, and particularly by Lord Kames, that of removing to Edinburgh. After a tenacity of office (not infrequent in every profession and in every age) long after years and manifold infirmities had incapacitated him for active duty, Dr. Plummer, the Professor of Chemistry, was compelled to admit a partner into his Chair. That partner was Dr. Cullen, who henceforth delivered the lectures on chemistry at the University of "Modern Athens," and was promised the full succession on Plummer's decease. There were circumstances in connexion with this appointment which were highly creditable to Dr. Joseph Black, who for some little time previously had assisted Dr. Plummer, upon the recommendation of Cullen himself, and who might, had he so minded, have turned that position to his own beneficial account. He was the first person, however, to inform his old preceptor of an attack of palsy which had struck down the Professor of Chemistry, and thus rendered necessary the appointment of a working coadjutor in that chair. Great jealousy was created among the Edinburgh doctors by the removal of Cullen to the sphere in which they themselves revolved. His Glasgow success and reputation had run before him, and made them tremble for their professional prospects. They even objected to the professorial appointment, that it had been made without the consent or "demission" of Dr. Plummer, who himself had resolved to oppose it, and even refuse the use of the chemical laboratory, which was his own private property, without a very adequate pecuniary compensation. Ultimately, however, a commission was signed on the 10th of March, 1756, by the Senatus Academicus and the Town Council of Edinburgh, electing Drs. Plummer and Cullen joint-professors of chemistry in the University. Happily, the elder partner in this firm did not long survive. In July following he gave up his chair and his life, and the younger partner succeeded to the full honours and emoluments of the chemical professorship, which he retained for a period of ten years.

The removal to a new place of residence, even under such advantageous circumstances as those which surrounded Cullen, were not unattended with difficulties. Practice did not begin immediately to furnish the exchequer in Edinburgh; and an injury to his leg, which kept the Doctor for some time a prisoner to the house, yet further increased his embarrassments. But he was not the man to be discouraged, and he had laid to heart what his friend, William Hunter, had lately written to him, concluding a letter in the following language :—

"I have found a specific in the author of the Georgics, against the spleen that is occasioned by *Pauperies*, which has been a wonderful palliative cure, at least with me, and has almost reconciled me to charms; as I have no doubt of its efficacy, in every case where there is faith enough, I will communicate it to you:

'Labor omnia vincit
Improbus, et duris urgens in rebus egestas.'"

In order that his time might be fully occupied until he had secured active professional engagements, Dr. Cullen now proposed to himself the translation of Van Swieten's 'Commentaries on Boerhaave's Aphorisms.' But the desiderated practice came on so rapidly after this intention, that the design was abandoned for duties connected with a sphere of more immediate usefulness. Of these, besides the private practice, was the elaborating that system of clinical instruction first commenced by Dr. Rutherford in the year 1748, in the Royal Infirmary of Edinburgh, by a special privilege of its managers. The same privilege was extended to the other university professors, though none appears to have availed himself of it till 1757, when William Cullen was at the bedside with a group of attentive pupils.

"Few (say the editors), if any, traces of the useful and important mode of teaching practical medicine by example are to be found in the early records of physic. Hippocrates, indeed, we are told by Galen, was in the use of conducting his numerous pupils to the bedsides of his patients, for the purpose of instruction; and from a well-known epigram of Martial (which, on account of the warning it contains, ought never to be forgotten by clinical teachers or their pupils), it appears that in his time a similar practice was followed by one at least of the physicians of Rome.

"'Languebam; sed tu comitatus protinus ad me
Venisti centum, Symmache, discipulis.
Centum me tetigere manus, Aquilone gelatæ,
Non habui fibrem, Symmache; nunc habeo.'—Lib. v. Epis. 9.

"Some centuries after the introduction of the Christian religion, the conjunction of hospitals for the reception of the sick poor with medical schools, is said to have been effected in the schools of Alexandria, in Egypt; of Gandasipora, in Persia; and of Edessa, in Mesopotamia; and in the great seminary of learning which flourished at Bagdad, attendance on hospitals is said to have formed a constituent and important part of the system of medical education." (Vol. i. pp. 102–3.)

The practice of clinical instruction commenced in Italy, in the schools of Padua and Pavia, about the middle of the sixteenth century; but for some reason or other, it was soon forbidden or suspended. Later than this—in the beginning of the seventeenth century—it was introduced into Holland, at Utrecht and Leyden. From the latter school, in fact, it came to Edinburgh, through the medium of Dr. Rutherford, who had been a pupil of the illustrious Boerhaave at the literary metropolis of the Dutch Provinces.

It is recorded that Dr. Cullen possessed in a very remarkable degree the qualifications necessary for a clinical teacher. He had great power of observation and description of disease, a real interest in imparting instruction to others, and a degree of candour rarely met with in reference to acknowledgment of diagnostic errors. In one of his lectures he remarks:

"It is not improperly said that the earth hides the faults of the physician. If every patient that dies were opened, as ours has been, it would but too often discover the frivolity of our conjectures and practice. In these lectures, however, I hazard my credit for your instruction, my first views—my conjectures—my projects—my trials—in short, my thoughts—which I may correct, and, if necessary, change; and whenever you yourselves shall be above mistakes, or can find anybody else who is, I shall allow you to rate me as a very inferior person. In the mean time, I think I am no more liable to mistakes than my neighbours, and therefore I shall go on telling you of them when they occur."

Of course the deductive method which Cullen pursued in his investigations rendered him peculiarly prone to error, and its frequent correction by facts which annihilated his theories. Like all the great Scotch writers, he argued from principles to facts, and not from facts to principles.

"His system is constructed by reasoning from general principles; and of that process he certainly was a consummate master. Between the premisses and the conclusion, he hardly ever lets error creep in. And in reference to the results of his speculations, he had one immense merit, which will always secure to him a conspicuous place in the history of pathology. By insisting on the importance of the solids, he, one-sided though he was, corrected the equal one-sidedness of his predecessors; for, with extremely few exceptions, all the best pathologists, from Galen downwards, had erred in ascribing too much to the fluids, and had upheld a purely humoral pathology."*

Such, indeed, and so remarkable was Cullen's success as a clinical teacher and lecturer, that he was regarded by those who best knew his qualifications, as the proper successor to the Chair of Medicine, when its vacation should occur, by the death or resignation of Dr. Rutherford. It should be mentioned, however, that in the days of which we write the theory and the practice of medicine were regarded as so distinct from each other as to require two separate professors. There was, in fact, a chair of theory and a chair of practice. The latter was the one occupied by Dr. Rutherford. A little jobbing and finessing appear to have characterized the proceeding which eventuated in the succession to his post. Dr. Rutherford had imbibed some prejudice against Dr. Cullen, which made him unwilling to resign in favour of that physician. There was a gentleman, however, at Aberdeen, towards whom he had no disfavour; and Dr. John Gregory, of King's College, was summoned from the chair of physic in the northern city to wear the more patrician mantle which was slipping from the shoulders of Dr. Rutherford. A great deal of bad feeling and worse writing obtained in Edinburgh upon this occasion. The warm attachment of Cullen's many friends aroused the malicious onslaught of his jealous enemies, for whom (wrote to him William Hunter) "you will always find contempt a most excellent cordial." In February, 1766, Dr. Gregory was declared the successful candidate. By a singular coincidence, the death of Dr. Whytt a few months afterwards vacated the chair of the *Theory* of Medicine, to which Cullen was at once and unanimously summoned, though he had partaken so freely of his friend

* History of Civilization in England, by Henry Thomas Buckle. Vol. ii. p. 545. London, 1861.

William Hunter's cordial, as to wish to lay himself under no further obligations to the body in whom the appointment was vested. Dr. Joseph Black succeeded to the Professorship of Chemistry. But even now matters were not arranged to universal satisfaction. Dr. Gregory's talents were not unrecognised: but there sprang up a general feeling that the two chairs of physic should be united, and that alternate lectures should be delivered by either Professor on the theory and practice of medicine in combination. This was effected in 1769–70; and, three years later, on the death of Gregory, the author of the "First Lines" reigned solus and supreme as Professor of the Practice of Physic in the University of Edinburgh.*

"Such (it is truly observed) were the difficulties to be overcome, and such the exertions required to procure, first, a place in the University of Edinburgh, and afterwards the proper situation in it, for the man whose genius, talents, and industry shed such a lustre over the institution, and continued in so remarkable a degree to extend and to perpetuate the fame of its medical school!" (Vol. i. p. 161.)

Whatever and how great may have been the merits of his predecessors, it is admitted on all hands that the office which Cullen was now called upon to fill did not lose in usefulness or dignity by his succession thereto. Indeed, it may be affirmed that of the many distinguished professors who had added to the reputation of the Edinburgh school, none had ever surpassed, not to say equalled, the latest occupant of the chair of physic. "To an intimate acquaintance with the theoretical and practical writings of his predecessors and cotemporaries, he was known to unite a vigorous and discriminating understanding, matured by long-continued observation and experience, with the most felicitous powers of exciting the curiosity and directing the pursuits and studies of his pupils." (Vol. i. p. 258.) Though he had been Professor of the Theory of Medicine, Cullen could not but regard the theoretic as something very subordinate to the practical. He never lost sight of this in all his subsequent career. "My general doctrines," he writes, "are only to be so many general facts." "The cure of disease is the ultimate end of all our studies." And yet the theoretical could not be excluded from a deductive mind; and many of the "nosologist's" own theories were sufficiently fanciful, and such as few observers of the present day would be willing to endorse. Moreover, they were often contradictory, especially when he indulged in therapeutic speculations, and elaborated his views upon the 'Methodus Medendi.' Not that we say this at all reproachfully. Theories, like generations, live and die, and retain their vitality for various periods. It must ever be so within the range of sciences which are progressive, and lack the element of mathematical exactness. And we much doubt if contradictions quite equal to any which ever emanated from the pen of Cullen are not to be found in the published works of most medical writers since his day.† But we have small space for theoretic contra-

* It appears that Dr. Cullen, with Dr. Gregory's permission, and in deference to an unmistakeable feeling among the students, delivered one course of lectures on the practice of medicine before the conjunction above alluded to was effected. This was in 1768.

† We may cite the following as very innocent and natural instances of contradiction:

dictions. It will not do for us, in an age which has witnessed such rapid strides in physiology and chemistry, of which we endeavoured to give a biographic culmination in a recent number,* to recur, save for the simple statement of the facts, to any laboured exposition of the doctrines of the animal economy as propounded a century ago.

"All, all are gone, the wild Arabian tale,
Aladdin's lamp, and Sinbad's magic sail."

Our views are somewhat expanded as to that "human system" which Cullen regarded as "a chemical mixt, as a hydraulic machine, and as an animated nervous frame."† The huge accumulations in Dr. Thomson's volumes of theories long since exploded, of disputes long since forgotten, and of fallacies whose reproduction can serve no purpose but that of the reader's embarrassment—how shall we censure them, and yet minister in our small measure to their circulation? It may be sufficient to state that the author of the 'First Lines' was equal to the science of his time, and beyond the general information of his period. By the light which he had he stumbled less than his cotemporaries, and wooed by his earnest and searching spirit the dawn of an expanding future.

In the year 1774, Dr. Cullen, in conjunction with many others who were equally interested in the progress of medical science, drew up a memorial to the Government on the subject of the "shameful traffick of degrees in physic" carried on by the Universities of Aberdeen and St. Andrew's. At the request of the Duke of Buccleuch, who had just been elected an honorary Fellow of the College of Physicians in Edinburgh, then under Cullen's presidency, this memorial was submitted to Dr. Adam Smith, his Grace's friend and quondam travelling companion on the Continent of Europe. The author of the 'Wealth of Nations,' however, was not at all disposed to embrace the views of the memorialists, and in an amusing and ingenious letter to Dr. Cullen he submits his reasons for non-acquiescence in their petition. "Monopoly of medical education" was no more to his taste than monopoly in trade and commerce. He did not mean to defend the signing of a certificate of professional competence of a man of whom nothing, or next to nothing, was known; but he could not see how the public suffered by such a step. "That doctors are sometimes fools, as well as other people, is not, in the present times, one of those profound secrets which is known only to the learned." "Do not all the old women in the country practise physic without exciting murmur or complaint? And if here and there a graduated doctor should be as ignorant as an old woman, where can be the great harm?" Dr. Adam Smith main-

The Doctor writes in his 'Treatise on the Materia Medica': "Whether we can add anything to the powers of life is to me very doubtful. If they are already sufficient for their purposes [the retention of life?], all our attempts to carry them further must be superfluous, and we can have this indication only in cases in which these powers are for the time deficient." "Tone may be considered as a state of the inherent or contractile power of the moving fibres, but at the same time the inherent power is in a very constant dependence upon the animal, and therefore tonics increase the inherent power, by increasing the vigour or continual energy of the animal power." "Stimulants are indicated in all cases of languid motion; that is, not merely in weakness, but in all cases when the motion of the system is inert, weak, and slow."

* *Art.*, Müller and Orfila. † Works, vol. i. p. 409.

tained that a degree granted upon the most rigid examination gave no real security for anything but the science of the examinee. It could not attest his honesty, or his discretion, or his social acquirements, or his capacity for practising that art which, to be successful in the highest sense, exacts largely from these collateral aids. In a Latin discourse delivered about two years afterwards, Dr. Cullen combated the free-trade fallacies of his friend, in respect of the introduction of such principles into the realms and relationships of science. If that "solitary Scotchman (to adopt the language of Mr. Buckle) who by the publication of one single work contributed more towards the happiness of man, than has been effected by the united abilities of all the statesmen and legislators of whom history has preserved an authentic record,"* could now look upon our progressive land, he would recognise the complete triumph of his principles in their legitimate sphere—that of commercial enterprise; but he would at the same time note their downfall, and acknowledge the justice of that downfall, in improved medical legislation, in larger requirements and more searching tests, and in an elevation of character and general attainments, in all who are engaged in assuaging the physical and mental diseases of mankind.

It was not until comparatively late in life that Dr. Cullen's literary career commenced—literary, that is, in the sense of publication. His 'Nosology' was first given to the world in 1769, in which he simplifies the classifications of disease proposed by Sauvages, Linnæus, and Vogel. It ran through four editions in the author's lifetime; the last, containing his latest corrections, and exhibiting his nosological labours in their most finished form, appearing in 1780. Into this later edition was also introduced the classification of Sagar, as embodied in his 'Systema Morborum Systematicum.' The 'Outlines of Physiology' was first published in 1772, and made a considerable sensation at the time of its appearance. It ran through three editions, a translation of the last appearing at Leipsic in 1785, under the title of 'Anfangsgründe der Theoretischen Arzneiwissenschaft.' The 'First Lines of the Practice of Physic,' though contemplated for more than thirty years, did not appear (the first volume) until 1776. The second volume was published in 1779, the third in 1783, and the fourth and last in 1784. A translation of this work into French was brought out by the great alienist physician Pinel in 1785. We cannot undertake to follow the editors through the seventy pages which they devote to the discussion of the first of the above-named works. A pupil of Cullen's at Edinburgh, who afterwards attained a great reputation as an able physician (Dr. Currie), has thus recorded his estimate of his preceptor's nosological labours:

"With more comprehensive views than Sauvages, a more lucid order, and a happier simplicity, Dr. Cullen divided the whole body of diseases into four classes and twenty orders. In his definitions, he excels in accuracy all who have gone before him; and it is, indeed, his distinguished and peculiar praise, that, not only in his 'Nosology,' but in his 'First Lines,' his descriptions of diseases receive no colouring from his theories, but are everywhere faithful to nature.' (Vol. ii. p. 75.)

* History of Civilization in England, vol. i. p. 197. London, 1859.

A later physician and physiologist (Dr. Wilson Philip) thus writes:

"It is not Dr. Cullen's praise that he invented an ingenious hypothesis, but that he new-modelled, if I may use the expression, the whole practice of medicine; that in his 'Synopsis Nosologiæ Methodicæ' he defined and arranged diseases with an accuracy that is still unrivalled; and in his 'First Lines' reduced their treatment to a degree of simplicity formerly unknown." (Vol. ii. p. 134.)

Speaking of the 'Nosology,' a French writer observes, "Celle de Cullen, qui parut en 1772, et qui *constitue un véritable progrès*."*

Perhaps no better specimen could be given of Dr. Cullen's descriptive power and accurate observation than "the diagnosis drawn by his masterly pen" of hypochondriasis and melancholia. Dr. Bucknill has introduced it into his able work (with Dr. Tuke) on 'Psychological Medicine,' and speaks of its author as one "than whom no medical writer has ever been more accurate and logical in his discrimination of disease, according to the light which he possessed."

"Hypochondriasis I would consider as being always attended with dyspeptic symptoms; and though there may be, at the same time, an anxious melancholic fear arising from the feeling of these symptoms, yet while this fear is only a mistaken judgment with respect to the state of the person's own health, and to the danger to be from thence apprehended, I would still consider the disease as a hypochondriasis, and as distinct from the proper melancholia; but when an anxious fear and despondency arise from a mistaken judgment with respect to other circumstances than those of health, and more especially when the person is at the same time without any dyspeptic symptoms, every one will readily allow this to be a disease widely different both from dyspepsia and hypochondriasis, and it is what I would strictly name melancholia. In this there seems little difficulty; but as an exquisitely melancholic temperament may induce a torpor, and slowness in the action of the stomach, so it generally produces some dyspeptic symptoms, and from thence there may be some difficulty in distinguishing such a case from hypochondriasis; but I would maintain, however, that when the characters of the temperament are strongly marked, and more particularly when the false judgment turns upon other subjects than that of health; or when, *though relative to the person's own body, it is of a groundless and absurd kind;* then, notwithstanding the appearance of some dyspeptic symptoms, the case is still to be considered as that of a melancholia, rather than a hypochondriasis."

Nor is Dr. Cullen's descriptive power less evident in his handling of febrile phenomena. Indeed we are prepared to endorse unhesitatingly the opinions of the editors when they affirm:

"With whatever defects Dr. Cullen's general definition of his class Pyrexiæ or of his order Febres may be conceived to be chargeable, every well-informed and unprejudiced mind will allow that the history of the phenomena and progress of a febrile paroxysm, as given by him from the sixteenth to the twenty-third paragraphs of his 'First Lines,' is, both in respect of the minute fidelity of the delineation and of the clearness and conciseness of the language in which it is portrayed, a perfect masterpiece and model of medical description." (Vol. ii. p. 107.)

It is but too well known to the profession how abundantly fertile the subject of fever has proved in theoretic disputations; and it is but

* Renouard: Histoire de la Médecine, vol. ii. p. 231. Paris, 1846.

too well known to ourselves how largely the editors have availed themselves of the fact for purposes of digression. After all, we are not prepared to state that the question is not now as open a one as in the days when Cullen wrote and Armstrong blooded. The occurrence of local congestions is a matter beyond dispute, but whether they are dependent upon that "constriction of the extreme vessels" which prevented free capillary circulation, and so threw back the vital tide upon the larger channels and the viscera, is not ascertained with any certainty or precision. One thing, however, is certain—that Cullen regarded fever as essentially a disease of debility; and that for a consistent treatment founded thereupon he had the misfortune to incur the censure of Dr. Welsh, for "having succeeded in nearly interdicting the use of blood-letting in fever." In the forty-sixth paragraph of his 'First Lines' he writes:

"Upon the whole, our doctrine of fever is explicitly this: The remote causes are certain sedative powers applied to the nervous system, which, diminishing the energy of the brain, thereby produce a debility in the whole of the functions, and particularly in the action of the extreme vessels."

Dr. Mason Good is of the same opinion, and speaks of fever as "characterized by debility of the living fibre."

These views have been extensively assailed, both by the pen and by the lancet, by those from whose special experience we naturally should have expected the soundest views and the most judicious treatment. And to bring the phases and the fashions of this treatment down to our own day, we will but allude to the startling reaction which followed—which always follows—extreme opinions. The apostle of that reactionary movement has been taken away prematurely, and the doctrine which has been named "Toddism" (query—*Toddyism*) is now in its turn receiving no measured condemnation. As an experimental account of the chemical effects of alcohol upon the human economy has so recently appeared in this journal, we shall not now enter into a discussion which demands at once the most cautious judgment and the most abundant charity. We have heard the late Dr. Todd spoken of in terms of the greatest harshness and injustice by men who possess not a tithe of the knowledge or capacity which were his. But Cullen was so treated. Every great and original mind must be prepared for obloquy. "L'ambition c'est une candidature au martyre." For ourselves, we shall only venture to affirm in reference to the subject of fever and its treatment, that whatever theory of morbid action or of the right means of encountering its effects may obtain at this or at any period, we are persuaded that no recent writers have known more than Cullen knew; and certainly they have not succeeded in imparting their knowledge to their generation in language half so perspicuous as did their illustrious predecessor, when he wrote:

"This theory of fever amounts to no more than saying that there are certain states of the body which are combined together in a certain order of succession, and that for this constant combination they are to be considered as a series of causes and effects. This I assume as a fact, and I suppose that this connexion is determined by a certain mechanism or organization of the body, but I do not pretend to explain the nature of this. I might offer conjectures upon this subject, but while they are only conjectures, I shall not trouble you with them, so shall go no further."

But to turn to a less limited field, we may remark that there is abundant evidence in the writings of Dr. Cullen of his singular accuracy of observation. A physician of our own day, whose path has been sunned by a brighter scientific light, whose experience has been immense, and whose felicity of diction has caused his lectures to be regarded as the very perfection of medical literature, fully confirms Dr. Cullen in his views of the frequent connexion between hæmoptysis and phthisis pulmonalis, and of their league with what we term a scrofulous diathesis.*

Nor is the "Nosologist's" accuracy less remarkable in a disease (so to speak) of a less objective character—mellitic diabetes. For comparatively little was known in the last century of animal chemistry and of the morbid anatomy of the kidneys. And yet Cullen and Prout coincide in stating that the undue prevalence of the saccharine element in this disease is dependent chiefly upon defective assimilating processes in the stomach, though the later physician does not entirely separate diabetes from structural changes in the organs which secrete the urine. In short, there is reason to suppose that Cullen was not only a very accomplished physician, but a very safe and judicious practitioner. We can only discover one of the accidents of the life terrestrial in which, imbibing a fallacy now exploded, he must have expedited the translation of mortality to celestial regions. It was his custom to recommend the injection of tobacco-smoke into the intestinal canal in cases of drowning, the object being "to restore the action of the moving fibres." A letter, by the way, from Dr. Cullen to Lord Cathcart, on "The Recovery of Apparently Drowned Persons," if not the cause of, was at least strictly coeval with the foundation of our Royal Humane Society, which was instituted in London in 1774, in imitation of similar societies in Amsterdam, Milan, Venice, Hamburg, and Paris.

We have now arrived at that period in the life of Cullen when active duties cease and friends and companions are one by one removed; and there remains but to the good physician to

"Await alike the inevitable hour."

David Hume and William Hunter have both gone away into the untried country from which there is no return. The philosopher and historian died in August, 1776, and Cullen's account of the cheerfulness and serenity (which he himself personally witnessed) of the closing scene, as conveyed in a letter to William Hunter, is one of the most interesting records in these volumes. We shall give our readers its penultimate paragraph:

"These are a few particulars, which may perhaps appear trifling, but to me no particulars seem trifling that relate to so great a man. It is perhaps from

* See Dr. Watson's Lectures on the Principles and Practice of Medicine.

trifles that we can best distinguish the tranquillity and cheerfulness of the philosopher, at the time when the most part of mankind are under disquiet, anxiety, and sometimes even horror. I consider the sacrifice of the cock as a more certain evidence of the tranquillity of Socrates than his 'Discourse on Immortality.'" (Vol. i. p. 609.)

Seven years later (in March, 1783), William Hunter also surrendered to the rigorous besieger of the citadel of life. The foundations had long been sapped by hard work and "wandering gout," and at the age of sixty-five the garrison capitulated. But a yet greater calamity was in store for Cullen. In 1786, Mrs. Cullen expired at Ormiston Hill, the country seat which he had purchased sixteen years previously. Before this event, it would appear that the physician had shown some symptoms of depression, which the editors are not backward in attributing to the old enemy, "Dr. John Brown," and that "Brunonian" business to which we have before alluded. But these symptoms were clearly the natural failings of age, to which all men are obnoxious. It is not to be supposed that these indications of decline were lessened by his bereavement. But in the society of his daughter, the pursuits of his garden and his farm, and the composition of his last work—a 'Treatise of the Materia Medica'—he found no little measure of consolation. Of a truth, this is more than we have ourselves been able to discover, in the hundred pages devoted by the editors to the *ante* and *post* literature of this subject—pages which are most wearisome and unnecessary.

Dr. Cullen appears to have manifested considerable reluctance to resign his professorship in the University—a reluctance which perhaps he inherited from his predecessor, and which history teaches us is peculiarly susceptible of transmission. He did so, however, in 1789, and the patrons paid him the compliment of re-electing him, and appointing an "assistant and successor" to do the work of which he had really for some time been incapable. But Dr. James Gregory only acted in the former capacity for two months, for William Cullen died on the 5th of February, 1790, having attained the patriarchal age of eighty years.

Of his seven sons and four daughters, the third son, Archibald, who graduated M.D. at Edinburgh, and afterwards went to the English Chancery Bar, has proved the channel for direct lineal descent. His eldest son is now a Lieut.-General in H.M. Indian army, and Political Resident at Travancore, in the Madras Presidency. We trust that the name will not become extinct with him, and that though he has already exceeded the Psalmist's limitation, he may long survive that rigorous climate. It is a charity, likewise, to express a wish that his constitution may not be unduly depressed by the perusal of the volumes, from which we have managed with some difficulty to extract a continuous narrative of his grandfather's career.

In conclusion, we regret that we are unable to speak in higher terms of a work upon which it is evident there has been bestowed a great deal of care and attention, and whose publication in its now complete form is the result of labours extending over many years. These circumstances, however, do but make the case more distressing, and force from us the avowal that we would gladly exchange half the learning for twice the discretion here made manifest. In other words, we should have preferred a readable and compendious memoir to an elaborately discursive and pretentious treatise. If there is a man of all others on our ancestral roll of whom a well-constructed and pleasant narrative would have been welcome, that man is William Cullen; for he possessed in an eminent degree that correctness of principle, and sobriety of judgment, and freedom from ostentation, which have made reverential esteem for his memory a sort of professional tradition. His biography, in an available form, would be one of the healthiest things to put into the hands of those just initiating their medical studies, as well as of those who for years have borne the burden and heat of medical labour. In its present shape, however, it would weigh down the most ardent disciple of Æsculapius, and alarm him at the very portals of special literature; for the fact is not to be disguised, that the bulky tomes which we have now reviewed are a huge mistake, and these united biographies a ponderous infliction.

REVIEW V.

On Diseases peculiar to Women; including Displacements of the Uterus. By Dr. HODGE.—*Philadelphia,* 1860. pp. 469.

ANOTHER work on the diseases of women, of royal dimensions from unroyal America! Our friends across the Atlantic do not appear yet to have learned that brevity is the soul of wit, or that conciseness in writing is more practically advantageous than a multiplication of lines and pages. So, in the present instance, Dr. Hodge has entailed upon his readers the labour of wading through a large book for information that might have been much more usefully imparted in a volume a quarter the size. We do not mean to say that the book does not contain many very practical and useful observations; but the mass of uninteresting material within which they are enclosed will probably be a hindrance to their free circulation, especially amongst a profession whose members are notoriously disinclined to any unnecessary amount of reading. Dr. Hodge has divided his book into three parts, each containing several chapters. The first part comprises Diseases of *Irritation;* the second, *Displacements* of the Uterus; and the third, Diseases of *Sedation.* His opinion is, that the diseases peculiar to women may be traced to the effects of irritation and of sedation in the different tissues and organs of the economy, especially of the uterine system; and he lays great stress upon the important part which displacements of the uterus play in their production, devoting no less than nine chapters to the consideration of the various displacements of the uterus and their causes, symptoms, and treatment. We are ourselves impressed with the importance of such displacements as causes of much general disorder of the health, and are convinced that they deserve more attention than, for various reasons, professional men have generally accorded them. They are, we are fully persuaded, frequently the obscure causes of many mental and bodily disorders, the source of much moral infirmity, and, in conjunction with constipation, the most fruitful exciters of hysterical disease. We fully concur in the following remarks of our author on irritable vulva and vagina. He says:

"By far the most frequent cause is uterine displacement, which, besides rendering the uterus sensitive, directly and indirectly affects the vagina, partly by the sympathetic extension of the symptoms of nervous irritation from the womb, and partly by the pressure, dragging, and similar sources of irritation from a prolapsed, retroverted, or retroflexed uterus." (p. 135.)

We may also add our belief that much of the vice of secret self-excitement is due to these causes, and may be remedied by replacing the uterus in its normal position, at the same time warning the subjects of them, with kindness and considerate argument, against the great evils to be apprehended by pursuing such practices, of which evils the unhappy patients themselves may have been entirely ignorant. No doubt so delicate a subject requires very delicate management; but a well-regulated mind will find out how best to deal with it; and why, we would ask, should we hesitate to try to obviate the consequences in the one sex more than in the other?

By the term *irritability*, our author simply means a capability of receiving impressions from surrounding agents, and thus producing phenomena. It is the "capability of being acted upon." The agents which excite or disturb the irritability of the tissues are termed "*irritants*." Those which depress or diminish its intensity are called "*sedatives*." An irritant, acting on the irritability of a part, produces an "*irritation*." A sedative causes a state of "*sedation*." The one is an exaltation, the other a diminution of vital excitements. One great object of the present work is to maintain that, in many cases, the independence of nervous diseases is complete; and also that, when complications exist demanding therapeutical assistance, the neurotic affection is often primary and of essential importance, and demands the chief attention of the practitioner. The following remarks deserve attention, referring as they do to a point which we believe to be more frequently overlooked than regarded, notwithstanding its almost vital importance in the treatment of what is called nervousness:

"These neurotic complaints are *physical;* they are in every way as much real diseases as those of organic life. They are irritations of the medullary matter of the brain, of the spinal cord, and of their radiations, the nerves; as truly as organic diseases, inflammations, fevers, &c., are irritations of the heart, arteries, capillaries, and organic cells. Since, therefore, the cerebrum is the organ of the mind, as well as of the body, and mental and moral causes may give origin to nervous diseases, and since remedies of an intellectual and moral character contribute to recovery in nervous affections; it is unscientific, as as well as unjust and cruel, to maintain that such diseases are mental or moral, a perversion of the 'intellect or the heart,' that they are 'imaginary,' a 'mere notion,' that the patient is 'affected,' wishes to be 'interesting,' to attract 'attention,' 'has a bad temper,' and that she would recover 'if she made the effort,' if she were 'forced to work,' and such other uncharitable suggestions. Let it be remembered that the business of the physician is with the physical being; and although he is often driven to the domains of the mental and of the moral philosopher to detect the causes, or to furnish the remedial agents of nervous diseases, yet he should be slow to admit that his agonized patient suffers from the state of her mind rather than of her body, or to report to friends and relations that his would-be patient is well when her consciousness tells her that she is sick. More correct mental, as well as physical science was exhibited by a distinguished theologian, who declared that cases of religious melancholy were by him always transferred to the physician." (p. 22.)

Our author, although evidently a practical man, is nevertheless very fond of theory; the readers of his book will probably, some of them, be inclined to break a lance with him in respect to several of his conclusions, and we shall leave it to the combatants to fight it out between themselves. We will, however, produce a specimen, and that not a bad one, of his reasoning with respect to a very interesting subject—namely, the distinction between "*inflammatory and simple congestions*:"

"The *inflammatory* form is a disease essentially of the ganglionic nervous system of organic life. It arises from an irritant acting on the organic irritability, and causes, therefore, an organic irritation, which is more or less speedily followed by a congestion, first and chiefly of the capillary vessels, and subsequently of the larger vessels. It begins at spots or points where the irritation exists most intensely, although diffused to other parts, and is followed, if the cause continue, by morbid secretions, alteration and destruction of tissues. The *simple* congestion is an affection of the cerebro-spinal nervous system of the organs of animal life. It is caused by peculiar irritants, acting on the animal irritability, producing therefore animal irritation. This peculiar state of irritation of the cerebral system is not always followed by congestion; terrible indications of its existence are often observed without any, at least appreciable, evidences of congestion. Even when congestion is present, it is so moderate and so secondary, that it is often neglected in therapeutics. When congestion does occur, it moves with wonderful rapidity, involving at once large and small vessels, the whole of an organ as speedily as a part, the adjacent tissues as well as those immediately concerned. How rapidly will a transient thought mantle the face, neck, and breast of a modest young woman with a scarlet blush! How suddenly will a cerebral irritation be sometimes followed by an apoplectic congestion, convulsions, and death! How instantaneously will the thought of a beloved infant send the arterial current to the lachrymal gland and its appendages, for the effusion of tears of joy or sorrow; or to the mammary tissues, for the production of food for a dependent babe! Such congestions are so rapid that, in common language, we speak of a rush of blood, of an afflux, or sudden determination, of engorgement, &c. Great as these congestions may be, profuse as are the secretions or the effusions which may follow, still the discharges are not morbid in their character, and we observe no disorganization. The integrity of the organ remains perfect, unless some mechanical injury ensues from the effusion. A compression or laceration of the brain, for instance, may, of course, result when blood is poured into its substance or cavities. In all these respects the distinctions are positive, and bear most decidedly upon the indication for treatment, and on the choice of therapeutical agents." (pp. 33, 34.)

Leucorrhœa, our author, contrary to the opinion of many pathologists, considers to be far more frequently uterine than vaginal, and also functional than inflammatory. That this is often the case, we have had opportunities of ascertaining by the use of the speculum, when we have seen the discharge issuing freely from the mouth of the uterus. Irritability he declares to be the proximate or essential cause of *Dysmenorrhœa*. When this exists, any normal or abnormal excitant at the time of menstruation will produce immediate suffering, neuralgic or spasmodic; when severe, it is accompanied with undue congestion, which may be followed by free discharges—menstrual, hæmorrhagic, or even by the formation and extension of membranous matters. *Amenorrhœa* he names Sedation of the Uterus, and considers it, in all cases, a *sign*, a *symptom*, of abnormal states of the uterus, of the ovaries, of the secretory organs, or of the general nervous and vascular systems. It is not, therefore, amenable to specific remedies—"emmenagogues," as they are called—to re-excite the uterus; and almost every practitioner, elevated above the level of the empiric, has found himself compelled to institute a general course of treatment, having no direct, but merely an indirect and secondary influence upon the pelvic viscera. (p. 420.)

We have before observed that a large portion of the book is devoted to *displacements* of the uterus, their various forms being accurately described and exemplified by diagrams. The treatment recommended consists principally of artificial support by means of pessaries, every form of which is described and commented upon, with the result generally of condemning them as inefficient, but particularly recommending what he calls "lever pessaries," a modification of the ring pessary invented by himself. We are fully convinced of the truth of most of his observations on the nature, symptoms, and consequences of uterine displacements, and think them deserving of very great attention; but as to the treatment, we feel sure in many cases no form of pessary whatever can be trusted, or even employed, and that often considerable relief may be obtained by occasionally adjusting and rectifying the position of the womb by manual interference, and guarding the patients against whatever may be likely to reproduce displacement. We have seen several extreme cases relieved by these means, when the symptoms were of a nature to render the unfortunate sufferer unable to mix with society, attend to the ordinary social duties of life, or even to leave her bed; and under certain circumstances we place more confidence in these occasional adjustments, coupled with rest, the local application of cold water, and such treatment as may improve the general health, than to any more permanent form of mechanical support; for it is just in such cases as are attended with the greatest amount of general distress and nervous irritability that the mechanical effects of pessaries are unbearable, aggravating rather than relieving the disorder they were intended to remove.

We take our leave of Dr. Hodge's work, without entering further into the various subjects of which it treats, except to remark, that with respect to uterine tumours—which are now exciting a good deal of interest in this country—he expresses himself very strongly against attempting to remove them by operation. Believing the symptoms resulting from these growths to arise, not from their nature, but from pressure on the pelvic tissues, he considers that vaginal supports are very generally not only adequate to their relief, even during a long life, but are far more certain and safe than a resort to such dangerous operations as have been proposed for their removal. (p. 375.)

REVIEW VI.

1. *Chemistry for Schools.* By the late DIONYSIUS LARDNER, D.C.L. —*London*, 1859.
2. *The First Step in Chemistry.* By ROBERT GALLOWAY, Professor of Practical Chemistry in the Museum of Irish Industry. Third Edition.—*London.*
3. *A Manual of Qualitative Analysis.* By R. GALLOWAY. Third Edition.—*London*, 1861.
4. *Chemical Recreations; a Popular Manual of Experimental Chemistry.* By J. J. GRIFFIN, F.C.S. Tenth Edition.—*London*, 1860.
5. *An Introduction to Practical Chemistry, including Analysis.* By JOHN E. BOWMAN, F.C.S., late Professor of Practical Chemistry in King's College, London. Edited by C. L. BLOXAM. Third Edition.—*London*, 1858.
6. *Handbook of Chemistry, Theoretical, Practical, and Technical.* By F. A. ABEL, F.C.S., Chemist to the Board of Ordnance; and C. L. BLOXAM, Professor of Practical Chemistry in King's College. Second Edition.—*London*, 1858.
7. *A Handbook of Organic Chemistry for the Use of Students.* By WILLIAM GREGORY, M.D., F.R.S.E., late Professor of Chemistry in the University of Edinburgh. Fourth Edition. pp. 627.—*London*, 1856.
8. *A Handbook of Inorganic Chemistry, for the Use of Students.* By the same Author. Fourth Edition.—*London*, 1857.
9. *Manual of Chemistry.* By the late GEORGE FOWNES, F.R.S. Edited by H. BENCE JONES, M.D., F.R.S., and A. W. HOFMANN, LL.D., F.R.S. Eighth Edition.—*London*, 1861.
10. *Elements of Chemistry, Theoretical and Practical.* In Three Parts. Part I. *Chemical Physics.* Part II. *Inorganic Chemistry.* Part III. *Organic Chemistry.* By W. A. MILLER, M.D., V.P.R.S., V.P.C.S., Professor of Chemistry in King's College, London.—*London.*

The Same. Second Edition of Parts I. and II.

11. *Lehrbuch der Organischen Chemie, oder der Chemie der Kohlenstoffverbindungen.* Von Dr. AUG. KEKULÉ. Erste und Zweite Lieferungen.—*Erlangen*, 1859–1860.

Text-Book of Organic Chemistry, or the Chemistry of Carbon-Compounds. By Dr. A. KEKULÉ, Professor of Chemistry in the University of Ghent. Parts I. and II.

12. *Leçons de Chimie Générale Elémentaire.* Par M. AUGUSTE CAHOURS.—*Paris*, 1856.

Instruction in General Elementary Chemistry. By A. CAHOURS.

13. *A Manual of Chemical Analysis, for the Use of Schools.* By C. GREVILLE WILLIAMS, late Principal Assistant in the University Laboratories of Edinburgh and Glasgow.—*London*, 1858.

14. *A Handbook of Chemical Manipulation.* By C. GREVILLE WILLIAMS.—*London*, 1857.

15. *A Handbook of Chemical Analysis, adapted to the Unitary Notation.* Based on the Fourth Edition of H. WILL'S 'Anleitung zur Chemischen Analyse.' By F. T. CONINGTON.—*London*, 1858.

16. *Analytical Tables for the Use of Students of Practical Chemistry.* By MURRAY THOMSON, M.D., F.C.S.—*Edinburgh*, 1861.

17. *A Manual of Qualitative Chemical Analysis.* By A. BEAUCHAMP NORTHCOTE, F.C.S.; and ARTHUR H. CHURCH, F.C.S.—*London*, 1858.

18. *A Set of Labels for Reagents; and a Table of Atomic Weights, according to the Unitary Notation.* Arranged by ARTHUR H. CHURCH, B.A., F.C.S.—*Oxford and London*, 1859.

19. *A System of Instruction in Qualitative Chemical Analysis.* By Dr. C. REMIGIUS FRESENIUS, Director of the Laboratory at Wiesbaden. Fifth Edition. Edited by J. LLOYD BULLOCK, F.C.S.—*London*, 1859.

20. *A System of Instruction in Quantitative Chemical Analysis.* By Dr. C. R. FRESENIUS. Third Edition. Edited by J. LLOYD BULLOCK.—*London*, 1860.

21. *Traité Complet de Chimie Analytique.* Par M. HENRI ROSE. Edition Française Originale.—*Paris.*

A Complete Treatise on Analytical Chemistry. By HEINRICH ROSE. A New Edition.

22. *Handbook of Inorganic Analysis.* By FRIEDRICH WÖHLER, Professor of Chemistry in the University of Göttingen. Edited by A. W. HOFMANN, Ph.D., F.R.S.—*London*, 1854.

23. *Handbook of Organic Analysis.* By JUSTUS LIEBIG, Professor of Chemistry in the University of Munich. Edited by A. W. HOFMANN.—*London*, 1858.

WHAT text-book shall I use as the basis of my lectures? what manual of general chemistry shall I advise my pupils to read? what system of instruction in analysis shall I adopt in my laboratory? We may presume that every chemical teacher proposes to himself such questions as these before beginning his active duties; perhaps, however, a ready solution, possibly more advantageous to the professor than the student, will suggest itself should the teacher be himself the author of a chemical treatise. While giving such a summary of the contents of the numerous works named at the head of this article as shall suffice to show how far they answer the purposes for which they were written, or for which they may be wanted, we shall endeavour to answer the questions to which we have alluded by describing in outline a complete scheme of practical instruction in chemistry. Our list might, indeed, have been of double its present size, had not the limits of the space at our disposal precluded such enlargement, while the inferiority of many of the omitted works rendered it unnecessary.

Whatever may be the ways in which chemical knowledge is to be applied in after-life, all students of the science must acquaint themselves with its main facts by the same course of experiment, observation, and study. The first step will be the consideration of the simplest and most general laws of chemistry; to begin by an examination of its minutiæ, and its application to any of the arts is a mistake, the bad effects of which subsequent remedies seldom succeed in curing completely.

In a science like chemistry, which necessitates a constant appeal to the senses, it is important that these should be so trained as to appreciate the value of experimental evidence, and the relations of theory to practice, and of facts to phenomena. Continued sight of apparatus and of the performance of diverse operations by more advanced workers, will assist the progress of the beginner most materially at first. Success is here in great measure dependent upon the thoughtful exercise of the imitative faculty, and thus the course of instruction in a public laboratory presents in some respects an advantage over that of a private school of chemistry. Anyhow, the personal superintendence of the teacher will require to be supplemented by book instructions, to which reference can be made for further directions and details.

The precision and exactitude of the facts of chemistry does not preclude variations in the mode of teaching this science. Some instructors lay special stress on one characteristic of a plan of teaching, and some upon another—in either case neglecting or disparaging other features of no less importance. Some consider that tables presenting at a glance perfectly methodical schemes of recognising by successive steps the various members of what is termed in analytical language a *group*, are to the last degree injurious to the sound progress of the learner. These writers will even go so far as to entertain a doubt whether the student should tabulate his own results during the course of an analysis, but they do admit, though with evident reluctance, that he may be allowed to make a record of his experiments. With reference to the supposed disadvantages of tables, the English translator of Dr. Fresenius's work on qualitative analysis says that they "serve no really good purpose; they rather, on the contrary, supply but very superficial information, and satisfy the student before they have really informed him." There is no doubt an element of truth in this sweeping assertion, for the directions in tables may certainly be followed in a merely mechanical way, without the exercise of memory or judgment. But it is in fact essentially impossible to represent the processes of analysis except in tabular statements. As, therefore, the methods of analysis are tabular in reality, they may as well be represented so in appearance also. But in Fresenius's book, each table, instead of being methodically concentrated on one page, is spread in detailed fulness over three or four. No teacher who has had any practical experience of the errors into which students continually fall, of their wants, and of the ways in which these may best be satisfied, would put into their hands a volume of tables, without a course of careful preparatory training, nor would he commence a course of practical laboratory instruction by offering his pupils for analysis substances, however simple, the composition of which was not disclosed to them. The duties of a laboratory often fall chiefly upon the professor's assistant, and we have heard of cases where the assistant not having himself had the necessary training in a well-organized public school of chemistry, has actually given pupils an analysis to make before they have prepared a common gas, or made themselves acquainted with the appearance and properties of the more usual elements, or studied the reactions of the more frequently occurring bases and acids.

The plan of a really efficient course of chemical instruction may be traced in a few words. A preliminary acquaintance is first to be made with the chemical as distinguished from the physical properties of matter; with the modern idea of simple and compound bodies; with the meaning of the terms cohesion and affinity, and with the circumstances that modify these phenomena; with the different conditions of matter, and with the use of the expressions atoms, molecules, and atomic weights or equivalents. Hence the learner will be led to the study of symbols and their uses when combined into formulæ for expressing the constitution and relations of different chemical substances. From formulæ by which chemical existences are signified, he will proceed to the consideration of equations by which chemical changes are represented. The terminology of the science will next claim the student's attention; he will learn the meaning of such terms as *sulphide, sulphite, sulphate,* and the relation of the bodies thus designated to their originating acids. A lecturer having an adequate knowledge of the subject will be able to make all the above points clear to the beginner, who would do well afterwards to refresh his memory by a careful study of the opening chapters of some elementary treatise on chemistry. In fact, the importance to the student, especially in the early part of his career, of attendance on a continuous course of lectures, can scarcely be over-estimated.

The student now commences practical work in a laboratory, some such plan as the following being pursued. An acquaintance should at first be made with all the more common elements, so as to enable the student to recognise them when they occur in subsequent experiments. Under the personal superintendence of a teacher, he will then prepare, collect, and experiment with various gases — hydrogen, nitrogen, oxygen, carbonic acid and ammonia, for instance. This done, several simple salts are next given him, both in the dry form and in solution; he is told what they are, and has merely to apply to them in regular order, and that order invariably the same, all the tests mentioned in his manual of analysis, in the sections on chemical reactions. Thus he identifies each substance by direct experiment with the description which he has previously read of it. Thus he makes himself master of those reactions upon which the recognition and separation of bases and acids are founded. After he has studied a group, he may perhaps be called upon to exercise his recently acquired knowledge in detecting one or more members of that group, but this is generally deferred to a later period, when he has become acquainted with the leading reactions of all the groups or subdivisions. The student is now offered substances for investigation, their nature not being disclosed to him. At first they contain no more than one basic and one acid radical. He takes a small portion of the substance in a dry condition, and submits it to a preliminary examination, chiefly with the aid of the blowpipe. But the student pursues his analysis further, operating also in the *wet way*. In tabulating the results which ensue from the application of tests in orderly sequence, he reserves for each division of each step an exactly corresponding place, never putting the *residue* or *precipitate* in that corner which the *filtrate* or *solution* ought to occupy. Beginning with the simplest, he proceeds step by step to the analysis of the most complicated mixtures, the more difficult operations of quantitative and organic analysis completing his course.

Throughout his course, the student cannot be too deeply impressed with the absolute necessity of applying chemical reagents or tests in a definite order. The difference between group tests and special tests must be clearly seen, and the rash and premature use of the latter during the course of an analysis carefully avoided. We quote some remarks on this subject from Messrs. Northcote and Church's 'Manual of Analysis' (p. 388):

"The student cannot be warned too early of the extreme folly of what may be termed analytical *angling*—of the promiscuous employment of tests which, when properly applied, are extremely effective in detecting individual substances. Complicated results may thus ensue, to unravel which may baffle all the ingenuity of the student; many substances, too, may thus be entirely overlooked, in consequence of the special test employed acting upon bodies other than the one sought for, and in a way not remembered. To take an instance of frequent occurrence in the laboratory. The student has a solution to analyse, the colour of which is green. He instantly concludes that the base is copper, and instead of employing the ordinary sequence of group tests, he devises a short and easy method. Knowing that hydrate of ammonium gives a characteristic reaction with copper salts, he adds it; a green precipitate is formed, and then re-dissolved; but the solution does not present the deep blue colour of cuprammonium salts. Thus he is disappointed, but still, under the impression that he is dealing with a copper salt, he tries the action of hydrate of potassium; a green precipitate occurs, somewhat pale, it is true, but the student nevertheless regards it as confirmatory of his original supposition. Ferrocyanide of potassium is next added, and the green precipitate which follows is a new perplexity. In despair at these results, so conflicting and so contradictory of his original idea, he adds sulphide of ammonium; the black sulphide formed confirms his first supposition; the doubts consequent upon the previous reaction clear away, and he definitely pronounces the solution to contain copper. Why has nickel been thus obviously mistaken for copper? Because the experimenter, in defiance of the conflicting evidence which the special tests have afforded, has entirely neglected all proper sequence of experiment, and omitted to apply the test which claims precedence of all—the group reagent, hydrosulphuric acid in an acid solution, by the employment of which he would at once have been able to decide the question about which so much time has been wasted."

This is not the place to describe the way in which a laboratory of instruction should be arranged, and this, in fact, could not be properly done without the aid of numerous illustrations; but we may refer to one or two important points in the conduct of a chemical school. Of these, the most essential is, that the teacher who has charge of the public laboratory should visit every student once or oftener each day, in order to examine the tabulated results of his experiments, and to point out how far his conclusions are borne out by the premisses on which they are based. Where this regular visitation is neglected, the progress of the students cannot be satisfactory and certain. We have before mentioned the propriety of initiating the learner into the more common processes of chemical manipulation on his first introduction to practical work. Where the number of pupils is considerable, these processes may be performed by them in a class, the demonstrator having first of all performed the experiments himself, shown them in all their details, and in every stage of preparation. Even such apparently simple operations as boring corks, bending glass tubes, and measuring gases, are not so easily performed by the beginner as to render instructions unnecessary. Each complete set of apparatus may be shared by two students. Such practical classes have been very successful in King's College, London, Mr. Bowman's work serving as an efficient text-book; and have been largely adopted elsewhere, as at the University laboratory of Oxford. In the latter case, they have been arranged to correspond generally with the professor's lectures, so that the chief experiments made in each lecture shall be repeated by the students themselves in the practical class meeting next after. The following quotation from a prospectus of this class for elementary practical instruction may serve to give some idea of its scope:

"The subjects treated of during the term will be as follows:
"Manipulation of gases insoluble (or nearly so) in water.
"Preparation, collection, and properties of hydrogen and of oxygen.
"Synthesis of water and the reduction of metallic oxides.
"Preparation of nitrogen from the atmosphere and from chemical compounds.
"Method of determining the densities of gases: use of the balance.
"Manipulation of gases soluble in water.
"Methods of collecting soluble gases. Preparation of ammonia, of chlorine, and of hydrochloric acid; difference in the density of these gases.
"Collection by displacement. Use of the mercurial trough.
"Estimation of gases.
"Estimation by weight.
"Estimation by volume, and method for the accurate measurement of gases.
"Analysis of air."

A second and more advanced terminal course includes the estimation of carbonic acid by volume and by weight; the formation of carbonic oxide; the oxidation of oxalic acid by metallic peroxides, and the method founded thereon for determining (e.g.) peroxide of manganese; formation, &c., of sulphurous acid; volumetric determination of iodine; preparation of the oxides of nitrogen; method of Cavendish for the analysis of air; and numerous operations connected with nitric acid, cyanogen, phosphoric acid, and arseniuretted hydrogen.

There is still another means of imparting information which has not as yet been systematically adopted with reference to chemistry, but which in other sciences has been employed under judicious management with considerable success. A museum illustrative of chemical science has yet to be formed, but it might without doubt be so arranged as to prove of very high educational value. We do not mean that a heterogeneous collection of chemical specimens would be of any use: to adopt no other principle of classification but the size of the bottles, as appears to have been the case in arranging the specimens belonging to the Chemical Society of London, might answer for a curiosity-show, but not for a really scientific museum. An effective chemical museum should avoid in particular two faults very conspicuous in most collections relating to other branches of natural science; for usually the various objects, if labelled at all, merely inform us that such-and-such a number has been assigned them, or that they were presented by Mr. or Mrs. So-and-So. And then how absurd is the collocation—we cannot call it the arrangement—of the specimens. It may be very amusing, but we venture to doubt whether it be very instructive to see hung in the closest proximity in Crosthwaite's Keswick Museum a cluster of fifty-five nuts from Greystoke Park, slippers of rattlesnake-skin, and the helmet of an Irish dragoon. And even the Ashmolean Museum at Oxford scarcely presents a more favourable appearance, although we are told that its contents have been "arranged with the greatest accuracy by J. S. Duncan, D.C.L., according to the best recognised system." As, however, we ascend the staircase of this famous museum, we find on our right some Indian canoes arranged with great taste upon fragments of Egyptian sarcophagi; and when we get into the grand saloon itself we perceive, "in the fifth window-place, a model in plaster of the field of Waterloo, representing the position of the contending armies when the Prussians approached; also a Babylonish brick. To the right of the window is a portrait of Mary Davis, on whose head grew two horns, which she shed twice." It is not enough to collect objects; they must also be classified; description, too, whether conveyed by one means or another, must follow classification.

In the lecture and the laboratory, chemical apparatus and chemical products are frequently displayed, yet in an extensive and well-arranged chemical museum of reference, the student might make further and more intimate acquaintance with what he has before seen only occasionally or at a distance. The specimens might be so arranged and described as to illustrate chemical symbols, types, nomenclature, formulæ, and equations. Wherever the abbreviated language of symbols was employed, the longer names should by no means be omitted, and the student might thus imperceptibly learn the connexion of the two methods of expression. In such a collection chemical apparatus will find a place as well as chemical substances; crystallography, chemical processes, and many of the arts dependent thereon, may also be illustrated by models, drawings, and diagrams.

We will now pass on to notice as briefly as possible some of the numerous books which have been published within the last few years as guides for the student of chemistry, pointing out the leading characteristics of each work.

Dr. Lardner's 'Chemistry for Schools' answers, we think, the purpose of its projector as stated in his preface. It is intended for the use of schools and families, and "comprehends so much of the elements of chemistry as may, with moderate attention, be acquired within a reasonable time by the younger class of students, and even as much as may suffice for those who, being more advanced in life, desire merely to obtain a general knowledge of the elements of the material world, and of the chief compounds into which they enter." The price of the book is extremely moderate, and it is illustrated with numerous engravings. Many, if not all of these we recognise as old friends, whose acquaintance we first made in the works of Regnault and other French chemists, and not a few have already done duty in other books by Dr. Lardner. The foreign origin of the illustrations is disclosed by the constant appearance in them of charcoal furnaces in every variety of shape and size, as sources of heat, while in England gas-burners are almost without exception substituted for clay-furnaces, which indeed are not employed except on the rarest occasions. The book, like the other works of this author, is written in an easy and attractive style, and is in general tolerably accurate. The late Dr. George Wilson was the author of an elementary text-book of chemistry very similar in plan and execution to that by the late Dr. Lardner, to which we have just alluded. It has met with very general acceptance, 25,000 having been sold in ten years. It explains the principles, and also points out the most striking phenomena of the science. We can only regret that the new issue has not had the advantage of the author's revision.

A small book by Mr. Galloway is somewhat similar in scope to that of Dr. Lardner. 'The First Step in Chemistry' answers well to its title. It is just the book to be used in schools where, although much time and attention cannot be devoted to chemistry, yet the importance of some general acquaintance with the chief facts of the science is recognised. The author has appended easy exercises to each chapter, in order to familiarize his young readers with the subjects of chemical nomenclature, notation, the meaning of such terms as elements, equivalents, affinity, &c. A clear exposition is made of the way in which equations are constructed; it gives also a more detailed account of the use and meaning of chemical terms than is to be found in other manuals, and will enable the beginner to understand what he reads in works of descriptive chemistry.

Although the title suggests play rather than work, yet Mr. Griffin's 'Chemical Recreations' is by no means a book to be despised. The details concerning the construction and employment of many pieces of apparatus will be found extremely useful. The work (more particularly in its earlier editions) was written in a charming style, and has now become deservedly popular. It is not disfigured by many of those violent denunciations against modern chemists which characterize most of the author's other works, and especially, and to the most painful extent, his latest production—'The Radical Theory in Chemistry.' Berzelius, Städeler, Hofmann, and other chemists are there attacked in turn, sometimes with bitter sarcasm, and sometimes with playful irony. Mr. Griffin objects to Hofmann's expressive nomenclature, and such terms as *tetrethylium* and *methyltriethylphosphonium*, which, whatever may be their defects, have not an unpleasant sound, and can be understood by every chemist; but he does not hesitate to coin such an alarming and uncouth expression as *zincen carbite cum zincabon hydronoze*, while Hofmann's melaniline appears as *phenylac cyana cum phenyl hydra*. In an elementary and eminently practical work such as 'Chemical Recreations,' there was happily but little opportunity for the development of these eccentricities; and the book, profusely illustrated as it is with several hundred woodcuts, is really very pleasant and readable. Among other interesting notices of apparatus for attaining high temperatures, we find an excellent description of the effective gas blast furnace of which the author is the inventor, and which has already proved a great boon to chemists.

The late Mr. Bowman's 'Introduction to Practical Chemistry,' even in its original form, was a very compact and useful volume. In it there appeared for the first time a carefully arranged and illustrated course of experiments on manipulation, as well as several great improvements in the way of exhibiting prominently those more important phenomena of which advantage is taken in chemical analysis. Now that the work has undergone most careful revision under the superintendence of Professor Bloxam, whose alterations and additions are most judicious, we know of no book which serves better to direct students commencing the practical work of the laboratory. It is now quite the favourite in several of the best London schools of chemistry. The following are some of the salient features of the work:—In Part I. typical examples are given of such operations as the Management of Gases—Distillation—Glass-working—Blowpipe Experiments—Determination of Specific Gravities—Processes of Reduction—Preparation of Reagents—Alkalimetry. Although the guidance of a teacher in performing these experiments is desirable, yet the descriptions in the text and the illustrative woodcuts are so intelligible, that no attentive student need fail of success. The author has ventured to introduce a slight alteration in the usual mode of printing chemical symbols, which is perhaps of some value to the student in the early stage of his career, though it has not been generally adopted, and scarcely commends itself to the approbation of the chemist. It consists in the employment of different type for representing the different conditions of the same substance. Any substance, such as water, which may appear in all the three forms,—solid, liquid, and gaseous—will of course be represented by the same symbols, only these will appear in different types. Ice, for instance, is printed in strong Roman type, water in italics, and steam in fine hair letters. This contrivance is made use of throughout the book in all symbols, formulæ, and equations, and applies as well to amorphous as to crystalline bodies, to solutions as well as to liquids proper, to vapours as well as to gases. Part II. is devoted to the reactions of the various acids and bases; and in Part III. the learner is taught how to combine his recently acquired manipulatory skill with his knowledge of the chemical properties of the various substances which may be given him for examination, so as to proceed to the actual analysis of substances of unknown composition by proceeding in accordance with the plans of operation given. Part IV. offers some sound elementary instruction in quantitative analysis; while Part V. describes the nature, preparation, and usual impurities of the reagents employed. In the Appendix are very many first-rate Tables (one exhibits clearly the behaviour of bases and acids with various reagents); and there is also a glossary of chemical terms.

Mr. Noad's work, entitled 'Chemical Manipulation and Analysis,' includes much material within the compass of a volume moderate in price and size. Unfortunately the typographical errors of the volume are so numerous as decidedly to interfere with its usefulness to the student. It contains explanations of chemical equivalents, formulæ, and equations, the directions as to the preparation and management of gases, and descriptions of various processes of quantitative as well as qualitative analysis. A set of analytical tables is added. The late Professor Fownes's 'Introduction to Qualitative Analysis' has also been superseded by other and more complete works. Professor Odling's 'Course of Practical Chemistry, for the use of Medical Students, arranged with express Reference to the Three Months' Summer Course,' is a book of much merit; and we should think that a revised edition of it, in which the best new methods of analysis were introduced, would prove acceptable to a large class of students.

A small book of about fifty pages, by Mr. Greville Williams, entitled 'A Manual of Chemical Analysis, for the Use of Schools,' is written so simply as to be easily mastered by boys. Good use is made of conspicuous type as a means of drawing the attention of the student to important facts. The general cautions at the commencement of the book, and the numerous exercises at the end, will be found very useful in teaching the elements of the science by schoolmasters, for whom a key, containing answers to the various questions propounded in the book, has been prepared by the author.

Mr. Greville Williams is also the author of 'A Handbook of Chemical Manipulation'—the only work, in fact, which treats with adequate fulness of this important subject. All the most conspicuous chemical facts may of course be studied experimentally by means of apparatus of the simplest and cheapest kind, much of which, indeed, the student may, with a little ingenuity, construct for himself; and he will find in Mr. Williams's 'Handbook' all sorts of ingenious devices and plans for home-made apparatus. But the student who wishes to advance further, and to become, maybe, an original worker in some department of the science, will require more information as to the mechanical art of manipulation, as to the methods adopted in research, and as to the uses and construction of those more complicated forms of apparatus which have been devised for various special purposes by successful chemical experimentalists. Such a student will find exactly what he wants in the present volume.

Modern chemistry owes, no doubt, its rapid development of late years in great measure to two important points connected with manipulation. The workers in this science have been characterized not only by a wonderful fertility of invention, but also by a spirit of minute and scrupulous attention to details, of which we have had such conspicuous examples in Sir Humphry Davy and Berzelius, who by its means were enabled to clear the foundations of chemical science from an immense amount of accumulated rubbish, and in very many cases to build up edifices which subsequent efforts have only served to strengthen. We doubt whether any other science has pressed into its service so many methods of inquiry and so many instruments of research. Not only does chemistry possess numerous forms of apparatus peculiarly its own, but it has borrowed from other branches of science such instruments as the barometer, the galvanic battery, the goniometer, the microscope, the polariscope, and the prism; and of all of these has made good use. Very important, also, have been the improvements recently effected in the construction of apparatus long considered as especially pertaining to chemists—as, for instance, in furnaces for attaining with facility the highest temperatures; and also in the use of such apparatus, as in the numerous and rapid processes of volumetrical analysis.

Hardly a month passes without the appearance of a new work, or at all events a new edition of an old work, on analytical, descriptive, or theoretical chemistry. Many of these books contain some few pages devoted to a description of the more usual processes and apparatus, but the really successful treatment of the subject requires a volume. Many years ago Professor Faraday incorporated into an interesting manual a number of useful hints and happy contrivances, many of them originated by himself; and his treatise was for a long time the only work in the language from which the student could learn anything systematically concerning the management of apparatus. But this book cannot now be obtained, and it is a fortunate circumstance, therefore, that it has been succeeded by a still more perfect and comprehensive book, suitable alike for every one engaged in chemical studies. The author, already distinguished for his numerous original researches in organic chemistry, has given to the world several new processes and contrivances suggested or perfected by himself during the course of his investigations. The sections on the use of the balance, and the various precautions to be taken in weighing, on specific gravities and vapour densities, on supports for apparatus, on distillation, and on the manipulatory details of organic analysis, are carefully written and full of information. The drawings in the book are singularly excellent; they are upwards of four hundred in number, and give so good an idea of the different pieces of apparatus which they represent, that the student who has not had the advantage of laboratory instruction will find it easy by their aid and that of Mr. Williams's clear descriptions to construct for himself almost any kind of apparatus that he may require. It is impossible to point out all the important parts of the treatise in a brief review; we can only say that no one can refer to the sections on glass-working, on miscellaneous operations, and on the processes and reactions employed in researches, without profit. The merit of this unique work has been acknowledged in Germany in an emphatic manner, an exact reproduction in every particular of the original volume having been published last year at Munich. For further details concerning the manipulation of gases, reference may be made to Bunsen's 'Gasometry,' which can be consulted in the original German, or in the English translation by Roscoe, or the French by Schneider. No public laboratory or chemist's library should be without Mr. Williams's 'Handbook of Chemical Manipulation.'

The student who has commenced practical work in a laboratory should not be content with hearing chemical lectures only, but should study some text-book of theoretical and descriptive chemistry more complete than those to which we have already alluded. The second edition of Messrs. Abel and Bloxam's comprehensive treatise is full of useful matter relating to every department of chemistry. It includes in one volume an immense variety of subjects. The analytical tables at the end of the volume are peculiarly good. The somewhat meagre account of organic chemistry may be supplemented by Dr. Gregory's 'Handbook of Organic Chemistry,' which is very full of details on this subject. The work by the same author on inorganic chemistry is a good text-book, and may be read with profit by students attending lectures and beginning to find out what chemical manipulation and analysis really mean. The volume which treats of organic compounds is still more complete, and offers a comprehensive abstract of the state of that department of the science as it was five years ago. A careful index of five-and-twenty pages enhances the value of the work. Personally we have very pleasant recollections connected with Dr. Gregory's Handbook. It was the first work we read on organic chemistry, and we shall not easily forget the pleasure its perusal gave. Indeed, the chapter on the derivatives from oil of bitter almonds had for us more charms than a romance.

The eighth edition of Fownes's 'Manual' is a triumph of the art of condensation. In one volume we are presented with all the more important facts of chemical physics, and of chemistry itself, organic and inorganic. Nor has condensation led to obscurity. The descriptions and explanations, though concise, are always clear and easily understood. The plan of the work is strictly adhered to throughout, the amount of space occupied by any subject being strictly proportional to its importance. In this eighth edition we find considerable alteration, enlargement, and improvement in the accounts given of the analogues of ammonia in the phosphorus, arsenic, and antimony series; of the newly discovered nitrogen substitutions; and of many questions connected with physiological chemistry—such as the occurrence of sugar in the urine in health and disease. These and various other changes and additions made by the distinguished editors of the work before us have very greatly increased its value. We have, however, to point out several omissions of acknowledged discoveries, for we think that these omissions prevent, in some degree, this new edition of Fownes's 'Manual' from being considered as so marked an advance upon the preceding issue of the work as might have been expected. Several important compounds with which chemistry has been enriched during the year or so previous to its publication are not noticed. Nor do we find much information about certain substances whose constitution has been elucidated, at all events in some measure, by recent researches. Wöhler's new compounds of silicon, Brodie's graphic acid, the recent determination of the equivalent of lithium, the binoxides of organic radicals, such as acetyle and benzoyle, the formation of toluylic acid from toluole, of chloride of benzoyle from benzoic acid, and of succinic acid from olefiant gas, and the hydrocarbons from india-rubber, isoprene and caoutchine, are omitted altogether. No explanation is given of the constitution of benzo-phenone, or of melam, melamine, and their many allies. Although pelopium does not exist, we find descriptions of its oxide and chloride. The editors say in their Preface, that but "few alterations were required" in this edition; we cannot, however, help wishing that more attention had been devoted to the revision of the work in its minor details. An "Essay on the Unitary System of Gerhardt" is an interesting and valuable addition to the present issue of the 'Manual,' and in the almost total absence of all information on this subject accessible to students, will be received with pleasure. Yet why is the Unitary System represented without any notice of its recent important developments? In a work like Fownes's 'Manual of Chemistry,' of which new editions are demanded at such short intervals, we expect to find a record of the progress which the science has made up to a period within six months of the issue of each new edition. In the present case, an account appears of Bunsen's and Kirchhoff's recent researches on Spectrum Analysis (a further elaboration of discoveries announced more than a quarter of a century ago by Mr. Fox Talbot), and hence our surprise was greater at the omissions to which we have before referred.

But, in fact, the faults to which we have made reference scarcely detract from the real value of this excellent manual, which is too highly appreciated to need any fresh commendation from us. The first three parts treat of Chemical Physics, Inorganic and Organic Chemistry; the fourth part, devoted to Animal Chemistry, will serve as a first introduction to that branch of the science, and may be read with advantage by medical students, among whom this volume is justly popular.

Dr. Miller's treatise presents a great many excellent features; indeed, we know of no other portable book of reference which is sufficiently comprehensive to satisfy the needs of a student who has made some considerable progress in mastering the rudiments of the science. The first part, devoted to the elucidation of all those questions which more immediately concern the chemist, is specially worthy of consideration. It has supplied a great want, for with the exception, perhaps, of the new edition of Bird and Brooke's 'Natural Philosophy,' we are at present without any good English work on physics, although Dr. Atkinson's translation of Ganot's Treatise is in the press; and Jamin, in France, has lately produced a complete 'Cours de Physique.' The second part gives a very fair account of the non-metallic elements and their compounds. These two first sections of the book have already reached a second edition, and now appear with many improvements and corrections. Indeed, the number of typographical errors, some of these being of the most serious kind, which were tabulated as Errata in the first edition of the complete work, was more than one hundred and thirty, and even then many were left unnoticed. We trust that a new and corrected edition of the third part, which contains the account of Organic Chemistry, will be forthcoming ere long. We venture to think that this part of the work is less satisfactory than the others. There are some errors in the formulæ of organic bodies which it is difficult to believe are merely typographical. One instance may perhaps suffice. On p. 434 a table is given to illustrate the relations of monatomic, diatomic, and triatomic alcohols. Among the substances mentioned in this table we find one named "ethylic glycerine," (?) to which the irrational formulæ $(C_4H_5)''' \ H_3O_6$ is assigned. Not only is this formula out of relation with the remainder of the table, but it rests upon (what appears to us) the groundless assumption that ethyle C_4H_5, can play the part of a triatomic radical. The expression should be written $(C_4H_3)''' \ H_3O_6$, and the name, we suggest to Dr. Miller, might perhaps with advantage be changed to *acetylic* glycerine. In his Preface to the third part, Dr. Miller makes some observations on the order in which he has treated the various subjects included in organic chemistry. He has evidently hesitated somewhat before committing himself to the system which he has adopted, and for the weak points of which he makes some excuse. It is possible he may be right in rejecting Gerhardt's method of classification as it stands without modification in the 'Traité de Chimie Organique' of that chemist, yet considering the mystery in which such complex products as the different varieties of sugar, starch, and gum are involved, it scarcely seems expedient to make them the starting-point in describing organic compounds. The most experienced chemist will sympathize with the young beginner in the science who finds himself, directly he has begun to read about organic substances in Dr. Miller's volume, sorely puzzled by the relations of sucrose, fructose, glucose, lactose, and melitose to polarized light, and lost among such delightful names as inulin and glycyrrhizin. The student is next led to the chief products of the fermentation of sugar—namely, alcohol and its homologues, with their various derivatives: here, of course, the author could not avoid following Gerhardt's method of arrangement into series. Dr. Miller says (Preface, p. vi.) that he has

> "Preferred to examine successively the different members of each homologous group, before passing to the consideration of the derivatives from the typical or leading member of each group. For instance, in the case of ordinary alcohol, instead of describing ether, aldehyde and acetic acid in succession, the different varieties of alcohol, such as wood spirit, fusel-oil, &c., are first described; then aldehyd and its homologues; after them the series of the vinic acids, and then that of the ethers."

The author considers that by presenting the members of a series of homologues successively to the student, he is enabled to trace more readily their relations than if "his attention were distracted by passing to other bodies of totally different character and properties." Now, however, that we can trace, thanks to the Unitary notation of Gerhardt and the suggestive researches of Laurent, very intimate relations between the alcohols and their numerous derivatives, Dr. Miller's plan no longer presents the advantages which it might otherwise have claimed. Indeed, one of the works named at the head of this article, Professor Kekulé's 'Text-Book of Organic Chemistry,' proceeds in this matter upon a method of classification at once simpler and more scientific. To a devotee of the inconsistent old notation in its worst form, which writes ether C_4H_5O instead of C_4H_5O,C_4H_5O,* and thus ignores its connexion with alcohol C_4H_5O,HO; and which affirms that chloroform is merely formic acid in which three equivalents of oxygen have been replaced by chlorine, it sounds quite correct to call ether a body totally different in constitution from alcohol. But the disciples of the Unitary System, who appreciate at the same time the value of types, and of the doctrines of homology and molecular equivalency, may point with pleasure to Professor Kekulé's success in smoothing the way to the study of organic chemistry, a task which he has accomplished without the least sacrifice of scientific order. Adopting Gerhardt's atomic weights, he traces with great clearness the real nature and relations of the changes which alcohols undergo when suffering oxidation, chlorination, &c., while the derivatives obtained by means of these various processes are shown to be intimately allied. So far as his work has yet gone, he has not been guided by "considerations of convenience" so much as by the "requirements of system," and yet he has made the most philosophical identical with the simplest method. Dr. Miller compares an homologous series with a ladder, the terms of the series succeeding each other as the rounds of the ladder. He likens the derived or heterologous series to other ladders placed by the side of the first. He then adds: "It is safer and easier to ascend and descend the steps of each ladder in succession than to step across from

* Adopting Gerhardt's equivalents, the formulæ of water, alcohol, and ether, become respectively $\left.\begin{matrix}H\\H\end{matrix}\right\}O$, $\left.\begin{matrix}C_2H_5\\H\end{matrix}\right\}O$ and $\left.\begin{matrix}C_2H_5\\C_2H_5\end{matrix}\right\}O$, thus exhibiting the relations of the three bodies very concisely.

one ladder to the other." The simile is pretty, but admits of another application. For the penetrating eyes of Laurent and Gerhardt saw more real, though perhaps less apparent connexion between the various derivatives of an alcohol than between two alcohols. Thus the rounds of the ladder may be taken to represent derivatives rather than homologues, the case becoming reversed.

That is scarcely a good reason which is given for retaining "in the elementary portion of the work, the older view which regards certain acids (carbonic, oxalic, sulphuric, and sulphurous) as monobasic, and which has been hitherto assumed in all treatises published prior to that of Gerhardt." The end proposed, the preservation of simplicity in the formulæ, is defeated, at the same time that an evidently incorrect view is taught because it is supposed to be easy. We cannot commend the formulæ given by Dr. Miller (and many others) for sulphuric acid, acid sulphate, and neutral sulphate of potassium respectively, to wit, HO,SO_3; HO,SO_3,KO,SO_3; and KO,SO_3, for we thus miss the simple relations subsisting between the three bodies. Retaining the old equivalents even, the formulæ $HO,HO,2SO_3$; $HO,KO,2SO_3$; and $KO,KO,2SO_3$ are much to be preferred; for when simply translated into the unitary notation they become $HHSO_4$; $HKSO_4$; and $KKSO_4$, not only evidently related, but as simple as possible.

To some of the author's statements which reappear in the new edition we are compelled to demur. The following observations, which will be found in the second edition of Part II., on page 82, seem to us somewhat dogmatic, and insufficiently supported by experimental evidence :—

"No metal will unite directly with an acid; in order that combination between them should occur, it is necessary that the metal should be in the form of an oxide. This oxidation may, however, be effected at the same time that the acid is presented to the metal, and the formation of the oxide and its solution in the acid may appear to occur simultaneously. Zinc, for example, does not unite as zinc with sulphuric acid; when this metal is placed in diluted sulphuric acid, the oxygen is supplied from the water which is decomposed, oxide of zinc is produced, and is immediately dissolved by the acid, whilst the hydrogen escapes in the gaseous form."

How much simpler and how much freer from hypothesis is that view of the mutual action of zinc and sulphuric acid which regards the metal as merely displacing the hydrogen of the acid. It is time that we ceased to place acids in a category different from that which includes their salts. The view which states acids to contain water and salts oxides, depends much more upon hypothesis than that in which salts are considered as differing from acids only in this, that they contain one equivalent or more of a metal in place of one equivalent or more of hydrogen. It must be conceded, however, that the defects to which we refer belong to the old system of chemistry in general, rather than to the present treatise in particular. We hope on another occasion to point out the advantages of Gerhardt's notation, the merits of which are partially acknowledged by Dr. Miller in the foot-notes scattered through the second edition of his work.

Yet the real merits of Dr. Miller's work are indeed so great that the slight defects in arrangement, &c., to which we have alluded become quite insignificant when we consider how much valuable information is condensed into a comparatively small space. It is especially in the second volume that we are struck with this completeness, which warrants us in recommending the work as an admirable dictionary of chemical facts. The sections devoted to carbon, chlorine, sulphur, phosphorus, and the common and precious metals, are full of interesting details, for which we shall look in vain in any other work of similar size. More notice is also taken of manufacturing processes, and also of the physiological and medicinal relations of the more important substances described, than is usually found in treatises on scientific chemistry.

We have alluded to Dr. Kekulé's 'Text-Book of Organic Chemistry' as a work of much merit. It is at present without a representative in English chemical literature. It is a conspicuous example of the benefits which accrue from the adoption of the Unitary notation of Gerhardt. The principles on which organic compounds are constructed, and the relations which they bear to one another, are discussed in a truly philosophical spirit. It is right that an exhaustive book of reference on organic chemistry, like those of Gmelin and Gerhardt, should contain the elaborate history and the fullest details concerning the methods of preparation even of insignificant and obscure substances, but it is time that such tedious accounts were banished from the smaller handbooks which are intended for beginners, and which profess to give the truths of a science, not the processes of an art.

The 'Lessons in General Chemistry' by M. Cahours, demand a few words of comment. The chapters on metals and assaying will be found very useful. The book answers admirably, not only for those attending chemical lectures, but for lecturers themselves, as an assistance in devising and arranging their apparatus. The plates and woodcuts, two hundred and fifty or more in number, are of unequalled excellence; indeed the work, which extends to one thousand two hundred and seventy-six pages, and yet costs no more than ten francs or thereabouts, deserves to be better known in this country. Of this work a second and greatly improved edition has just appeared.

Mr. Galloway's 'Manual of Qualitative Analysis' has just reached a third edition. It is of moderate size and price, and can be easily followed by students.

The plan of instruction in analysis pursued at Giessen was given to the world by Professor Will in a small volume, which afterwards appeared in an English dress under the superintendence of Dr. Hofmann, and was adopted in the Royal College of Chemistry, London. This work, entitled 'Giessen Outlines of Analysis,' has long been out of print, and has, so we are given to understand in the preface by the English translator of Professor Fresenius's 'Course of Qualitative Analysis,' been superseded by the German original of the latter work. Mr. Conington has, however, made the fourth German edition of Dr. Will's 'Outlines' the basis of a compact volume, which treats not only of qualitative but also of quantitative analysis, and even includes some examples of technical determinations. In consequence of the comparatively small size of the work, it is, as a manual of general analysis, suggestive rather than exhaustive.

Mr. Conington's translation is accompanied by a separate book of tables, presenting numerous improvements upon the German original. The first, second, third, and ninth of these synopses of analytical operations refer to the preliminary examination, and to the method of converting insoluble into soluble substances; they are full and accurate. A judicious selection of the commoner and more important silicates might replace with advantage the somewhat tedious catalogue of Table XII., which, while it gives next to no information about them, names such rare mineral silicates as the following: kielhauite, chondrodite, wolskonskoite, and sordawalite. Several improvements on the old methods of separation are introduced into the tables; some, however, of the special tests employed, such as that for cadmium in the presence of copper by means of cyanide of potassium and sulphuretted hydrogen, have been condemned as untrustworthy.

Dr. Thomson, of Edinburgh, has quite recently brought out a set of analytical tables, differing in some respects from those usually appended to manuals of analysis. The first nine tables treat of the behaviour of the bases and acids with reagents, and, together with the notes on the chemical examination of urines and urinary calculi, and on the reactions of the organic bases, are decidedly the best parts of this little volume. The schemes for the actual analysis of substances are very far from perfect, some of the best reactions (such as that of cobalt with nitrite of potassium) given in Part I., being omitted in the subsequent tables. We must object also to some of Dr. Thomson's formulæ. Sulphuric and nitric acids are invariably written as SO_3 and NO_5, the rare anhydrides of these acids. Again, other symbols might have been found for nicotine, coniine, and morphine, instead of Ni, Co, and Mo, expressions already appropriated to nickel, cobalt, and molybdenum. The author, it is true, distinguishes the alkaloïds by the positive sign, but the necessity for any such addition to the symbol might easily be avoided.

Of the next work on our list, Messrs. Northcote and Church's 'Manual of Qualitative Chemical Analysis,' we have before* expressed a favourable opinion. It is evidently the result not only of much practical experience in the laboratory, but of an attentive study of the various methods which have been proposed from time to time for instruction in analysis. It is written in the unitary notation of Gerhardt, and was, we believe, the first work published in England in which that system was adopted. The authors have not scrupled to make very considerable use of tabular statements in their treatise. There are tables which give at a glance the most characteristic properties of the elements; tables of reactions; tables for the detection of a single base and a single acid; and general analytical tables; in all, there are forty of them. The details given of the reactions of the

* British and Foreign Medico-Chirurgical Review, vol. xxiv. p. 94.

rarer metals, so often omitted from analytical books, will enable the advanced student to detect their presence with ease. The arrangement of this manual presents several novelties to which the authors in their preface direct the attention of the reader, and which they are right in regarding as decided improvements. In a former review we have noticed the most prominent of these new features in virtue of which, as we before remarked, the work exhibits "a greater completeness of plan, a more accurate adjustment of subject, and a more harmonious subordination of less important parts than is usual in works of this nature." A single principle of classification has been adopted, and is strictly adhered to throughout the book. In describing, for instance, the behaviour with reagents of different bases and acids, the same order is invariably preserved, so that the student will know exactly where to find what he is seaching for. The "Method of Analysis" is clearly described, and the general directions are easy to follow; due prominence is given to the preliminary examination, both for acids and bases, a department of analysis the importance of which has been greatly overlooked. One of the authors of this manual has arranged a set of labels for reagents, and a table of atomic weights, in order to familiarize students with the changes introduced into chemical formulæ by the adoption of the unitary notation.

Fresenius's 'System of Instruction in Qualitative Analysis,' ably and accurately edited by Mr. Bullock, is invaluable as a dictionary of processes for the detection and separation of the various chemical substances. The fifth English edition is brought down in point of information to a late date, the best and most recent processes are to be found in it, although the author has not admitted any of which the accuracy is doubtful. The author, in common with many other teachers, disapproves of analytical tables; his work consequently does not contain any, but in lieu of them a very elaborate and systematic course of analysis, in which the various steps to be taken are detailed at length. To obey the directions thus given demands, however, no more useful intellectual effort than to carry out the plans indicated briefly in an analytical table. It is possible in either case to work mechanically, without considering the real nature of the changes involved in the different operations; and although the use of tables is perhaps more liable to this abuse, yet they may be so constructed as to necessitate a previous acquaintance with chemical facts, or, at all events, a constant reference to chemical works for further or special information. An appearance of considerable confusion is presented by the various sections of Dr. Fresenius's analytical course, in spite of the use of various types, of marginal references, numbered sections, and of an ingenious but rather puzzling way of indicating various grades of subordinate operations to be performed in the analysis of each group by such devices as the following: I, A, 1, *a*, *α*, *aa*, *αα*. These marks, and many others, are employed in regular succession, but when we come to εε we have, alas, forgotten all about A, or do not know where to catch up again the lost threads of our inquiry at ββ. Long use of the volume will of course gradually relieve the student from these difficulties in great measure, but they are best obviated by giving the plans of analysis in a set of tables.

If the arrangement of Dr. Fresenius's work on 'Qualitative Analysis' is open in some degree to objection, no fault can be found with the author's admirable synopsis of quantitative methods. Both volumes are replete with trustworthy information, but the second is an admirable example of painstaking research and honest labour. It unites the merits of all the best works on the subject with peculiar excellences of its own, not the least of which is the critical and experimental examination of the numerous processes of quantitative analysis recently suggested.

The sketch of operations, apparatus, &c., preliminary to the descriptions of the methods for separating and estimating mineral substances of all kinds, is clear and comprehensive. The section on the ultimate analysis of organic bodies includes all the best processes of Liebig, Bunsen, Dumas, Warrentrapp and Will, and Hofmann. The construction of the admirable gas-combustion furnace of the last-named chemist is described at length, and illustrated with woodcuts. The important questions connected with the calculation of analyses receive full attention; the determination of vapour-densities, and the deduction of empirical and rational formulæ, being treated at length.

The second or "special" part of the volume relates to the analysis of waters, of technical products and minerals, of the ashes of plants, of soils, of manures, and of atmospheric air. It is difficult to commend too highly the judgment displayed by the author in the selection of processes which he has made. The directions given as to the apparatus required, the various observations to be made and operations to be performed in order to accomplish a complete analysis of a mineral water, leave nothing to be desired. The experiments to be performed at the spring or well, relating to the temperature of the water, the collection of the gases, &c., are described first, and then the further operations to be subsequently carried on in the laboratory. Ample information is afforded as to the rarer constituents of natural waters for which the analyst must by no means fail to look. A careful analysis by Mr. Northcote of the Wheelock brine of Cheshire actually disclosed the presence of no less than 17 grains of bromide of sodium in the imperial gallon. Our attention is next directed to the section on acidimetry, alkalimetry, chlorimetry, valuation of manganese and iron ores, and analyses of gunpowder, silicates, clays, and limestones. On pages 576–580 will be found the chemical methods for the determination of sugar; they are accompanied by remarks pointing out the conditions of a successful experiment. In the section on the analysis of air, the beautiful and simple method of Pettenkofer for the estimation of the carbonic acid in air is given, p. 610. This expeditious process, which does not require more than six litres of air, may be executed by an inexperienced manipulator in less than an hour. Nothing is required but a good-sized glass balloon or bottle capable of being closed air-tight, a burette, and standard solutions of lime-water and oxalic acid. We can speak from actual experience of the success of this method in determining with accuracy the percentage of carbonic acid in crowded rooms, &c. In some cases, however, it is equally necessary to ascertain the amount present of that still more injurious gas, carbonic oxide. The third part of the work contains excellent exercises for practice, a number of most useful tables, and some very valuable notes, experiments, and observations by the author himself. Many of these serve to confirm the accuracy of the methods recommended in the body of the work, and relate chiefly to the solubility of various precipitates in water and in certain saline solutions. Carbonate of barium, for instance, was generally supposed to be insoluble in pure water; although this is not exactly the case, yet Fresenius has determined that 1 part of this salt requires 14,137 parts of water for solution. And if neutral nitrate of silver be added to this solution, an immediate cloudiness, arising from the formation of the carbonate of that metal, indicates the presence of the earthy carbonate. But Fresenius has further shown that carbonate of barium requires no less than 141,000 parts of water containing carbonate of ammonium to effect its solution; it is in the presence of this latter salt that it is precipitated in the course of analysis.

The exhaustive treatise by H. Rose on 'Analytical Chemistry' is a magnificent work, but will be found more useful as a book of reference for the professional chemist, than as a laboratory manual for the student. The qualitative portion alone extends over more than 1000 large pages, and the complete work consists of no fewer than 2200.

In addition to the general works on analysis to which we have drawn attention, many volumes on special methods of estimation of organic and inorganic bodies have appeared from time to time. Many of these have related to pharmaceutical preparations and to the substances employed in agriculture or the arts. Some, however, such as Professor Bunsen's 'Gasometry,' to which we have already alluded, describe in detail particular departments of chemical analysis proper, yet at the same time give illustrations and examples of the processes employed, by showing the application of these to the examination and analysis of numerous substances met with in commerce or in common life. The handbooks of Professors Liebig and Wöhler are of this class.

Professor Wöhler has collected in his 'Handbook' over one hundred examples to illustrate the most important processes for ascertaining the proportion of each elementary constituent of mineral substances. It is difficult to speak too highly of the skill with which the selection has been made, and of the clear and simple language in which the methods to be followed are described. The aim of the author is given in the preface to his work in a few words, which we quote:

"This collection of examples for practice in chemical analysis is designed chiefly for the use of the laboratory. It is drawn up under the impression that it is easier for most minds to obtain a clear insight into general relations and laws by the study of special cases, than inversely to acquire a knowledge of individual cases by first directing the attention to general rules. An endeavour has been made to arrange the book in such a manner as still to leave enough to demand the reflection of the student and the explanations of the teacher."

Not only is the principal object of the book, as stated in these words, answered, but it will be found a very convenient and useful reference book in many special analytical inquiries. For instance, the 110th example is headed, "Mineral Waters, Well Waters, and Saline Springs," and affords the student an instructive sketch of the way in which the various constituents of these liquids are to be determined. The occasional and rarer ingredients of such water are also referred to, and directions given for detecting the presence of bromine, iodine, fluorine, lithium, strontium, and arsenic. In the case of arsenic, its occurrence has recently been ascertained in so many instances, as in the water of Arvène, in the hot spring of Wiesbaden, and in the common drinking water of Whitbeck in Cumberland, that a most careful search for it becomes necessary in every examination of medicinal waters. Frequently, where arsenic has not been found in appreciable quantity in the water itself, chemists have succeeded in detecting it in the sands and detritus of the stream, or in the ochreous and calcareous deposits of the water. In consequence of the great attention which the subject of the occurrence and detection of arsenic has lately received, the section of the volume entitled "Examination for Arsenic in Cases of Poisoning" admits of some slight alteration and extension. It has been shown that the real difficulty is to find anything in which, if a sufficiently large amount be experimented upon, arsenic does not exist. Mr. Dugald Campbell's simple improvement on Reinsch's test, in which he reduces the size of the fragment of copper foil employed to a minimum, and thus is enabled to detect minute traces of arsenic which would otherwise escape notice, seems to merit adoption. But it appears to us that we are under great obligation to Professor Bloxam for his long-continued and laborious researches as to the best way of employing electrolysis for the detection of arsenic. His recently-perfected process seems to leave nothing to be desired. One great advantage of his method lies in its enabling us to dispense almost entirely with the use of suspected reagents. The arsenic appears in the form of arseniuretted hydrogen as usual, but the hydrogen is produced not by means of zinc and sulphuric acid, about the purity of which we are compelled to doubt, but by the electrolytic decomposition of water, the galvanic current necessary for the purpose being generated by a few cells of Grove's battery. In the section on mineral waters, mention of the occurrence of manganese is accidentally omitted. This element is almost invariably present in chalybeates and petrifying springs. It is found, for example, in several of the Harrogate waters, and in the calcareous stream in the valley of the Teme, which has deposited that immense mass of tufa known as Southstone Rock. On the Continent, many ochreous deposits have been found which yielded a considerable percentage of manganese on analysis, and occasionally, though rarely, they have been ascertained to consist chiefly of the oxides of this metal. Walter Crum's test for manganese is decidedly the most delicate. The solid residue from the evaporation of a large quantity of the water must be employed, and the manganese will usually be found accompanying the iron in that portion of the solid residue which is insoluble in chloride of ammonium and ammonia. This is collected on a filter, and dissolved in pure nitric acid. Puce-coloured oxide of lead, the peroxide, is then dropped in, when, if manganese be present, a magnificent purple colour, arising from the production of permanganic acid, will be produced.

An excellent little book of directions in organic analysis was translated from the German of Liebig by Dr. Hofmann some eight years ago. However, most, if not all, of the information it contains is to be found in Mr. Greville Williams' 'Handbook of Chemical Manipulation,' a more recent work. In this department of analysis several new plans have lately been devised. Of these, the most important is Dr. Hofmann's method of analysing organic substances by means of gas, which, as recently perfected, is more easy, rapid, and manageable than the older method, when once the apparatus has been got into working order. Where the heat necessary for the combustion of the substance is greater than glass tubes will stand, porcelain tubes, glazed inside and out, may be substituted with advantage. And if the substance be explosive and liable to be scattered, it may be mixed with some inert body, such as silica, and the mixture so arranged in a long platinum boat, that it can be heated in separate portions; two or more plugs of purified asbestos being inserted at either end of the porcelain tube; and if these plugs are properly mounted on platinum wires, so as to be limited in their movements by the glass caps of the combustion-tube, the analysis may be conducted with perfect success. The potash employed for the absorption of the carbonic acid in these determinations of explosive bodies should not be contained in the ordinary bulb apparatus, but in one or more U tubes, filled partly with dry potash and partly with fragments of pumice stone soaked in a saturated solution of the caustic alkali. Some of the processes recently suggested in the department of organic analysis do not seem to have much to recommend them, and have not attracted much notice. It has been proposed to determine the nitrogen of organic substances by means of the absorptive power for ammonia possessed by sulphate of zinc; to oxidize carbon into carbonic acid by means of chromic acid in place of oxide of copper; and to determine sulphur, phosphorus, arsenic, chlorine, bromine, iodine, as well as metals existing in various organic compounds, by heating these substances in closed tubes with nitric acid of definite strength. This last process, recently elaborated by M. Carius, promises to be of considerable use in the analysis of some of the more difficultly decomposable substances containing phosphorus, arsenic, &c., such as those which the researches of Hofmann and others have lately made known.

Review VII.

1. *Practical Observations on Diseases of Joints involving Anchylosis, and on the Treatment for the Restoration of Motion.* By Bernard E. Brodhurst, F.R.C.S., Assistant-Surgeon to the Royal Orthopædic Hospital.—*London*, 1861. pp. 120.
2. *The Reparative Process in Human Tendons after Subcutaneous Division for the Cure of Deformity.* By William Adams, F.R.C.S., Surgeon to the Great Northern and Royal Orthopædic Hospitals.—*London*, 1860. pp. 175.
3. *On Diseases and Deformities of the Spine, Chest, and Limbs.* Part I. *Disease of the Spine causing Posterior Angular Projection, Abscess, and Paralysis.* By Richard Hughes, M.R.C.S., L.R.C.P. Ed. (Exam.), Surgeon to the Brighton Orthopædic Hospital.—*London*, 1860. pp. 40.

Those who are now of an age to look back with a clear memory to the state of the civilized world before 1815, feel almost as though they were at this time living in another planet. Travellers were then two days and nights passing from London to York by the mail, and were accompanied by a man, who protected them with a blunderbuss and a bugle. Letters between the same places cost fifteen pence postage. A member of Parliament franked his correspondence, and his political supporters conceived they had a right to demand that his signature should mulct the Treasury in their favour. We were never less than six weeks, and generally longer, getting to New York; from six to nine months between London and Calcutta. The distances seemed so great, and the cost of postage was so enormous, that a son who had gone out to India was (unless among the wealthiest class) perhaps never heard of again till either he came home a millionaire, or till some one brought back a token and the tidings of his death years ago. Among the working classes the difficulty of correspondence made a separation of twenty-five or fifty miles sufficient to prevent parent or child, brother or sister, ever hearing of each other again. Our present powers and abilities need not now be counted; in a few more years our posterity may look back upon our ends as only beginnings, upon our great obstacles as but little stepping-stones, upon each of our triumphs as very nearly a defeat. So does one generation reach higher than the other, treading on the bones of its forerunners.

Surgery, one of the most conservative of arts and most jealous of innovation, has not meantime stood still, but has spread wider and conquered new fields for the exercise of its powers. We may look back into old works (for now in twenty years a book is superannuated), and find diseases well described, rules of practice precisely given. More especially may we be struck with the fact that, in the treatment of acute diseases, surgery has not much changed, nor do even the least venerative spirits find great reason for altering what their predecessors have done; but, in the management of long-standing diseases, reform has been effected; limbs are now saved which were formerly ruthlessly lopped off, and numberless people are now restored to ease and comfort who would formerly have been abandoned to a life of protracted suffering. Perhaps, among all the newer acquisitions in surgery, few illustrate so completely this change as the treatment of deformities.

Reforms of any magnitude are in no art so sudden as they at first appear; they are preceded by some foreshadowings, some travailing of advanced genius, some birth of a new truth into a world not ready to receive it; thus it is certain that the expansive force of steam had been examined into, and the general plan of its application sketched, long before the steam-engine was applied to practical purposes—a sort of electric telegraph was described long before Wheatstone made it. There is nothing new under the sun.*

Doubtless deformities existed before civilization had made any great progress, and among many nations, who did not consider themselves savages—the Lacedæmonians, for instance—infants thus unfortunate were exposed in a cave to the hunger of wolves, or of other wild beasts. After a time, and under milder rule, attempts were made to save such children, and, if possible, to correct their deformity. Hippocrates gives a fairly accurate description of the treatment to be adopted both at the spine and limbs. It seems that there was a popular mode, which the Father of Medicine condemns, of treating posterior curvature, particularly of that sudden form frequently met with, and which was then supposed to be a dislocation. "When the spine protrudes backwards from a fall, it happens that few recover, wherefore concussion (Κάτăτασις) on a ladder has never straightened anybody that I knew of."† In the next paragraph he observes that, as the weight of the head is but small, those who have the hump on the neck are less benefited by these successions on the ladder with the head downwards, but such should rather be succussed with the feet below, since the lower parts are heavier. He then goes on to describe the catatasia; the patient (and truly in those days a sick person deserved that name) was fastened by various soft bandages and shawls to a short ladder, which was however long enough to project beyond the head and feet; the man, thus secured, was dragged up to a high tower, or to the gable of a house, and thence was pitched down, usually head foremost, when the impact of the whole apparatus on the ground, acting upon the body, was expected to straighten the spine (περι Αρθρων ὑπόμνημα Γ. κη′). The other method, which Hippocrates admires much more, and which appears to be his own, is to make extension from the patient's chest and shoulders, and counter-extension from the loins and thighs, by means of bands fastened round levers at the head and foot of a wooden couch on which the patient is lying prone.

* It is not intended to impugn the originality of Mr. Wheatstone's invention: among the strange farrago of alchemy and astrology which books two hundred years old contain, there are frequently to be found quaint and wondrous descriptions of instruments actually made or to be made, and which have been subsequently discovered; but no one unacquainted with the construction and working of the thing described could by these vague descriptions make out much of what was meant.

† περι Αρθρων ὑπόμνημα (Γ. κ′.)

"For extension thus made could do no harm if properly performed, unless one sought to do it purposely. But the physician, or some person who is strong and not uninstructed, should apply the palm of one hand to the hump, and then, having laid the other hand upon the former, he should press firmly, observing whether this is to be done directly downwards, or towards the head, or towards the ischium. This means of applying force is quite safe, and it is also safe if any one sits upon the hump, and lifting himself from it, lets himself fall on it again; and nothing would forbid to stand with the foot upon the hump, and to support this weight on it, and to concuss it gently."*

He then asserts that the most efficient way to make pressure is by means of a lever, whose arrangement he describes as being fixed at one end to the couch or a wall, as a fulcrum, while two men at the other, forcing down the power end of the lever, violently compress the hump beneath it.

The same author also mentions, and describes with acuteness, and here and there with remarkable accuracy, certain distortions of the limbs.

"Wherefore, then, some of these congenital displacements, if to a small extent, may be reduced to their natural condition, and especially those at the ankle-joint. Most cases of congenital club-foot are remediable unless the declination be very great, or when the affection occurs at an advanced period of youth. The best plan, then, is to treat such cases at as early a period as possible, before the deficiency of the bones of the foot is very great, and before there is any great wasting of the flesh of the leg. There is more than one variety of club-foot, the most of them being not complete dislocations but impairments connected with the habitual maintenance of the limb in a certain position. In conducting the treatment, attention must be paid to the following points: To push back and rectify the bone of the leg at the ankle from without inwards, and to make counter pressure on the bone of the heel in an outward direction, so as to bring it into line, in order that the displaced bones may meet at the middle and side of the foot; and the mass of the toes, with the great toe, are to be inclined inward and retained so; and the parts are to be secured, with cerate containing a full proportion of resin, with compresses and soft bandages in sufficient quantity, but not applied too tight, and the turns of the bandages should be in the same direction as the rectifying of the foot with the hands, so that the foot may appear to incline a little outwards; and a sole made of leather, not very hard, or of lead, is to be bound on, and it is not to be applied to the skin; but when you are about to make the last turns of the bandages, and when it is all bandaged, you must attach the end of one of the bandages that are used to the bandages applied to the inferior part of the foot on the line of the little toe, and then this bandage is to be rolled upwards in what is considered to be a sufficient degree to above the calf of the leg, so that it may remain firm when thus arranged,—in a word, as if moulding a wax model, you must bring into their natural position the parts which were abnormally displaced and contracted together, so rectifying them with your hands, and with the bandaging in like manner, as to bring them into their position not by force but gently, and the bandages are to be stitched so as to suit the position in which the limb is placed, for different modes of the deformity require different positions; and a small shoe made of lead is to be bound on externally to the bandage, having the same shape as the Chian slipper. But there is no necessity for it if the parts be properly adjusted with the hands, properly secured with bandages, and properly disposed of after-

* Loc. cit.

wards. This then is the mode of cure, and it neither requires cutting, burning, nor any other complex means, for such cases yield sooner to treatment than one would believe. However, they are to be fairly mastered only by time, and not until the body has grown up in the natural shape."*

Five hundred years later Galen wrote his commentaries upon this as upon all other passages of Hippocrates, but added nothing worthy our attention. Oribasius† figures the machine of Hippocrates for reducing spinal displacements. The work seems to have been written about A.D. 350. About one hundred years after this we know that the spinal curvature was frequently treated by the production of an eschar‡ on each side of the deformity. The sores were kept running a long time; in fact, a regular issue was established, generally by means of the actual cautery, but sometimes also by caustics.§ It will be well to notice the descriptive accuracy of the Greeks and the verbal redundancies of the language—thus, *κυρτωσις*, *ὕβωσις*, and *κύφωσις*, all mean a humpback from an angular bend of the spine posteriorly; *λόρδωσις*, means a bend forwards; *σκολίωσις*, a lateral curvature. With regard to the extremities, the term *κυλλος*, signifies distorted, lame; *κυλλοποδιων* (an epithet applied to Vulcan), localizes the deformity in the foot, thus meaning crook-footed, club-footed; while the particular mode of distortion is defined by the words *ῥαιβος* and *βλαισος*, which signify respectively an inward and an outward twist of the limbs. Latin writers were very few, and they were but followers, commentators, epitomizers of Hippocrates after the method of the particular sect to which each one might belong. There is by no means the same richness in the Latin as in the Greek language; the word "gibbus" means humpbacked, what is now called "angular curvature;" and the other forms of spinal distortion were defined either by the Greek term or by a sentence in the Latin tongue. The words varus, and valgus or vatius, signify, the first an inward, the last two an outward, bend of the limbs.||

A considerable interval of time has now to be passed over; during about the thousand years that mediæval Rome was in her glory¶ scientific knowledge was at its lowest ebb. Physical investigation was all but tabooed; men's minds were from necessity drilled to authority in medicine, and to think differently from guidance was not possible. The medical works which were written during that period are scarcely one of

* Hippocrates' Works: Sydenham Society's translation, vol. i. p. 632.

† Collecta Medicinalia (*Συναγωγαι ἰατρικαι*) de Machinamentis librum, cap. xxv.

‡ See Aetius. Contractæ ex veteribus Medicinæ tetrabiblos, and other Greek authors of the fourth and sixth centuries. In the Saracenic school the use of caustics and the cautery was frequent.

§ Hippocrates, followed by Galen, had already recommended cauteries in several maladies: the one that comes nearest to our subject is that loose state of parts about the shoulder-joint which permits frequent dislocation of the bone. He blames the ordinary mode of application, and proposes another.

|| It should be observed that these terms were not only applied to deformities, but to postures in standing or in active exercise, to normal conformity, &c. Thus, Ovid, "tenuique a pectore varas in statione manus; et pugnæ membra paravi." Met., v. 33, lib. ix.

¶ This interval is roughly taken from 550 to 1550.

Christian, but nearly all of Saracenic and Jewish origin, and even these mere expansions and argumentations on Hippocrates and Galen. Men in those days used up the store of knowledge accumulated generations before them, as now we are using the fossilized forests of a past æon. We find mention made of several inunctions for deformities under certain sidereal influences; certain charms and amulets;* also relations of cures by some of the old methods—for instance, Actuarius, in the eleventh century, describes the extension of the spine. Albucasis, in the thirteenth,† recommends the establishment of an eschar. Nothing new or noteworthy, however, was done during all these years in the treatment of deformities; they were, particularly if congenital, regarded with a superstitious feeling either of horror, pruriency, or of morbid veneration often opposed to any true spirit of inquiry; and we find in old books on this subject pictures of many composites of human and brute forms analogous to the Centaurs and other creatures of Greek mythology.

About the year 1560 appeared the twenty-second book of Ambrose Paré's works,‡ which is called, 'De Eorum quæ seu naturâ seu ex eventu desunt reparandorum Artibus.' Of this book we are now especially interested in the eighth and eleventh heading; the former is called, "De Gibborum corrigenda deformitate," and is a description of an iron corset, consisting of a carapace and plastoon, in which the body of a crooked-spined person is to be laced. He gives particular directions about the necessity, in growing individuals, of changing these moulds every three months more or less, according to the age and rapidity of growth.

"CAPUT XI.

DE CORRIGENDIS VARIS ET VALGIS.

Of helping those that are vari and valgi, that is, crook legged, or crook footed, inwards or outwards.

"Those are said to bee Vari whose feet or legs are bowed or crooked inwards. This default is either from the first conformation in the womb through the default of the mother, who hath her Legs in like manner crooked or because that in the time when she is great with child she commonly sits with her legs across or els after the child is born & that either becaus his legs bee not well swathed when hee is laid into the cradle or els because they bee not well pleased in carrying the infant; or if hee bee not well looked after by the nurs when hee learneth to go for the bones are very tender and almost flexible as wax.

"But contrariwise those are called *Valgi* whose legs are crooked or bowed

* The only arts which seem to have been freely followed by Christians of this period were magic, astrology, alchemy, &c.; and monks were during the great part of the time the only Christian doctors.

† Methodus Medendi, cap. xliv.: De cauterizatione incipientis gibbositatis.

‡ Ambrose Paré's works appeared at different times from the year 1545 to 1575, and went through several editions; they have been collected and translated into French by Malgaigne, but we conceive rather too freely. An excellent edition in the original Latin is to be found in Uffenbach's Thesaurus. The passage on varus and valgus, quoted in the text is taken from a translation made in 1649 by one Thomas Johnson; we have, however, been obliged to supply in brackets certain important omissions.

outwards this may com through the default of the first conformation as well as the other for by both the feet also & the knees may be made crooked which thing whosoever will amend must restore the bones unto their proper & natural place; so that in those that are *varous* he must thrust the bones outwards as though he would make them valgous (those that are valgous as though they should be varous) neither is it sufficient to thrust them so but they ought also to be so retained there in their places after they are so thrust; for otherwise they beeing not well established would slip back again.

"They must be stai'd in their places by applying of collars & bolsters on that side whereunto the bones do lean & incline themselves, for the same purpose boots may bee made of leather, of the thickness of a testone haveing a slit in the former part all along the bone of the leg & also under the sole of the foot that beeing drawn together on both sides they may be the better fitted & sit close to the leg. And let this medecine following bee applied all about the leg ℞ *Thuris mastich-aloes; boli Armeni, ana* ℥j. *Aluminis rock resinæ* pini, siccæ subtilissimè pulveris, ana ʒiij. farinæ rolat ℥iss; album ovor. q. s. make thereof a medicine. You may also add a little turpentine lest it should drie sooner or more vehemently than necessary. But you must beware & take great heed lest those that were varous or valgous should attempt or strain themselves to go before that their joints be confirmed for so the bones that were lately set in their places may slip aside again and moreover until they are able to go without danger let them wear high shoos (like buskins) tied close to their feet that the bones may be staied the better and more firmly in their places but let that side of the soal of the shoo be underlaid (sit ibi altior) whither the foot did incline before it was restored.*

Another seventy years brings us to Fabricius Hildanus, who, in his work,† figures splints for straightening both the elbow and the knee which have been fixed in a bent position. These machines consist of two splints hinged together lengthwise; a screw lying across the angle at which they are joined serves to open or to close it; these are a good deal like the splints now in use, and which go by the name of Liston or Amesbury. One of them, however, for the knee differs from the others, being formed of a single straight piece of thin iron, hollowed so as to fit the back of the limb; a screw passes through this portion near its middle, and is connected with a ring that encircles the knee, and fixes a pad or shield lying over the patella. By turning this screw, the ring, and of course the joint also, is drawn backwards. Hildanus also gives a case of contraction of the fingers after burns, and points out a method of cure by means of finger stalls, between which and a bandage above, the wrist threads are tightly strained.‡ Fifteen years later—viz., in 1656, we find in Scultetus splints of different forms for straightening crooked limbs, among them one with a ring much like that already described as figured by Hildanus. The same work also contains a mechanism almost identical with that in Oribasius for stretching the spine with ropes, and flattening it with a lever beam. The Torquen, as Nuckius called it, and one or two other modifications, differ in little but that the extension is made from the head of the patient. The slight differences in the means of applying power are not worthy of con-

* The Workes of that famous Chirurgion, Ambrose Parey, translated out of Latine, and compared with the French, by Thomas Johnson. London: Printed by Richard Coles and Willie Du-gard, and are to be sold by John Clarke entering into Mercers Chappell, 1649.

† Observationum et curationum medico-chirurgicarum centuria.

‡ De cicatricum turpitudine earumque ablatione, cap. xiv.

sideration. Also about this time there was used a mode of suspending children so that their own weight produced extension.

"The artificial suspension of the Body is performed by the help of an instrument cunningly made with swathing Bands, first crossing the Breast & coming under the Armpits, then about the Head & under the Chin & receiving the hands by two handles so that it is a pleasure to see the Child hanging pendulous in the air & moved to and fro by the Spectators. Some that the parts may the more be stretched hang Leaden Shoos upon the Feet and fasten weights to the Body that the parts may the more easily be extended to an equal length."*

Moreover a sort of splinting for a crooked spine was thus adapted:

"To straighten the trunk of the Body or to keep it straight they use to make Breastplates of Whalebone put into two woolen Cloaths & Sewed together which they so fit to the Bodies of the Children that they may keep the Backbone upright, repress the sticking out of the Bones & defend the crookedness of them from a further compression. But you must be careful that they be not troublesome to the children that wear them & therefore the best way is to fasten them to the Spine of the Back with a handsome string fitted to that use."†

The management of other deformities not spinal progressed more freely. A case of club-foot is thus reported by Lamzweerde: "*Observatio* lviii.—The wife of John William Balenge, in the village of Langeweck, brought forth on the 7th April, 1658, a male child, with the foot inverted, so that the sole was above, for the metatarsus was bent downwards, so as to be underneath. Having seen the foot, we soon turned the metatarsus, and having adapted splints and bandages, reduced it to its natural situation without any inconvenience."‡

About this time, or rather, indeed, a little earlier, an attempt in an entirely new direction was made for the cure of a deformity. The case was one of wry-neck from contraction of the sterno-cleido mastoïd. A surgeon, Isaac Mincius, being called in, destroyed the skin in that situation with a caustic, and divided the muscle.§ The operation was not entirely successful, owing apparently to want of care in keeping the head turned away from the affected side, yet sufficiently so for Tulpius to give the name of the surgeon, that others similarly affected might be relieved. A few other surgeons, among them Cheselden,

* Glisson de Rachitide, translated by Nich. Culpepper. 1651. † Op. cit. p. 321.

‡ Chirurgiæ veteris ac modernæ promptuarium tabulis viginti novem exornatum. Instrumenta varia eorumque usum exhibentibus necnon Observationum Medico Chirurgiarum centuria illustratum Operâ et studio Johannis Baptistæ à Lamzweerde, Phil. et Med. Doct. Amstelodami, apud Johann, Somere, Bibliopolon sub insigne Perkins, 1672.

§ We owe to M. Vidal de Cassis' 'Pathologie Externe,' this reference to Tulpius; Observationes Medicæ, Lib. IV. cap. lvii. p. 372, 1685. The following is the physician's account of the operation: "Factum autem fuit curationis initium a crustâ, per lixivium coctum, cuti inustâ. deinde vero directum scalpellum, supra claviculæ os ab ore versus jugulum. at tardius certe ob crustæ lentitudinem, ac timidius propter venarum, arteriarumque jugularum viciniam, quam naturâ tendinis, ac nervorum requiret, quibus iccirco magis punctim, quam cæsim læsis, sequabatur protinus vehemens cervicis, faciei, ac brachiorum convulsio, quæ tamen brevi iterum evanuit, adacto penitens scalpello, et præciso animosius, integro, qui invitio erat musculo, cujus amputatio capiti astutum restituit integrum erectioris figuræ usum, ac libertate sese quocunque locorum movenda. qua tamen ne abuteretur, circumpositæ capiti fuere variæ fasciæ, quibus deinde, ob vulnus brevi consolidatum, amotis, permansit tamen aliquandiu inveterata illa inclinatio in latus affectum, adeo difficile est longam corrigere consuetudinem."

adopted this proceeding with some modifications, so that the example was not allowed to die, and we find it fifty years afterwards recorded as one of the rare operations of surgery.

"The operation of cutting the wry-neck is very uncommon, and is never to be practised but when the disorder is owing to a contraction of the mastoideus muscle only, as it can answer no purpose to set that muscle free by dividing it (which is all that is to be done), if the others of the neck are in the same state, and more especially if it has been of long standing from infancy, because the growth of the vertebræ will have been determined in that direction, and make it impossible to set the head upright.

"When the case is fair, the operation is this. Having laid your patient on a table, make a transverse incision through the skin and fat and something broader than the muscle, and not above half an inch from the clavicle, then passing the probed razor with care underneath the muscle, draw it out and cut the muscle. The great vessels of the neck lie underneath, but I think when we are aware of their situation, the danger of wounding them may be avoided. After the incision is made, the wound is to be crammed with dry lint, and always dressed so as to prevent the extremities of the muscle re-uniting, to which end they are to be separated from each other as much as possible by the assistance of a supporting bandage for the head, during the whole time of the cure, which will generally be about a month."*

The method was not at this time transferred to any other muscle—indeed, its performance at the neck seems to have been regarded rather as a curiosity than as a commendable proceeding, and it had therefore no effect upon the treatment of deformities in general, which were managed entirely by bandages supporting ivory splints, and such other means as we have seen described by Paré, Hildanus, and others. It is to be observed that the principles whereon the treatment of the crooked spine is founded, are occasional extension, combined with more or less constant support, the former by the extending couch and lever, or by hanging up the patient, the latter by an iron corselet, or by splinted stays. It may be doubted whether many of the patients, particularly those whose deformity was of long standing, were much improved by these methods, but sometimes, as in cases related by Hildanus, Lamzweerde, and others, the disease was much alleviated or cured, for much in all surgical matters depends on the manual skill and adaptive power of the individual surgeon. We may be sure, however, that no great improvement in the general mode of treatment was at this time invented. Men at the end of the seventeenth and in the eighteenth century were by no means untainted with the cacoëthes scribendi — witness the ponderous volumes of that date. If any means having the slightest claim to novelty and efficacy had at that time been invented, we should certainly be able to find upon the subject either a quarto or folio, having a frontispiece representing Minerva or Apollo with a flute or spear in one hand and a surgical instrument in the other, leaning

* A Treatise on the Operations of Surgery. By Samuel Sharpe, Surgeon to Guy's Hospital. Sixth edition. 1751. Chap. xxv. p. 202. It may be remarked, that what Sharpe calls a probed razor is termed by Scultetus and others a syringotome; it is not in the least like a modern tenotomy knife, but is a broad-bladed crooked implement of very awkward construction.

negligently against a column, beneath flowing curtains, so looped up by Louis Quatorze tassels as to display a grove of trees and a church-steeple in the background. It is only after an interval of about eighty-five years from Isaac Mincius' operation that we find anything note-worthy in the treatment of deformities. The first is a machine, described by Heister;* it was a cross made of thin steel, to each of whose four extremities a leather band was fixed—the upper and lower for the neck and pelvis, the two lateral ones for the shoulders. We might take occasion to observe upon this that it is a fair type of many such contrivances, very ingenious as far as the mechanical idea is concerned; yet a patient who could wear this cross must be tolerably straight, for if very crooked would be infallibly strangled. The most important step about this period is a treatise, which appeared in France in 1741,† by M. Andry, which he calls Orthopædia; there had previously appeared a couple of poems—one called 'Pædotrophia,' the other 'Callipædia'—in that queer pseudo-classical taste of the time, which renders it difficult to ascertain whether the writers believed most in Olympian counsels or the College of Cardinals.‡ It is evident that Andry compounded the word whereby he named his work in imitation of these predecessors. It is, we believe, the first use of a term which since then has become familiar, but whose formation and meaning the author felt himself bound to explain:

"As to the title in question, I have formed it from two Greek words—namely, from Orthos, which signifies straight, free of deformity, according to rule, and from Paidion, which means child. I have composed from these two words that of Orthopédie, to express in a single term the plan of this work, which is to teach different methods of preventing and correcting bodily deformities of children." §

The means which M. Andry proposes are, for infants, certain inunctions and bandages, which confine or compress different parts of the body; for older children, the avoidance of all things tending to increase, and the substitution of such as may diminish, an awkward habit—such as the discontinuance of low tables which cause a stoop, and the substitution of higher ones; also certain exercises, such as carrying a weight upon a shoulder that is higher than the other, or a book or other small object on the head which has a tendency to incline to one side; occasionally he recommends a splint as for crooked legs, saying that "the same means must be taken to straighten them as are adopted for straightening the crooked stem of a young tree;" ‖ also for feet turned inwards or outwards he recommends "splints of strong cardboard or of wood, or little plates of iron, for they are much better than all the boots that are usually employed in these cases."¶

* Chirurgie, 1739.

† L'Orthopédie; ou, l'Art de prévenir et de corriger dans les Enfans les Difformités du Corps le tout par des moyens à la portée des Pères et des Mères, et de toutes les Personnes qui ont des Enfants à élever. Par M. Andry, Conseiller du Roy, Docteur et Professeur en Médecine au Collège Royal, Docteur Régent et ancien Doyen de la Faculté de Médecine de Paris, &c. Avec Figures. 1741. Avec approbations et privilège du Roy.

‡ Scævola [Ste. Marthe], the author of the first-named work, counsels the woman in labour to invoke three times, with a loud voice, the goddess Lucina.

§ Op. cit., preface, p. ii. ‖ Op. cit., vol. ii. p. 282. ¶ Op. cit., p. 286.

This date—viz., 1741—forms a sort of landing-place where we may turn and advantageously look back upon the steps we have climbed. The means, whose adoption one after the other has been traced, are numerous; mechanical extension, artificial sores produced by caustic or the actual cautery, division of contracted muscles, splints, bandages, mechanical shoes, or other means of artificial support. We find one or the other of these applied to a crooked spine, to a distorted foot, to an ankylosed joint, to a wry neck, or other deformity; and at last we find gymnastic exercises established as curative and preventive treatment. Let it, then, be remembered, that one hundred and twenty years ago all these means were used; it may be that the mode of applying the principles were rude, and the mechanism awkward, but many a case of slight distortion, particularly if attacked young, was thereby cured, time and perseverance being accorded; severe cases, and those which had been allowed to continue to a late period, were probably seldom, if ever, much benefitted by such means.*

We shall now trace the advance of another hundred years. We cannot always go directly forwards in a straight line, since we must turn to regard the progress in spinal treatment and that in limb treatment in different stages, according to their advancement. First, as regards the spine. In the year 1779, Percival Pott, of St. Bartholomew's Hospital, published his 'Remarks on that Kind of Palsy of the Lower Limbs which is frequently found to accompany a Curvature of the Spine.' The whole object of these remarks is to recommend the formation of an issue on each side of the hump by means of caustic—a plan of treatment to which he accorded such confidence as to say of it, that he desired

"That as little time as possible might be lost in conveying to the profession in particular, and to mankind in general, the means of relief under an affliction which, till these were known, has not admitted of any."†

And further on,

"That the patients whom I have attended in the early part of the distemper, of whatever age, have all got well; that is, have all not only regained the use of their legs, but have become fit for any exercise and labour. Most of them have become much straighter, some quite straight, and all of them perfectly free from all kind of inconvenience resulting from the curve."‡

This pamphlet attracted great attention not only in England, but also abroad, and has had considerable influence on the treatment of such cases ever since. In France the malady still goes by the name of mal vertébral de Pott, and we find this nomenclature still retained by the latest French writer on the subject, M. Bouvier.§

In the same year in which Pott wrote his 'Remarks' above quoted, M. David, of Rouen, published a book called 'Dissertation sur les

* The division of contracted muscle being confined to the wry-neck, was, as we have seen, a rare operation, and was performed in such a way that the length of time employed in healing must have been considerable, and that contraction was likely somewhat to recur.

† The reader will remember our reference to Aetius, Albucasis, &c.

‡ Loc. cit., pp. 32, 37.

§ Leçons Cliniques sur les Maladies de l'Appareil Locomoteur.

Effets du Mouvement et du Repos dans les Maladies Chirurgicales,'* in which the author remarks:

"As for the bones which are primarily affected, they are no sooner relieved of these *débris* than they begin to resume their solidity; and if several vertebræ have participated in the disease, they form themselves into one common mass of ossification, thus terminating this *grand cure*, which, as we see, must be *the work of nature, of time, and of repose.*"

It is certainly to be regretted that a work containing views so true, and apparently so reasonably expressed, should have remained so utterly unknown as to have produced no effect whatever on the current treatment of spinal disease. On the other hand, Pott's method (but a revival of the Greek and Saracenic treatment), which was practised everywhere, and still influences to a very great degree the management of spinal diseases, was not entirely unopposed. Benjamin Bell, eleven years after the appearance of Pott's pamphlet, remarks of the treatment:

"This I have practised in various cases, and in some instances with obvious good effects; but in all of these there was reason to suppose that the seat of the disorder was in the ligaments, and not in the bones of the spine. When they have appeared to prove useful where the bones have been affected, I conclude that the mitigation of symptoms has arisen from the cause I have mentioned—the pressure upon the spinal marrow becoming lessened in the progress of the disorder."†

In 1784 was published Le Vacher's account of his machine, consisting of a corsage strongly beset with whalebone, and carrying a metal socket, in which rides a staff. This staff being crooked, overhangs the head, supports a portion that grasps the forehead and occiput, and can be more or less gradually lengthened or shortened in its socket by means of a ratchet and lever, thus supporting all that part of the spine between the socket and head; but, be it observed, by means of the thorax. In 1794, Schmidt of Marburg described an instrument of his own invention. A metal band passing round the *crista* ilii supports two branches, which run up to below the armpits and terminate like crutch handles, being kept in place by another band of metal that runs round the back of the shoulders.

Mr. Darwin invented two machines, the one for a sitting, the other for a recumbent person—the former consists of an arm chair with a square upright back, to each arm of which is fastened a crutch-handled support, which goes under the axilla of the patient, whilst to the back is affixed a crooked iron staff, from which hangs a well-contrived apparatus for grasping the head and partly supporting the weight by the neck. The other is only to be used where the patient is lying or sleeping on a bed, which should slope from the head to the foot about twelve or sixteen inches; a machine which supports the head by the jaw and occiput, prevents the patient sinking down in the bed, and keeps up a constant though

* The title of the work and the quotation is taken entirely upon the authority of M. Bouvier, op. cit., p. 43.

† B. Bell: Surgery, vol. vi. p. 299. The cause which the author refers to is a slow decrease in the suddenness of the bend, from the vertebræ above and below becoming participators in the curve, thus producing a less amount of pressure on the cord.

gentle extension on the spine, the aim of the inventor being to act upon the ligaments of the spine while the muscles are at rest.*

We find here the application of four principles—extension, support, rest, and counter-irritation—all aimed at by different means and machinery. Since the beginning of the present century, the mechanism whereby it has been attempted to carry out these indications has been sometimes improved; sometimes actual retrogression to the faults of the most ancient methods has taken place. It would lead us much too far to describe, or even to enumerate, the various contrivances that have been invented. Almost every writer on spinal disease has a new application of the machinery for supporting the trunk in the erect position, all more or less founded upon Glisson's splinted stays or Schmidt's apparatus with certain additions. Chessher had his collar; Shaw, Verral, Tamplin, Heine, Guérin, &c. &c., each had a somewhat differently constructed apparatus. The supports consist primarily of a steel portion, which, embracing the pelvis, acts as the foundation for the whole structure, and from which certain uprights run up the back or along the sides. These latter carry crutch handles that lie in the axillæ, also certain spring pads to press on the convex side of the curve. Mr. Chessher appears to have stretched the trunk with a windlass, and then to have fastened his collar and support upon the back while the extension continued, so that the patient could walk and move about with a constantly stretched spine.

Some fifty years ago, Dr. E. Harrison began to treat spinal curvatures on the old dislocation fallacy, by violent extensions, dragging his patients by pulleys up to the ceiling, and by severe pressure, shampooing, thumbing, &c., on the excurvated part; we are ignorant if he, like Hippocrates, recommended his patients to be sat upon.

The mechanical couches now in use are chiefly adaptations of Darwin's principle, the inclined plane. Shaw's couch is inclined, and the flat portion on which the patients lie consists of two parts, the upper one fixed, the lower capable of sliding down on the framework of the couch; the patient lies with the shoulders on the upper, the loins and buttocks on the lower part of the couch. When this latter is released, the tendency to slide down produces a certain amount of extension which can be increased by the addition of weights, while a head-stall may be applied, which prevents the patient being dragged down, and causes the extension to be more evenly distributed throughout the column. Guérin's couch is also a plane slightly inclined, though its inclination is not meant to produce extension, for which purpose a windlass and straps fastened to the loins are used, the head being confined in a complicated apparatus. The plane part of the couch consists of three portions, the upper and lower of which can be turned on pivots to the right or left, so that each curved part of the patient's trunk may be screwed into a shape contrary to that assumed by the deformity. Among the most useful inventions in couches is Verral's prone couch for angular curvature; in this mode of treatment the patient is kept

* Zoonomia, vol. ii. p. 90, 4to ed. 1796. We could wish that this book were better known at the present day; it contains the results of much acute and original thought.

lying on his stomach, with his head in a hole, for some months—that is, until in favourable cases the curvature has healed; the position seems at first sight unbearable, but patients generally affirm it to be comfortable.

The condition in which was the surgery of distorted limbs forty years after the publication of Andry's book, may be judged from the fact that one of the most eminent surgeons, in 1781, stated that, in spite of the undoubted merit of certain mechanisms, he had been very seldom able to reduce a club-foot, and then only in young children.* A little previous to this date, however, there arose almost simultaneously in England, France, and Switzerland, certain specialists who devoted themselves to the cure of club-feet. In the first country Jackson, in the second Tiphaisne and Verdier, boasted and also advertised great success in the treatment of that deformity. After the manner of quacks, they kept their procedure secret. It is certain that they acquired considerable notoriety; but it is also certain that they did not by any means cure all whom they pretended to have restored. Venel, of Canton Berne, was a man of somewhat different stamp; he was a regularly educated practitioner, clever in machinery, and humane in character. He established, not far from the town of Berne, an institution in which he received children under seven years of age, having distorted feet, and treated them according to a method which, unfortunately for his reputation, he kept strictly secret. There is no doubt that he intended ultimately to publish his mode of treatment had not death been beforehand with him. In the meantime, however, the number of his ascertained successes had been sufficient to excite great interest in Germany; and one of his patients, son of a merchant in Frankfurt, was examined by Dr. Ehrenmann, and was observant and ingenious enough to be able to give a description, even to make a cardboard model of Venel's machine. Ehrenmann had an iron one made. Brückner of Gotha saw this instrument, and copied it, and Naumburg of Erfurt procured a fourth imitation. All three surgeons treated several cases of club-foot with the implement. Brückner founded on such treatment his work 'Ueber einwärts gedrehte Füsse,' 1798, and Naumburg his 'Abhandlung über Verkrümmungen,' 1796. This imitation of Venel's machine was thus used:—the foot was first fastened in a leather buskin with a strap attached; then while thus covered it was placed in an iron box, and the strap assisted in holding the limb immoveable. The iron box or shoe is a very complicated apparatus with a staff or lever to run up the leg, and is composed of moveable plates, screws, &c., which gradually squeeze the part into a normal shape, while the staff, turning the foot on its long axis, causes the sole to face directly downwards instead of inwards. One great feature of the treatment was the slowness with which it was commenced, the machine being at first applied only for an hour, and with very little tension daily; then both time and force were gradually increased until the child could bear its application during the night. It generally took two years to complete the cure either by this method or

* Camper: Sur la meilleure espèce de chaussure.

those of Jackson, Tiphaisne, &c. The instruments were either not such as permitted locomotion (and the imitation of Venel's certainly not), or jealousy for the secrecy of the treatment caused the proprietors to forbid the patient walking about.*

The success of these men, although doubtless a good deal exaggerated, was sufficient to stimulate a large number of persons to work at machines for the cure of club-foot. Benjamin Bell invented a useful apparatus; also Mr. G. Wilson, a mechanist; Leutin, in Germany, also,† and several others used machines of more or less applicability. But one great fault of them seems to have been that they aimed only at turning the sole of the foot downwards, and took no notice of the bend in the intertarsal joint.

A little time afterwards an ingenious mechanician and truss-maker, Mr. Sheldrake, published an essay on club-foot.‡ In speaking of the defect in the instruments then in use, he animadverts upon their weight, the difficulty of the child's moving about, its consequent loss of health and power, and continues:

"As so much has been said of the defective principles upon which these instruments are constructed, it would seem invidious to enlarge upon the various ways in which they are frequently misapplied; but it will be permitted me to observe, as the effect of near twenty years' observation and experience, that although these instruments are constructed upon principles not totally inapplicable to the diseases they are intended to cure, they are so inadequate to the purpose, that not one patient in twenty upon whom they are applied in the common way derives any benefit from the use of them."

Mr. Sheldrake then says that he has constructed an instrument which answers every purpose:

"The idea upon which this method is founded is to substitute a spring so adapted to the nature of the distortion, that when bound upon the limb its action will draw the deformed parts into their natural situation; when it is necessary to allow of motion in the limb, that motion, by increasing the reaction of the spring, accelerates the cure."

In 1803, Scarpa's monograph, 'Sulle Piedi Torti,' appeared. The condition of parts and the indications of treatment are admirably described. The machine which he invented is still used, and named Scarpa's shoe, and is a very valuable instrument. We are bound to observe that he appears to us to have taken the outlines and plan of his instrument from Brückner's description of Venel's apparatus, but that he substituted spring power for the screw force, an idea which, if we may believe Malfatti, he derived from Tiphaisne. Malfatti was a surgeon of some note, and had taken much interest in the treatment

* This instrument of Ehrenmann, Brückner, &c., was, as we shall see in the sequel, very different from, and much clumsier than, that which they attempted to copy. The lever or staff has, ever since Venel's time, been an essential part in all the machines for the reduction of club foot.

† See Bell's Surgery, vol. vi. p. 290, plates lxxxii. and lxxxiii., Ed. 1788; and Leutin's Beiträge zür ausübenden Arzneiwissenschaften, &c. Bell, in his work, blames surgeons for giving up deformities as incurable and leaving them in the hands of bone-setters and quacks.

‡ Observations on the Causes of the Distortions of the Legs of Children, and the Consequences of the Pernicious Means generally used with the intention of Curing them. By T. Sheldrake. 1794.

of deformities. When Scarpa's instrument was first invented, Malfatti went to Italy to visit him. On the latter's return he translated the Italian work into German, and in a preface relates the following history, which he affirms to be according to the author's (Scarpa's) own assertion:

"In the year 1781, during his residence in Paris, he passed, by chance, Tiphaisne's door, which was hung around with pictures of sundry monstrosities and deformities. He learnt that these were taken from the feet of children whom Tiphaisne had perfectly cured, and immediately he tried to make acquaintance with that specialist. Scarpa saw him frequently; but his oft-repeated visits and questions extracted nothing from one so jealous of his secret as Tiphaisne, except on one occasion, when he said, 'nature will not yield to violence, but only to a gradual force'—a phrase which still further stimulated the Italian's desire to see the instrument. After many useless attempts he at length succeeded, by means of the housekeeper, in penetrating for a few minutes into Tiphaisne's inner room where the patients were treated, but this only after giving his word of honour that as long as Tiphaisne lived he would neither say nor write anything on the subject. Scarpa, however, found nothing more than a steel spring lying on a cushion. This single part of the apparatus was enough to enable a man so thoroughly acquainted with anatomy to construct (after a few experiments on spring-power) the present shoe, which, if not exactly like that of Tiphaisne, is hardly likely to be inferior to it in efficiency and perfection."*

It is not our intention to check our course in order to describe fully this instrument, as it is now very well known; but it seems desirable to point out that it consists essentially of two springs: one running along the outer side of the foot in varus tends to unfold its bend in the medio-tarsal joint; the other, running from the shoe up the leg, drags on the outside of the distorted extremity, and so causes the sole to face directly downwards. One very essential part of the construction is that it permits the patient to walk about, and so does not injure his health—a principle upon which Sheldrake (with whose pamphlet Scarpa was doubtless unacquainted) had already insisted.

About this time there commenced in France a perfect fury for running after deformity curers; part of this was mere fashion and part of it was caused by the prevalent form of dress, whose peculiarity was a remarkable lack of covering, rendering very apparent every defect of form: thus any young lady, whose spine was a little bowed or whose knees turned too much in, was anxious to have such an exposed defect remedied. At first most of these persons sought relief in Germany, where the imitation of Venel's treatment had produced several institutions of similar sort, and where the treatment by Scarpa's shoe was zealously followed.

In 1813, however, a M. d'Ivernois, who seems to have been a relative of Venel's, set up a so-called Orthopædic, or, as he liked to term it, an Orthosomatic establishment. He had studied under Venel and possessed his machines; that for the foot is evidently very different from and much lighter than Ehrenmann's and Brückner's. We will not describe it more particularly here than by saying that it consisted

* Malfatti's German translation of Scarpa's work: Sulle Piedi Torti, Preface, p. iii.

of a sole of wood, along the outer part of which ran an iron rim pierced with holes giving passage to straps that drew the inverted foot outwards and unfolded the medio-tarsal bend. The lever, of course, remains the most essential part of the apparatus.* M. d'Ivernois was successful, and soon had, of course, a host of imitators, among them a M. Milli, who had copied a machine for stretching the spine from an institute in Würzburg, belonging to Herr Heine, and who enjoyed some repute in Paris at the beginning of the century.

We have now traced the treatment of club-foot by machinery to the very height of its perfection. The bandage of Hippocrates, and the moulded leather shoes, the irons, and the splints, we have seen gradually replaced by elastic spring-power, applied in different directions so as slowly to unfold the contracted parts and gradually to turn the extremity into the proper position for walking;† beyond this point it appears that machine treatment was not to be carried. Let us now examine the progress of another system, which was destined ultimately very much to overshadow the instrumental.

We have already seen that for the treatment of one species of deformity—viz., the wry neck, division of the contracted muscle had, since 1652, been occasionally performed. Thelenius, in the Duchy of Saxony, extended this idea, and in a case of equino-varus divided the tendo-Achillis and the skin together on the 26th March, 1784. The wound was entirely healed on the 12th May, and in 1789 the surgeon, in publishing his observations, says, "the patient can walk properly like other people." On the 10th May, 1806, J. F. Sartorius saw a case of distorted foot produced by abscesses, &c. On the 16th he divided the Achillis tendon by making a longitudinal incision four inches long over its middle, opening the cellular sheath, passing a bistoury beneath it, and cutting from within outwards; the foot was not returnable to a proper posture; he divided some scars in the neighbourhood.

"I then ordered my assistants to hold the limb firm, and used all my strength at once to bend the foot upwards, which I succeeded in doing, but caused at the same time such a rending and cracking as though every bone were broken. The patient screamed terribly, but the violent pain soon decreased."‡

Sartorius's description ends thus:

"May this remarkable case, which terminated so favourably, encourage surgeons to show in similar conditions the same boldness and determination that I had; perhaps many similar patients might, by such a procedure, be cured of their deformity."

Michaelis adopted a somewhat different idea: he endeavoured to

* A description of this machine may be found in the Encyclopédie Méthodique, art. Orthopédie. The article is signed Bricheteau and d'Ivernois; the latter is evidently the chief author; he speaks of Scarpa's shoe contemptuously as a heavy and complicated apparatus, "with which it is difficult to believe that any other than that celebrated surgeon should ever have cured a club-foot."

† It is hardly necessary to observe that at the same time the morbid anatomy of the part became better and better understood.

‡ Siebold's Saml. seltener und auserlesener chirurg. Beobacht. Band iii. s. 258; Stromeyer's Beiträge, &c., p. 39.

weaken the contracted tendons by cutting them partially through, so that they might then yield more easily to a stretching power. His first operation was performed on the 18th November, 1809; in little more than a year he had cut eight cases. He gives no account whatever of his procedure; and it may be very doubtful, in spite of his assertion, whether he did not entirely divide the tendon.*

Notwithstanding these many experiences and the absence of any fatal case, or, indeed, of any case which had not been improved by the operation, the procedure fell again into desuetude for several years, until Delpech, of Montpelier, had recourse to it in 1816.† Even this revival exercised little immediate effect. Some of the Paris journals, not in the least aware that the tendo-Achillis had ever been divided before, blamed Delpech very much for his attempt, and spoke with astonishment that any one should have seriously proposed such an operation. Thus, although the case was successful, Delpech never adopted the procedure again. Nevertheless, he reproduced, in 1828, an account of the case, his reasons for performing it, and such clear and succinct views concerning the mode in which section of the tendo-Achillis proves useful, that we must transcribe them. Moreover, his mode of operating was novel, and very much like that adopted at the present day. Delpech says:

"1st. The tendon to be divided must not be laid bare; its section must be done by a détour, and not by an incision through the skin parallel with that in the tendon. Without taking this precaution the dangers of exfoliation would be incurred.

"2nd. Immediately after the section of the tendon its ends should be placed in mutual contact, and they should be maintained in that position by a suitable apparatus all the time necessary for their reunion.

"3rd. Reunion can only take place by means of an intermediate fibrous substance (*tissu inodulaire*) which, before its solidification, can and ought to be submitted to a gradual and careful extension that shall give it the length required to make up the shortness of the muscle.

"4th. When this amount of extension has been gained, the parts are to be immovably fixed in the attitude thus established, and are to be thus confined until the new substance has acquired its full solidity."‡

The account of his operation runs thus:—

"The patient reclining prone, so as to present the tendo-Achillis in full view, we plunged the blade of a straight bistoury in front of this tendon, making it pass quite through from the inner to the outer side of the leg, so as to divide the skin on both sides to the length of about an inch, and the cellular tissue in front of the tendon. This instrument was immediately withdrawn and replaced by a bistoury of very convex form, whose edge was directed backwards towards the tendon, which was divided transversely without injuring its cutaneous covering."§

The reason why Delpech chose this method of procedure is thus described:—

* Hufeland and Heintz: Journal der pr. Heilkunde, Band vi. Stuck v. Seite 1; and Stromeyer.

† The history of the case was first published in the Chirurgie clinique de Montpelier, 1823; afterwards in his Orthomorphie, 1828.

‡ Orthomorphie, vol. ii. p. 330. § Chirurg. clin. de Montpelier, tome i. p. 184.

"The only thing which it appeared to us was seriously to be feared was mortification of the divided tendon. . . . . Although the exfoliation of a tendon which has been laid bare is not always produced by the contact of air, we had frequently observed it; this agent was at least an additional stimulant which it seemed prudent to avoid. It was with such design that we performed our operation so as not to involve the skin which covers the tendon; and the slight exfoliation that followed in spite of our care, entirely confirmed our fears and justified our method."*

It certainly is strange, that having clearly reasoned out and carefully thought over every point of the procedure, its consequences and possibilities, Delpech should have abandoned, not the theory, but the practice of this operation. Between the one book which we have quoted and the other is an interval of five years; and in 1830, this acute and learned surgeon was murdered at an age too early to see his operation revived, and at the same time his honour vindicated, by the ingenious vigour and the honourable straightforwardness of a Hanoverian surgeon.

There is no doubt that the various sections of muscles and tendons, from that of Isaac Mincius downwards, had produced a certain small amount of individual advantage; and though to the world in general they had been unavailing, yet the surgeon who possessed sufficient knowledge and research to be aware of these cases, must have been greatly emboldened in any such attempt he might desire to undertake. The honour of the last and permanent revival belongs undoubtedly to Louis Stromeyer, who by no means detracted from his own deserts in fully acknowledging those of his predecessor. Delpech, he says, entirely showed the aim of the operation (the establishment of an intermediate substance which might be stretched), and admirably developed the theoretical grounds upon which the mode of procedure is founded. But—

"Delpech's method of operating did not quite fulfil the indications (the exclusion of air) which he himself proposed, and without doubt he conceived these after he had been taught by this case what were the dangers in section of the Achillis tendon. In repeating this operation he would probably have chosen another mode of procedure."†

"The tendons of the resisting muscles are then to be cut, if they can be reached without much difficulty, but if not, the muscles themselves may be divided. These sections must, if possible, take place under the skin. For such purpose the narrowest bladed instruments, of various form, are to be chosen; a bistoury with moderate curve generally answers the purpose. Such sections are usually most easily performed when the limb is so held that the part to be divided becomes prominent; the knife is then to be passed behind that structure, is to form a puncture of exit (Ausstichpunkt), and then is to divide the tightened organ, chiefly by pressure, but also by a careful slow stroke of the blade. The skin yields and follows the knife, so that the external wounds have only the breadth of the instrument. I have, however, often divided the

* Op. cit., p. 229.

† Beitrage zur Operativen Orthopädik, p. 59. The writer of this article cannot agree with Stromeyer in the last sentence. If Delpech had not intended to avoid the entrance of air (the indication in question), why should he have so carefully spared the skin over the tendon. Moreover, Stromeyer chose for his operation the same place for the entrance and exit of the knife, dividing the tendon in the same direction. The only difference, one whose effects are considerable, is the size of the external wound.

Achillis tendon without making a puncture of exit, whereon I lay no great stress, because two little wounds get well just as quickly as one. Generally a cracking sound announces the successful termination of the operation.*

Stromeyer's first operation was performed on the 28th of February, 1831; his first paper on the subject appeared in 'Rust's Magazin,' in 1833; he relates two cases, and of his remarks upon them we will quote the following:—

"Among the changes which I have introduced into Delpech's method, the most noteworthy appears to me to be in the mode of dividing the parts. The advantages of these changes are very apparent, as in both my cases the small wounds united by the first intention, and extension of the intermediate substance could be begun on the tenth day, whereas Delpech could only commence on the 28th day. Also the scars of the skin formed no junction whatever with the tendon, a point to be considered, for if they do so, the skin will be dragged upon very much, while in walking the tendon glides up and down, and may cause considerable trouble. In both cases it was very remarkable how evenly the intermediate substance was developed, so that it is so little thinner than the tendon itself, and that it is only by the aid of the external wound that this slightly attenuated portion can be detected, a fact whereof my colleagues in the institute perfectly satisfied themselves. This circumstance, which did not occur in Delpech's case, is, I believe, due to the mode in which my section is made, whereby probably the posterior wall of the sheath is uninjured, and forms a sort of mould for the exsuded lymph.

In 1834, Stromeyer published his second treatise on section of the tendo Achillis simultaneously in 'Rust's Magazin' and in the 'Archives Générales de Médecine.' In consequence of these papers, several German, Alsatian, and French surgeons adopted this operation. In 1836, Dr. Little was sent by Dieffenbach from Berlin to Hanover. Stromeyer operated upon him successfully. Dr. Little then treated some patients under Dieffenbach's eye in Berlin; the Prussian surgeon became enamoured of the operation, and was one of its zealous advocates. In 1837, Dr. Little came to London, and by his means the procedure was imported into England, and the Orthopædic Hospital established. In 1838, Dr. Stromeyer published his work from which we have already quoted, whose translated title runs 'Contributions to Operative Orthopædia, or Experiences of Subcutaneous Section of Shortened Muscles and of their Tendons.' In this work the author carefully goes through all the causes which led him to adopt the operation of dividing the tendo Achillis, and gives about two dozen cases of this operation. He then describes its extension to other muscles, and to other parts of the body; gives a short account of the pathological conditions present in false ankyloses; shows how, after section of muscles, it becomes comparatively easy to straighten the joint, and records seventeen cases of this procedure.

Armed with so powerful a means for the reduction of malposition as division of all resistant parts, it is not astonishing that the treatment of deformity should have suddenly attained a much greater development than we have found it to have hitherto reached. In England, Dr. Little, Messrs. Lonsdale, Tamplin, and Bishop; in France, Bouvier,

* Op. cit., p. 18.

Duval, Guérin; in Germany, Stromeyer, Dieffenbach, and many others, carried, in eight or nine years after Stromeyer's first treatise, myotomy and tenotomy at least to their legitimate limits.

In France, one of the most prolific writers claims to be the originator of the method, or at least to have established it; but we find in M. Guérin's writings so slight a mention, or indeed, total ignoring of what his predecessors had done, that we must conclude he only heard reports of their labours, and never read either Delpech's or Stromeyer's works. We do not wish for one moment to deny M. Guérin's merits as a clear and logical writer, nor as a bold operator, extending the practise of myotomy to a number of muscles never attempted before or since, but the claim which he puts forward to be the originator of a "subcutaneous method" cannot be allowed.*

The spread of subcutaneous tenotomy became, after 1837, as rapid as its discovery had been previously unaccountably delayed. A number of names attach themselves in one way or other to this form of procedure, either as great and successful operators, or as careful investigators into the processes whereby tendons heal. It is unnecessary to give a list of such names here. To Dr. Little in England and Germany, and in Paris to Duval and Bouvier, belong the undoubted priority of taking up and extending Stromeyer's practice. It should be observed, however, that Little was only a short time in Germany, and acted only as a conductor between Stromeyer and Dieffenbach; this latter took up the operation, gave it great extension, adapted it to cases of strabismus, and applied it more largely to contracted and distorted joints.†

* We find in none of M. Guerin's writings any mention of tenotomy until the year 1839, in his Quatrième Mémoire sur les Difformité du Système Osseux (Stromeyer's book was published in 1838, his first memoir in 1833): in it we find cases in which the tendo Achillis, the tendon of the flexor pollicis longus, extensor pollicis tibialis posticus et anticus, the biceps semitendinosus and semimembranosus femoris, sartorius, pectineus, biceps humeri, sterno-mastoïd, clavicular part of the trapezius, the plantar aponeurosis, the fascia lata were divided. Yet in 1839 M. Jules Guérin, in the memoir above mentioned, after giving a very clear description of congenital club-foot, and of the different muscles affected, observes: "Therefore now as we know the number and effects of the retracted muscles for each variety of club-foot, now that we know that such and such form depends upon the retraction of this or that muscle, it is unnecessary to show that to each of these muscles, according to its importance and the amount of its resistance, we ought to apply an operation confined hitherto almost exclusively to the tendo Achillis." (Mémoire sur les Variétés Anatomiques du Pied Bot, p. 48.) Again, in his Essais sur la Méthode sous-cutanée, we find: "I established as a principle that wounds made under the skin and maintained out of all contact with the air do not inflame nor suppurate, and become organized immediately. I have proved this principle by means of a great number of experiments upon animals and of operations on the human subject. I have made it the foundation of a general method, whereof I have indicated and realized a great number of applications." (p. 8.) Hunter is here entirely ignored, and two pages further a very sorry attempt is made to depreciate Stromeyer; it being asserted that the inventor of subcutaneous tenotomy had merely modified a proceeding of Delpech in order to avoid suppuration and exfoliation of the tendon. Every one long ago knew that wounded tendons exposed to the air suppurate and exfoliate; and that Delpech, Stromeyer, and all their followers, were guided by this principle. We quite acknowledge, with M. Guérin, that the avoidance of suppuration and sloughing of tendons is the end aimed at by the subcutaneous mode of incision, both by Stromeyer and his followers, and that this could be done was practically demonstrated in 1833. The only thing, then, in this subject of subcutaneous wounds which we can allow to be M. Guérin's own, is the notion that they do not inflame; and that notion is a mistaken one.

† Guérin conceived that it might be useful in cases of lateral curvature of the spine, and in one or two instances divided a great number of the dorsal muscles; the practice, however, has not been justified by experience.

We have already seen that Hildanus and Scultetus and others employed powerful machines to straighten crooked joints; their use continued in a more or less modified form. When Stromeyer established the principle of subcutaneous tenotomy, he applied a double splint with screw power to crooked joints whose flexors had been previously divided. About the year 1830, Louvrier, in Paris, had employed a machine for the sudden extension of crooked joints, but the results were by no means good. Both these proceedings appeared unsatisfactory to Dieffenbach: the former was too long, and kept the patient in a constant state of pain for months; the latter produced many bad effects. The Prussian surgeon hoped by combining the two to avoid the evils of both, and to a great degree succeeded,* but the force necessary (often the combined strength of three or four men) must have produced agonizing pain, and it is only since the introduction of anæsthetics, whose value in such cases was first pointed out by Langenbeck, that the plan of violent extension as a treatment of ankylosed limbs has reached its present perfection. We cannot here stop to trace the history of this treatment more minutely.

The third edition of Mr. Brodhurst's book, whose name stands at the head of this article, has been some months before the public. We cannot but think that Mr. Brodhurst has been a little meagre in his reference to the experience of other surgeons, his predecessors, and even his contemporaries. His book is a detail of his own experience, well put together and clearly written, without any false attempt at ornament or rhetorical flourish. In size little more than a pamphlet, it may be said to consist of three parts: the first, containing thirty pages, discusses the diseases which may produce ankylosis; the second (forty pages) handles the diagnosis and treatment of true and false ankylosis; the last fifty pages are filled up with cases very well collected and recorded. The value of the book lies in the second of these parts, and we may inform our readers already acquainted with Dieffenbach's and Langenbeck's practice, that the new points on which Mr. Brodhurst insists are, gentleness of manipulation, *perfect* anæsthesia, replacing the limb in the position in which it was ankylosed, and maintaining it in that posture for a few days. We must observe that the title of the book hardly corresponds with its scope, which is really the treatment of ankylosis. We do not think that thirty rather widely-printed pages on the production of that condition contains enough matter to warrant the title, 'Diseases of the Joints involving Anchylosis.'

But it was not merely in cases of deformity from ankylosis that Dieffenbach's name stood pre-eminent. He writes, in 1839,† "I have now operated on 500 club-feet and 60 wry necks." In England, Germany, and France a similar activity prevailed. In the last

* Dieffenbach Ueber Durchschneidung der Sehnen, &c., 1841. We are bound to remember that the proceedings—section of tendon and forcible rupture of ankylosis—are very like those described by Sartorius.

† Casper's Wochenschrift.

country, the so-called "Méthode sous-cutanée" began to make claims that attracted the attention of the Academy, and in the session 1856–57 a very lively discussion, extending over several sittings, took place upon this subject. It arose from a claim put forward by M. J. Guérin to have applied this method to the evacuation of ovarian cysts, a claim founded upon the use of a somewhat complicated canula, or pair of canulæ, provided with a tap and syringe, so as more effectually to exclude the air. MM. Velpeau and Malgaigne denied the evil attributed to the admission of simply a small bubble of air into a wound, while they at the same time denied to M. Guérin and to the orthopædists as a body, the right of arrogating to themselves the discovery that air freely admitted into abscesses, synovial and other closed cavities, produced evil effects. Thus, amid a quantity of extremely clever and witty, but somewhat verbose speaking, the points really under discussion assumed the form of, How far and in what manner air admitted into a wound was injurious? and whether, after all, it was the exclusion of air or other physical condition which rendered Stromeyer's method of dividing tendons and muscles so safe? whether the wounds thus made healed without any inflammation whatever, or by means of the adhesive inflammation of Hunter? and then, as a minor point, whether the details upon which M. Guérin laid great stress, were sufficient to constitute him the originator of a new method? Into the last minor point we will not enter, but we may at once lay down as a fact now well ascertained, that the reunion of subcutaneous wounds takes place by means of an inflammatory act, which, in nearly every instance, stops short of suppuration; whereas an open wound of the same dimensions does in general suppurate and produces the sloughing of fasciæ and tendons involved. The free contact or the exclusion of air seem to be the causes of this difference; at least, such are the most obvious distinctions between the two cases; but other circumstances may produce or aid in producing these different sequence of events. A wound into a closed cavity, as, for instance, a joint, often sets up a violent inflammation, and such effect is often attributed to the contact of air; even a free subcutaneous incision will not produce the same effects. The admission of air into the cavity of an abscess is often followed by the most disastrous consequences. In these last two instances, the effect may be well attributed to the septic character of the air; but in the case of a wound in a tissue—for example, the subcutaneous tissue—other circumstances come into play. The air itself may act as a septic, or as a desiccating agent, or again, a wound with a large external opening is placed in such a condition that the organizable or protective fluids easily flow away, and thus healing may be prevented. We know that suppuration will be caused by anything which frustrates the reparative process, whether that foreign body be solid, fluid, or gaseous; passing a single thread seton under the skin is a subcutaneous operation, yet if the thread be allowed to remain, suppuration will follow; the same effects generally result, but more slowly, if silver were substituted for the silk. Solid bodies cannot be absorbed, but pure water, oil, or gas passed into a tissue are absorbed, and then the wound heals. Emphysema takes place, and the infiltrated cellular tissue neither suppurates nor sloughs. Under such circumstances, what becomes of the dreaded effects of a limited amount of air? M. Bouley brought forward experiments performed on a dog. He produced artificial emphysema of pure air, and on examining it twenty-four and forty-eight hours afterwards, he found that the oxygen had slowly decreased, and was replaced by carbonic acid; the areolar tissue, like a slow-breathing lung, changed the injected air. Under such circumstances, the admission of a bubble of air with the knife has not that extreme importance which some have attempted to give it. Indeed, Mr. Paget found that tendons might be divided by open wound; if the incision were quickly closed, and *long-continued* exposure to the air avoided, suppuration did not follow. Of course, the more perfect operative procedures would endeavour to prevent such occurrence by very simple means.

It is, we believe, with a view to clear up this subject that Mr. Wm. Adams entered upon his course of investigations and experiments, the details of which he has brought forward at various periods and at several societies, and now again in the work before us. The field into which the writer enters is hardly more modern than the Stromeyerian operation; scarcely, indeed, had the first account of that proceeding appeared, than several observers set to work to discover by what process healing took place, and therefore by what means the section of tendons is useful. Günther, Bouvier, Von Ammon, Duval, Pirogoff, Guérin, Velpeau, Tamplin, Paget, &c. &c., have all contributed to the elucidation of this subject. The appendix of Mr. Adams' book gives a very fair and complete *résumé* of the views which these authorities have taken. We can only very briefly pass in review the more important differences of opinion. Delpech and Stromeyer believed in the formation of an intermediate substance (*organization inodulaire*, Delpech), which, before assuming the fibrous character and toughness of tendon, might be stretched to a length corresponding to the amount of shortening in the muscle. Stromeyer added to this an idea that the section exercised a dynamic influence upon the muscle, diminishing its contraction and thus producing its elongation.* Tamplin alters this idea somewhat, and imagines that the new substance which unites the divided ends of the tendon, after a time contracts like any other new formation, and dragging upon the muscle, lengthens it.†

It is certainly a singular evidence of Delpech's perspicacity that every true experiment and result of reasoning shows his view concern-

* John Hunter, in speaking of the effect produced by rupture of the tendo Achillis, mentions an entire loss of contractile power in the muscle. Hunter's Works, by Palmer, vol. ii. p. 281.

† We think this theory is altogether unfounded, for if the force of the contraction were sufficient, we should have just those difficulties with the management of apparatus to prevent the limb rather than the muscle yielding to the force, and the patient would suffer just the same pains as in the instrumental stretching of muscular tissue, and if the contraction took place only after the limb was released, it would certainly much more easily deform the part again than lengthen the muscle.

ing the stretching of the new substance to be correct. The new material is formed by an inflammatory action in the sheath and in the cut ends of the tendons. The idea of Pirogoff, Gerstaecker, and others, that the effused blood is formed into new structure, and the notion of Guérin, that repair takes place in subcutaneous wounds without any inflammation, are both manifestly false. Mr. Paget and other observers, with whom Mr. Adams agrees, consider that the new material is formed out of a fluid effused by the surrounding inflamed tissues. We conceive that these views will have to be very much modified: if the newer doctrines concerning the inflammatory process to be found both in Mr. Simon's article on inflammation,* and in Mr. Barwell's work on the joints (which appeared almost simultaneously), be true, we can only look upon this fluid effusion as a contrivance for provisionally filling out the space which is subsequently to be occupied by budding growths from the inflamed tissues, by what Virchow calls "solid exudation."

Mr. Adams formerly denied the possibility of a linear cicatrix, in which Mr. Tamplin believed, and which Mr. Brodhurst afterwards obtained. Mr. Adams' experiments upon the tendons of animals had been impugned by Mr. Tamplin† and Mr. Brodhurst,‡ and we think with very considerable justice, as that gentleman took no pains to confine the subject limb, and therefore his conclusions as to the length of the cicatrix are invalid, but less so, we think, than the defence which he put forward in a note imputing to Messrs. Tamplin and Brodhurst that they "did not rightly understand the purely scientific objects of inquiry sought to be established by the experiments which I performed."§

There are frequently mentioned throughout Mr. Adams' book certain differences, partly of opinion, partly as to a matter of priority between him and Mr. Brodhurst. It is a very slight matter, but it really appears that a paper by the latter gentleman appeared at a provincial society before Mr. Adams had advocated some similar views, whilst the latter had the start at the Medico-Chirurgical Society. The question is about some little differences in the cicatrices of tendons, and we cannot help imagining that if these gentlemen's minds were busied in a larger field, they would not condescend to divide about such slight matters.

The above consideration leads us to judge the value of the works which have served as the pretext for the present history of deformities and their treatment. We are somewhat struck by the fact, that since Stromeyer's brilliant discovery was realized, very slight advance whatever has been made in that art; perhaps it may as yet be too early that the turn for improvement should fall to that department of surgery. It is but a short time ago that a paper was extensively circulated among the profession, protesting against the multiplication of special hospitals; the plea urged for their redundant existence was, that in dividing surgery into a number of special branches, or rather twigs, the science and practical results of each

* See Holmes' System of Surgery, &c. † Lectures on Deformities: Brit. Journ., 1860.
‡ On Club-foot. § Note to p. 88.

would be greatly increased; but is it so in the present instance? We cannot see that in one of the works, with its fully described experiments and observations, the author reaches further than Mr. Paget's more original researches had already done; we cannot see that in the other a more scientific, a more practical or less dangerous treatment of ankylosis is advocated, than Dieffenbach's or Langenbeck's, or than may be found any operating day in the general hospitals of this metropolis. The last pamphlet on the list is simply a recommendation of a treatment known and estimated twenty-five years ago, and never neglected since. The treatment of club-foot has not progressed since Dieffenbach, nor that of spinal disease since Shaw. If there be not more good effected by the special than can be produced by the general hospitals, harm may be done instead; it may be feared that minds directed always to one subject may too easily perceive that one to the exclusion of others. To human nature not only is there nothing like leather, but at last everything looks like leather, so that even Dr. Little, who himself introduced tenotomy into England, felt bound to endeavour to check too great a partiality for its performance.* A one-sided tone of mind may thus be produced, in which a special instrument would take the place of the proper treatment for acute pleurisy, or a tendon be divided in acute inflammation of a joint. We fail to see the peculiarities in a distorted spine or limb that should render either unfit for treatment in a general hospital, more especially as at the Orthopædic so many persons are treated as outpatients.

REVIEW VIII.

Chemistry in its Relations to Physiology and Medicine. By GEORGE E. DAY, M.A. Cant., M.D., F.R.S., Professor of Medicine in the University of St. Andrews. With Five Plates.—*London,* 1860. 8vo. pp. 526.

To judge the work before us simply by an ideal, though practicable, standard, would probably leave it at a disadvantage in the estimation of our readers. It is therefore but equitable to premise that the author started with defined and limited intentions. For this reason the comparison of his purposes with his achievement will be our principal object in these pages.

Dr. Day has endeavoured to comprehend in a reasonable space all the most important departments of physiological chemistry. This being the explicit profession of his first introduction, we accept, notwithstanding a much more promising title, his performance as claiming literary merit rather than originality of thought and research. With this limitation before us, we were not a little surprised to find that so little attention had been paid by the author to the elaboration of his design and the perfection of its execution, to the amalgamation of the material, and to its reproduction in a homogeneous and fluid form, that

* On Unnecessary Orthopædic Operations. Lancet, 1856.

the preface must needs contain such an acknowledgment as that the last thirty pages of a certain chapter are little more than a *condensed translation from a foreign work.* It is further most extraordinary, that the principal sources acknowledged in the introduction should be themselves compilations of different merit, and that the author should lay no claim to having consulted original articles in journals, essays, pamphlets, monographs, and original treatises. He has chosen a process much more convenient, but less glorious; he has made one author his "principal guide," and made "free use" of all his works in this department of chemistry; he has "borrowed a considerable amount of material" from a set of treatises and a report; from another work he has "extracted much important matter," "made free use" of this monograph, and "mainly founded" some chapters on ancient books in existence. Even the plates are reproductions, *some on a reduced scale, of well-known engravings.* Truly, where so much has to be acknowledged, candour itself ceases to be a merit; but even admitting merit in this description of candour, there remains the sentiment of regret that such a mode of literary husbandry should have been resorted to, when others were not only practicable, but necessary, for the attainment of the objects of the author and the satisfaction of the demands of science.

At first sight the arrangement of the volume strikes us as methodical, as there are three parts, the first treating of the organic substrata of the animal body, the second of the composition of animal fluids and tissues, and the third of the great zoochemical processes. When we came to consider that a comprehensive treatise on physiological chemistry should treat of the inorganic substrata of the animal body as well as of the organic, the title of the first book seemed insufficient; but it lost all significance when we found a chapter on the inorganic constituents of the animal body concluding the very book on the organic substrata. The author has sprung a leak upon the word "organic," which has already caused so much dispute and confusion in chemistry, and is now only retained as a conventional term of classification, without the significance formerly attached to it. All elements entering upon the composition of the animal body or any other organism, are substrata of that organism; the whole of the sixteen elements and their combinations composing the human body are its substrata; potassa no less than hydrogen is required absolutely in the processes carried on in the economy, and potassa and its compounds are substrata of the organic process no less than hydrogen and its compounds. It is not correct, therefore, to term metals and their compounds entering upon the economy of organisms inorganic constituents; the adjective which comprises these constituents as a class is "mineral," and signalizes both their origin and their indestructibility by heat.

This first book, then, treats, as we should express it, of the proximate compounds found in the animal body, entering upon the structure of its tissues or the composition of its fluids. These compounds the author proposes to consider less in their chemical than in their physiological "relations." (p. 1.) This distinction would not appear to us to imply a difference, but for the sentence concluding this information, which refers the reader who may be anxious for all the necessary information relating to the purely chemical details, to the ordinary manuals on chemistry. In the author's language, "chemical relations" evidently signifies "chemical details."

This leads us directly to the consideration of the question which has never yet been satisfactorily answered, whether, in treatises on physiological or any other special branch of chemistry, chemical descriptions of single bodies, of proximate constituents, ought to find any place at all. There can be no doubt that elementary considerations have to be excluded, because they are supposed to be in possession of the reader; the description of the phosphates in the blood cannot begin with the elementary chemistry of phosphorus, because the object of the treatise would thereby be foiled, and instead of a physiological chemistry, a treatise on physiological, including elementary and experimental chemistry would be produced. The chemical *relations* of a substance become apparent only by its position in the general system of chemistry; the physiological value of a substance, on the other hand, becomes evident by the place and part it takes in the physiological process. To curtail the description of chemical relations is a most questionable proceeding, as we can never know which of the relations possess the greatest importance in a given case in pathology or physiology. At best, therefore, an abstract of general chemistry of this kind is useless, because incomplete, and may be dangerous, because what is not seen may be supposed not to exist: it is useless, because it is not sufficiently complete to give practical directions for chemical operations; and being threads drawn at random out of a tissue of a complicated nature, the subjects have no connexion with each other, and cannot be represented in a perspicuous manner.

It would be different, however, with a treatise intended to be a guide for practical study. We will suppose a treatise on the physiological chemistry of the liver, which was to instruct the student at the same time in the mode of analysing the liver and the bile, and in peculiar proceedings for determining quantities devised exclusively for physiological purposes. Such proceedings would be excluded from general treatises on chemistry, and would therefore of necessity be described in the treatise not only specially but exclusively devoted to their consideration; their successful application would imply an intimate knowledge of all the properties of the proximate substrata, and for the sake of convenience these proximate substrata might be described in the same volume, but at full length, with all the chemical detail of the most elaborate manual, and with all the accuracy required for special reference in scientific inquiries. Of such a treatise we could see the use and object; it would have a particular advantage over manuals, because it could give the newest information, which is sometimes of the greatest importance. Substances newly discovered, new properties of substances previously known, processes for their extrication under complicated circumstances, characteristic reactions and modes of quantitative analyses, would properly be contained in such a treatise.

The heading of the first chapter of Dr. Day's treatise contains an adjective to which there are many objections. It is already a doubtful proceeding to range under one head a large number of substances which have a single positive character in common, such as the presence of oxygen or of sulphur. But to make the absence of an element a common characteristic of an enormous class of most different substances, to unite acids under one standard because they contain no nitrogen, and term them non-nitrogenous, appears to us a questionable mode of classification. The class of organic acids, as distinguished from alcohols, ethers, or bases, is well defined; they have been found to arrange themselves naturally into certain sets, according to certain well-marked homologies of composition. These sets are termed homologous series or groups. Among these the fatty acid series is best known. It has nothing in common with the benzoic acid series, but is closely related to the lactic acid group, not because both contain no nitrogen, but because one may be transformed into the other, or into a group of acids containing nitrogen. The title of non-nitrogenous, therefore, includes under one denomination bodies differing to the greatest extent, it fails to describe relations which do exist between others, and it excludes from their proper places in the systematic arrangement nitrogenized substances which are closely related to others containing no nitrogen.

We can illustrate this complicated statement by the simplest example. Acetic acid belongs to the fatty series. By the introduction of amide into its composition (substitution of H by NH_2) it is transformed into amido-acetic or acetamic acid, a body identical with glycocoll, the lowest member of that amido-acid series which runs parallel to the fatty series. Acetamic acid, by the substitution of peroxide of hydrogen for amide (HO_2 for NH_2) becomes oxy-acetic or glycolic acid, which is the lowest member of the lactic acid series, or series of the oxy-acids of the fatty series. These metamorphoses can be effected forward and backward, starting with any of the three different acids, and clearly prove that they contain the same nucleus or radical, unchanged by the substitutions on its surface.

Acetic acid, glycocoll, and glycolic acid must, therefore, be placed together in any arrangement, whether chemical or physiological, in the sense of Dr. Day's Book I. Let us see what are in reality the places occupied by these bodies in Dr. Day's attempted classification. Acetic acid stands under the first group (p. 11); glycolic, or, as the author terms it, glycic acid, under the fifth (p. 19); and glycocoll appears under the first group of the second chapter, which treats of nitrogenous basic bodies. The relations of these bodies to each other are not even mentioned in any of the three respective places, and their physiological relations remain, therefore, at the best, out of the question.

It is similar with the analogous relations of lactic, metacetonic or propionic acid, and alanine. How different our idea of lactic acid becomes by the knowledge that it is oxy-propionic acid, and that it may be produced by removing the amide from alanine, the artificial amido-acid of propionic acid. But of these relations the relative chapters in Dr. Day's work make no mention.

The knowledge of such relations will not enable the physician at once to apply them to the cure of disease; he does not thereby acquire immediate power over the acute rheumatic process, even should it in reality turn out to be the product of an abnormal development of lactic acid in the blood, or other parts; but it will enable the observer to ascertain the mode in which lactic acid may be produced in the muscle of flesh-feeders, or to inquire whether it may not sometimes originate in the decomposition of sarcosine, the amido-acid from creatine, and isomeric, perhaps identical with alanine. Every new chemical relation of a body opens a fresh avenue for inquiry of this kind. These avenues a treatise on physiological chemistry must not only endeavour to keep open, but to extend; and a more ambitious plan includes the opening of new approaches. It is one of the greatest deficiencies of this work that it contains not the slightest progress in this direction.

Passing to the series of fatty acids (p. 6), we find these subdivided into two classes, the incongruity of which is very striking: the man of the laboratory would never divide these acids into volatile on the one, and solid acids on the other hand. The counterpart of volatile is non-volatile: the author found this term, which is relative, inappropriate to apply to the solid acids, because they are all volatile under certain conditions; this should have taught him that relative volatility is no principle of chemical classification; having adopted "solid" as the adjective of the second division of acids, the first division naturally claimed the common positive property of fluidity as the principal characteristic. The division of fatty acids into fluid and solid ones is sensible and practical; it is the result of a glance at a series of specimens put before a person entirely unacquainted with the order obtained by the knowledge of their composition; but the division into volatile and solid ones, which we do not remember to have seen before, is under all circumstances untrue, solidity not excluding volatility, illogical because ineffective, and unpractical because useless for the commonest purposes of distinction.

The rational composition, as it is termed, of organic bodies is a most intricate subject of inquiry; and as the fatty acids always were the most prominent subjects of chemical study, so with regard to rational composition have many experiments been made, and many hypothetical considerations been put to the test upon them. At one time they were considered as copulated compounds of an alcoholic radical and oxalic acid; because on decomposition all (excepting formic acid, which rebelled against this explanation) yielded oxalic acid, and because on deducting from the formula of acetic acid the elements of methyl, or from propionic acid those of ethyl, and so on, there were left the elements of oxalic acid ($C_4H_4O_4 - C_2H_3 = C_2HO_4 = C_2O_3 + HO$). But this explanation, for which there was much support in reactions of a very striking nature, had to be abandoned when it was found, on the one hand, that one equivalent of oxygen in the assumed oxalic acid possessed a different significance than the other two; and on the other, that oxalic acid was a bibasic acid, that its formula had to be doubled ($C_4H_2O_8$ instead of C_2HO_4), and that it was the first and typical member of a separate series of acids, which hitherto had been ranged under succinic acid, and termed the succinic acid series, and was now properly termed the oxalic acid series. In good manuals, published in 1856, we find oxalic acid heading the group of bibasic acids, of which succinic acid was formerly the representative. Everybody, we believed, had agreed to this change, and was now adhering either to the theory of types, according to which a fatty acid is water (a double atom HHO_2) in which one hydrogen is replaced by a radical, or to the carbonyl theory, according to which a fatty acid is a combination of the hydrate of the trioxyde of carbonyl with one equivalent of hydrogen (formic acid), or some organic radical. We should therefore have expected that the author, if he intended to meddle with rational composition at all, would have expounded in an easy and somewhat popular manner either of those theories, or both; we should have expected him to turn such an opportunity to good account, for the sake of our chemical and our medical brethren, who stand in need of a mutual interpreter; our lowest expectations would have extended to a good extract from the latest publication allowed to be good by common consent. Our expectations, even the most moderate ones, were sadly disappointed when we read (p. 7) the following sentence: "There is good reason for believing that the volatile fatty acids are conjugated oxalic acids."

The good reasons were all against this assertion six years ago, and there was and is now not one reason, good or bad, left to support it. When the author wrote that, "on the assumption that oxalic acid constituted the acidifying principle (*sic*) of the acids of this group, he should consider it first in the series of acids," oxalic acid was already the type and first member of its own independent series.

The author, in proposing to consider oxalic acid first in "the" series of acids, must have meant first in *this* series, the *fatty* series of acids. It cannot be otherwise, for oxalic acid is not to be found in the author's second group, the succinic series, where it ought to have been, but is placed, in name at least, at the head of the fatty series, where it ought not to have been. Once named, the relations of oxalic acid to the fatty acids disappear; it combines with lime, and all the author has to say on this subject is a meagre, and in three distinct statements incorrect, account of oxalate of lime.

In the order usually adopted by chemists, the compounds of acids with basic bodies are placed in the chapters treating of the acids. Any one who may be inclined to try the reverse plan and put them under the chapters governed by the bases, will speedily convince himself of the practical necessity which has dictated this arrangement. The paragraphs devoted to the consideration of the solid fatty acids are consequently the proper places for describing their compounds with glycerine. But Dr. Day has not adhered to this usage, and on scarcely two pages has given certain information concerning these acids, which in no sense repays perusal. It is true that he has expressly reserved for a later occasion his observations on the origin and importance of the two acids which, by their massive occurrence, claim earliest attention, and we shall presently refer to his treatment of the chapter upon which we are thus directed. Before, however, doing so, we will consider the account of the acids themselves.

Dr. Day is well aware that Heintz has denied the existence of an acid of the properties and formula ordinarily attributed to margaric acid. Without any other reason than that this view was, as he alleges, not generally adopted, he casts it aside, and reiterates the old statements relating to margaric acid.

Palmitic acid is only just mentioned, and the two lines devoted to it include the remark that "no one else has found it." On p. 74, Heintz comes in for another share of criticism, which, in addition to its impoliteness, is entirely undeserved. The author remarks in a foot-note, that Heintz had "for many years been actively engaged in the examination of the fats, and had thrown their chemistry in a sad state of confusion." He then quotes the conclusions from a former memoir of Heintz, which had been abandoned by that chemist himself. Such a quotation may serve the purposes of a controversialist, but in a comprehensive treatise which is to contain the newest and best doctrines, it is at least superfluous. The author ultimately in a few lines relates the latest results of Heintz. This chemist has given more attention to the study of the fats and fatty acids than any other living analyst; his first series of researches was faulty, owing to imperfect analytical methods; the second series, in which he employed a new method, which may be described as the method of partial precipitation, and which is a good counterpart of Liebig's famous method of partial saturation for the separation of volatile fatty acids, put the chemistry of the fats upon a firmer basis than it had been before. The last results promulgated by Heintz having been obtained through the instrumentality of a new analytical method, it was doubly satisfactory to find them confirmed by the synthetical researches of Berthelot. But to these the author has not even adverted, an oversight which we will endeavour to supply, in order to elucidate a shade thrown upon the subject by the author's mode of compiling.

Glycerine, or the hydrate of the oxyde of glyceryl, is a triatomic alcohol; the radical glyceryl is triatomic, because it substitutes three equivalents of hydrogen when introduced into other compounds; the alcohol yields three equivalents of hydrogen, when three equivalents of a monatomic body, or one equivalent of a triatomic body, are introduced into it. It is not necessary to substitute all three equivalents of hydrogen at the same time; one, two, or three equivalents may be substituted, and thus are formed the mono-, di-, and tri-glycerides, which are not salts, but compound ethers of glyceryl.

According to the theory of radicals, the acid which forms a fat with glyceryl may be supposed to combine with the oxyde of the radical of the alcohol whose water remains uncombined; but again, such a compound is not a salt, but a compound ether. Glycerine contains three equivalents of water (termed basic), each of which may be replaced by one atom of a monatomic acid.

When glycerine combines with any fatty acid—be it acetic, valerianic, palmitic, stearic, or with oleic acid—it may do so in three different proportions. The substitution of all three equivalents of water by three equivalents of any of these acids causes the elimination of six equivalents of water, and the production of the neutral glycerides or triglycerides. To this class of neutral glycerides belong the fats, and every system of chemistry or physiological chemistry would have to introduce the fats to our notice under the following names and formulas:

Tributyrine (syn. butyric acid—triglyceride, tributyric acid—glyceryl—ether), formula, according to the theory of types:

$$\left.\begin{matrix}(C_8H_7O_2)_3\\(((C_6H_5)))\end{matrix}\right\}O_6.$$

The single brackets enclosing the formula of butyryl signify that it is a monatomic radical; the three pairs of brackets enclosing the formula of glyceryl indicate its triatomic character. Tributyrine represents six atoms of water, in which three atoms of hydrogen are replaced by three atoms of butyryl, and three others by one atom of glyceryl.

Tripalmitine (syn. triglyceride of palmitic acid):

$$\left.\begin{matrix}(C_{32}H_{31}O_2)_3\\(((C_6H_5)))\end{matrix}\right\}O_6.$$

Tristearine (syn. triglyceride of stearic acid):

$$\left.\begin{matrix}(C_{36}H_{35}O_2)_3\\(((C_6H_5)))\end{matrix}\right\}O_6.$$

In a similar manner trioleine is constituted and represented by putting the formula of oleyl in the place occupied by the fatty acid radical.

Berthelot, by enclosing various proportions of glycerine and fatty acid in hermetically sealed tubes, and heating them for some time, succeeded in producing all natural fats artificially; but he also produced the mono- and diglycerides, thus proving in another way the triatomic character of glyceryl, which had been found in the course of the artificial preparation of glycerine from iodide of allyl, where in one reaction the monatomic allyl is transformed into the triatomic glyceryl.

Such is the doctrine established and taught during the last five years, but Dr. Day passes it altogether without notice. He places the description of the fats under a chapter entitled, "Haloïd Bases and Salts." We know some substances termed haloïd salts, but we have never heard of "haloïd bases." Chlorides, iodides, and bromides, and other analogous compounds, could be termed haloïd, salt-like bodies, at the time when the presence of oxygen in a body was an essential condition of its being allowed to constitute a true salt. But to arrange such a variety of bodies as the oxydes of doeglyl, cetyl, cerotyl, melyssyl, lipyl, and their hydrates, most of which are alcohols, under the heading of haloïd bases, and their ethers under that of haloïd salts, and to conclude that chapter with an appendix on "Lipoïds," of which the alcohol cholesterine, so ably handled by Berthelot, is the typical representative, is an antiquated proceeding.

Such a system might yet pass the severest criticism, if in its execution care has been taken to ensure correctness and relevancy of the details. But in the description of the fatty acids we have searched in vain for the record even of the readiest reaction by which they can be distinguished from oleic acid and neutral fats. This reaction is produced by boiling the fatty acid (palmitic or stearic) with a solution of alkaline phosphate; the acid is dissolved in such a manner as to form an imperfect and turbid solution, termed an emulsion. As a direct consequence of this oversight or omission, p. 76 contains the following passage: "In the blood and lymph the fat is for the most part saponified and dissolved, and not in a free suspended state. It is only in special conditions that we find unsaponified fat in suspension in the blood," &c. &c. Not a word is said about the remarkable observations published some time ago, that fat in chylous blood (and all blood is chylous for a time after digestion), and fat in chylous urine, is present in the form of fatty acids, suspended in the very form of that emulsion which is produced by alkaline phosphates.

We will leave the succinic, oleic, benzoic, and lactic acid groups to struggle with their own deficiencies, and pass to (p. 24) the non-nitrogenous resinous acids. We there find lithofellic and cholic acid (the cholic acid of Démarçay and cholalic acid of Strecker) placed together. In a foot-note the author thinks he ought, perhaps, to have added benzoglycolic acid, although it had never yet been found preformed in the animal organism. The author might have added another objection—namely, that it was not resinous. But, then, lithofellic acid is not resinous either so far as we know. And that cholic acid in one modification is resinous, is so far outweighed by its property of crystallizing readily from alcohol and ether, and, though not so readily, from water, in a splendid variety of forms, that we should hardly dare to term it a resinous acid. The term resinous is generally applied to substances which do not crystallize under any circumstances. Thus choloïdic acid might be termed a resinous acid. If an acid, in order to be placed under any of Dr. Day's chapters, requires to have been found in the animal organism, we doubt the claim of cholic acid, for we are not aware of the occasion upon which this acid had been indubitably discovered in the normal fæces of man when the author wrote. But even allowing this claim, cholic acid possesses a far better one of being described in a treatise on Medical Chemistry, in that it is the basis of the biliary acids and a product of their decomposition. It should, therefore, have been described under "biliary substances."

Though biliary substances form a class by themselves, the author has thrown them together with uric acid, and some other luckless fellows, under the superscription of "Nitrogenous conjugated acids." The first four of these acids, says the author—namely, hippuric acid, and glyco-, hyo-, and tauro-cholic acids—are placed by themselves in the first group, because they especially present the leading characteristic of conjugated acids; that is to say, when treated with concentrated acids or with alkalies, they resolve themselves into a nitrogenous body, which we regard as the adjunct, and into a non-nitrogenous acid.

Uric acid not presenting the leading characteristics of conjugated acids should consequently not have been described in this chapter. But the author avoided this difficulty by inventing a special subdivision for it, which is distinguished only by the absence of all definition. Chemists do not consider uric acid as a conjugated or copulated acid in the ordinary sense. The fact that it contains three different radicals does not constitute it a conjugated acid. The author's classification of uric acid is consequently proved to be faulty both by his own definition of the class under which he ranged it, and by the usage of other authors. The author himself recollected (p. 56) that uric acid was closely allied to the two neutral bodies, hypoxanthine and xanthine, and indicated the place where he ought to have put it.

Hippuric acid should have been placed with the benzoic acid series, under the heading of copulated or conjugated compounds of benzoic acid. To this division also would have belonged any description of benzoglycolic acid, regarding which the author was in doubts whether it should not go with the resinous acids. Hippuric acid is the amido-acid, not so much of benzoglycolic acid as of another as yet unknown acid of which benzoglycolic is the oxy-acid. The following parallel perspicuously exhibits these relations:—

Acid.	*Amido-acid.*	*Oxy-acid.*
Propionic (or Metacetonic).	Propiamic. (Alanine.)	Oxy-propionic. (Lactic.)
Unknown. (Benzo-acetic?)	Hippuric.	Benzo-glycolic.

The second division contains an account of the tartar which M. Verdeil caught—namely "Pneumic" acid (p. 62). This paragraph caused us the greatest surprise, particularly as the author when he went to press was aware that pneumic acid had drawn its last breath. For, he says in a foot-note, with the greatest *naïveté*, that "since the above paragraph was written, Cloetta seemed to have satisfactorily proved that pneumic acid is merely taurine." If Cloetta's proof satisfied the author, why did he mention pneumic acid?

Having analysed at some length four different chapters, we feel now called upon to notice a peculiar feature with which most of the chapters conclude—namely, the "tabular views of the composition" of the substances contained in the relative chapters. Such a tabular view is nothing else than a collection of the names, formulas, and figures expressing the per-centic elementary composition of substances thrown promiscuously together. The first "tabular view" (p. 27) consumes two and a half pages, the second one (p. 53) little more than a page, the third (p. 70) as much again, the fourth chapter lacks this tail, but on p. 93, in the table belonging to Chapter V., this deficiency is made up, and the "tabular view of the composition of the neutral non-nitrogenous bodies" exhibits glycerine, cholesterine, milk-sugar, and glycose in sweet embrace. On p. 99, we find the animal pigments tabulated (one page and a half), and admire the belief which tabulates the formula and composition of "hæmatin freed from iron." Page 117 exhibits the percentic composition of the "protein bodies" (one page and three quarters), and p. 123 the proximate derivatives, as they are termed (should perhaps be derivates), of the protein bodies (one page and a quarter). Almost ten pages are thus occupied with words and figures which we are certain will not be of use to any one single reader. If the treatise had been of so complete a nature as to include advice for analysis, the percentic composition of every substance would have found a proper place under the chapter devoted to that substance. But to withdraw these details from their proper relations, and to put them together in a form in which they necessarily exhibit no relation whatever to each other, is a waste of type and paper.

The second chapter offers some features of so extraordinary a nature, that we are obliged to go back to it for a moment. It is devoted to "Nitrogenous basic bodies." The first sentence contains a clerical error, whereby the author contrives to say the reverse of what he intended: "The organic bases are divisible into two well-marked groups, according as they contain or are devoid of oxygen; the former being, without exception, volatile; and the latter non-volatile or fixed." (p. 30.)

That glycine, sarkosine, and leucine should find themselves classed among the "non-volatile alkaloids" will no doubt cause considerable astonishment to these amido acids, particularly when they consider that their true relations to the lactic-acid group might have been known, as is apparent from p. 30.

The question about the crystalline condition of leucine has been settled for some years; but the author repeats the doubts, without giving their explanation. The statement of Funke and Lehmann that leucine crystallizes in rhombic tablets and prisms, or in needles in starlike groups, is correct for sublimated leucine. From solutions leucine is obtained in granules, which exhibit a deceptive radiary appearance; but even high powers of the microscope do not reveal this appearance to be produced by needles. The observations of Virchow, who found only acicular crystals, cannot refer to leucine, and, in our opinion, must be explained by his having mistaken tyrosine for it.

If a competent person will read the description given by Gorup-Besanez of "the body homologous to leucine" found in the pancreas of the ox, he must see at a glance, even without knowing the results of

Scherer's analysis of the same organ, that this "homologous" body is nothing but leucine itself. A little criticism, therefore, would have caused, a little knowledge enabled any author to eliminate, this error. We must leave the author to account for the reason why it was not expunged from p. 33.

In the description of tyrosine there occurs much looseness and omission: "It dissolves readily in alkalies and acids, from which it again crystallizes unchanged on evaporation." This sentence should have run thus: "It dissolves readily in alkalies, including ammonia, and in mineral acids. From the solution in ammonia it crystallizes on evaporation; from the solution in fixed alkalies it can be obtained by neutralization with acetic acid; from the mineral acids by the addition of acetate of soda; in the nitric-acid solution it is destroyed by evaporation." The author has never seen or handled tyrosine; it is therefore but natural for him to deplore the absence of delineations of its crystalline form, and to omit stating its most important and most practical reaction, namely, the production of a crimson colour and precipitate by boiling with nitrate of red oxyde of mercury containing a minimum of free acid.

It would puzzle the author, no doubt, were he called upon to obtain sarkosine by boiling, as he advises, and others have advised before him, creatine with baryta water. (p. 35.) A solution of creatine saturated while boiling, requires ten times its weight of solid hydrate of baryta, added at once, and subsequent smaller additions, to be decomposed in the manner referred to.

On looking at the part devoted to urea (p. 37), it struck us as possible, from its peculiar proportions, that the author might have written it for a book on a different plan than the volume before us, as it contains the following items: chemical decompositions; combinations with oxyde of mercury; with chloride of sodium, nitric acid, oxalic acid; tests; occurrence (p. 41); daily amount; influence of sex (p. 43), of food, of exercise (p. 44), of medicines, of diseases (p. 45); its presence in blood (p. 46); its origin (p. 47). The chapter, as it stands, would have been useful in a manual of one-third the dimensions of the book before us. It could not have been written by a man who contemplated to devote nearly ninety pages to the description of the physiological chemistry of the urine in another part of his treatise. It clearly reveals to us the want of unity of design in this volume, which, once apprehended, becomes evident on many other occasions. The second and third books are executed upon the plan of the eighth volume of Gmelin's 'Chemistry;' but with that plan the scheme of the first book does not tally; and no explanation of this discrepancy suggests itself to us, but that the author had conceived his "Physiological Chemistry," and partly carried it out, when the appearance of the volume of Gmelin's 'Chemistry' put him upon a new track, which he now followed, without adapting his first part to his subsequent designs.

A substance which has only been seen in the microscope and never been analysed, of which it is not known whether it has basic properties and contains nitrogen, or is acid and contains none, cannot with any propriety be ranged with "nitrogenous basic bodies." It was therefore with the utmost surprise that (on p. 51) we perceived myeline placed between guanine and cystine, notwithstanding the author's own confession that the chemical nature of this substance seemed extremely doubtful. If the author had used precise thought and language regarding the chemical nature of myeline, it would have appeared to him not only extremely doubtful, but unknown, without an alternative. He placed it where it must mislead all those who possess no further information on this matter, and did not place it where, from what we know of the substance, it might perhaps have been placed. It would have been of more advantage to the chapter on the "nitrogenous basic bodies" if, instead of introducing this paragraph on myeline the author had amended the chapter on hypoxanthine, which was apparently written in 1856, and at the time the author went to press had become obsolete by the discovery of sarkine and xanthine as normal ingredients of many animal parts.

We will pass over the chapter on the carbo-hydrates (which word is made to signify starch and various descriptions of sugar) and all the antiquated assertions about the effects of the pancreatic juice and the formation of sugar, and will notice the chapter on animal pigments. It comprises five and a half pages, exclusive of the "tabular view" already referred to. Of bile pigment the author avers that he cannot give even an empirical formula to represent its composition (p. 96), thus showing that he is not aware on that page of the researches of Heintz on this subject. But in the "tabular view" the empirical formula and per-centic composition of biliphaeine are both given according to Heintz. Nothing is said of the composition of the green colouring matter of bile, which was also ascertained by Heintz, and stated in the same paper in which he described the composition of the brown modification. A chapter exhibiting such inconsistencies and omissions is obviously very deficient. For the rest, it contains so few redeeming features that we cannot entertain its apology; no future work on animal chemistry will, we trust, exhibit a chapter under the superscription of animal pigments. Bile-pigment, the chemical nature of which has already been discovered, will have to be placed with the biliary acids, and the other pigments to their respective places among their relatives, which are partly known, and have partly yet to be discovered.

We had patiently wound our way through the brushwood of one hundred pages without meeting a tree to give us shade or tender its savoury fruit on heavy branches, without meeting a bush of fragrant flowers to refresh our senses, when, on the aspect of a new field beginning on p. 101 (chapter vii.), we were utterly amazed to find that the author really adhered to proteine, whatever in truth might be the nature of that "great doubt" which he admitted had been thrown on the theory of this substance. He terms the albuminous substances "proteine bodies," and alleges "convenience" as his principal motive for this act.

Proteine never had any existence. It never was isolated, either by Mulder himself or any one of his followers. It was proved by others, and admitted by him, that the substance erroneously declared to be proteine, the radical free from sulphur, always contained sulphur, and that in variable proportions. All subsequent sophistry remained unaccompanied by proofs; and the theory of proteine which, twenty years ago, moved the entire world of the learned, has ascended to those aërial regions where dwell the spirits of departed scientific hypotheses. Some time ago we read an amusing article in 'Macmillan's Magazine' on Scientific Hoaxes. We then thought that two of the greatest scientific hoaxes had been omitted, and intended to write a letter on the subject to the editor of the magazine. One omission was the account of the Würzburg professor, who after having, during an entire year, dug up the rarest fossils of the most curious and unheard-of animals, and after having had accurate engravings taken of all of them, one day discovered his own petrified likeness in the clay strata of the same stone-pit, where the overflowing mirth of the students had formerly buried the various productions of their lively imagination. And the second omission we thought was proteine, which claimed its name now properly as taking the first place among the hoaxes, because it was practised upon the *savans* of the whole world. We regret not to have carried out our intention, as it might have prevented the shadow of proteine from reappearing on the other side of the Tweed. As it has done so in this memorable instance, we must bethink ourselves of some serious remedy to put into the hands of all those who may think themselves threatened by a visitation. That remedy consists in reading aloud during the hour of danger, or even before, the exorcisms of Liebig, Laskowsky, and Fleitman, in the 57th, 58th, and 59th volume of the 'Annalen der Chemie.'

The repetition of a fallacy is, under all circumstances, a serious and sad mistake, and is the more to be regretted if it is due, not to ignorance, but to a faulty mental operation. "Albuminous bodies" is a very good expression, and signalizes substances similar to or derived from white of egg. But the pretentious name of Greek origin introduced by the Dutch chemist is not only inconvenient, but false without a qualification. For this reason we, in common with most modern authors on chemistry, have carefully avoided using any of the denominations introduced by Mulder for those substances of his which we now know have no existence. And to this practice we shall not only adhere, but endeavour to convert others by the inducement of our example and the weight of our argument.

It caused us no surprise to find that where this exploded fallacy had been reiterated, all mediation towards the insight into the rational composition of albuminous bodies should be wanting. The researches on this the very key to all physiological chemistry are already numerous, and afford an excellent opportunity for an interesting chapter. But the author had as little comprehension of the importance of the subject as he had of the nature of those researches. He could not tread the higher walks of chemistry, because he stumbled in its very elementary footpaths, and came to extraordinary falls. The following passage on p. 105 is one of the lowest:

"When digested for some time with solutions of the caustic fixed alkalies, or when fused with them, there is a development of ammonia, and of both formic and carbonic acids, while various neutral or basic nitrogenous substances are formed—namely, leucine, tyrosine, glycine, &c."

This sentence, lacking noun and pronoun, defies elementary notions about acids and alkalies; for never before was caustic fixed alkali known to develop either formic or carbonic acid.

We pronounce it with regret, that the first book of Dr. Day's work is so completely defective, that he would have done well to leave it aside altogether. It contains nothing that might redeem it, and if allowed to exist by the author, will infallibly damage him in a much wider circle and for a longer time than he can make up for by the credit which the two remaining books may do him for assiduity and learning.

We approach the second book of Dr. Day's treatise with a feeling of great relief from the task, neither agreeable nor easy, which the first book imposed upon us. The chemistry of the animal juices and tissues contained in this book occupies the greater part of the work (from p. 141 to p. 414, 373 pages out of 517). In accordance with the acknowledgment in the introduction, we find that the author has made Lehmann his principal guide, and has adopted the arrangement upon which his 'Zoochemie,' being the eighth volume of Gmelin's 'Chemistry,' is executed, with this immaterial alteration, that the account of the exudations is placed before that of the solid tissues. This book fulfils the author's promise by giving all the most important data concerning physiological chemistry in a concise and perspicuous form. Sometimes the style is a little too aphoristic, and is governed by the division of the text into paragraphs, following each other rather abruptly; but this system, though not inviting to cursory reading, is useful in those frequent cases where a reference to single points becomes desirable. It is possible that the author, in adopting this arrangement, was influenced to some extent by former experience in reporting the progress of certain branches of animal chemistry. To this circumstance no doubt some parts of the book owe the introduction of conflicting statements; and these parts certainly have more the appearance of a series of short reports strung together, than that of a homogeneous cast of amalgamated materials. However that may be, they are good and useful, and on the whole accurate. The whole of this second book will be read and consulted with advantage, and on it hinges, in our opinion, the value of the entire publication.

The author's remark on the condition of the saliva in rabies, and his suggestion to study the poison of venomous serpents for the purpose of unravelling the mystery of hydrophobia, appear to be little calculated to lead the inquirer in a proper direction. The desire to analyse subtle animal poisons chemically will not easily be gratified, on account of the difficulties placed in the way of obtaining the necessary amount of material. There is, moreover, no analogy between any of the symptoms produced by venomous serpents and those produced by the bite of mad dogs. It is therefore highly improbable that the most accurate knowledge of the serpent's venom would teach us anything regarding the poison of rabies, and this improbability is strengthened by the consideration that the former is a physiological secretion of a special organ created for the purpose of killing other animals for food, while the latter is a product of a disease which originates most commonly in the canine species, and in the production of which an abnormally high temperature appears generally, though perhaps not necessarily, to have at least some share.

The mode of obtaining gastric juice from man by the introduction into the stomach of dry pease and a little water (p. 158) is, we think, less suitable for researches connected with the transformations of starch than the method which has latterly been extensively employed for obtaining the juice from dogs, according to which the cleaned cartilages from the windpipe of cattle are introduced into the stomach. Half-a-pint of gastric juice may thus be obtained even from a small dog in a short time, and its purity is guaranteed by the appearance of the cartilages, which are almost unchanged even after some hours, provided the gastric juice had been constantly kept flowing out of the stomach. Filtered through calico and then through paper, the juice is clear and fluid, of a somewhat yellowish colour, and of the peculiar flavour belonging to all parts of the dog. We lately tasted an extraordinary nice specimen, and found it somewhat saline and acid, and, with the exception of the flavour, by no means repugnant. The wonderful powers of this liquor appear to us by no means unravelled by the various experiments made with acid and pepsine in digesting machines. Its daily quantity in a man of ten stone has been estimated by the author, upon the basis of the observations of Bidder and Schmidt, to amount to thirty-seven pounds. A pound and a half of juice is therefore, on an average, produced during every hour of a man's life, and if the estimate is not exaggerated, it shows the necessity of giving rest to the stomach at intervals to preserve so enormous a function from injury and breakdown.

The question why the stomach is not itself digested, but resists the influence of its own secretions, has engaged the attention of the author (p. 165), and he believes the immunity of the stomach to be "due to the continuous re-formation of epithelium during the process of digestion." This explanation is of course unproved, and appears to be unavailing even as an hypothesis. The single cells of epithelium, as they leave the glands of the stomach, could not prevent the gastric juice from touching the surface of the stomach. But little epithelium is in effect secreted. The true explanation of the resistance of the stomach is partly chemical, partly mechanical. The flow of the juice from all parts of the surface of the stomach is centripetal and uninterrupted, so that the surface is constantly coated by the last secretion. It is irrational to assume that the juice, in the moment when it leaves the peptic glands, should have any chemical destructive affinity for the very tissues which secreted it. An affinity may begin to show itself after the juice has undergone changes. It does begin to show itself as soon as the secretion ceases. If after the cessation of the secretion, gastric juice and undigested food remain in the stomach, its surface is corroded, producing much pain, and if it lasts longer, extravasation of blood. This condition constitutes a common form of painful indigestion. Although we have no doubt that the affinity between the coats of the stomach and its juice was regulated by design, we are nevertheless ready to admit, and hereby give a distinct chemico-physical explanation of this phenomenon. Alkaline blood pervades the entire crimson surface of the stomach during digestion; its rapid flow is indicated by the large quantities of gastric juice obtained; the act of secretion separates *acid* from the blood, the alkali remains in or returns to the blood; the blood, after the act of secretion, is therefore more alkaline than before it. Now, even if the coats of the stomach had more affinity for the gastric juice than is reasonable to assume that they should possess, the small portions of juice which might approach the tissues or soak them, would be constantly neutralized by the alkaline blood pervading every fibre of this tissue. The surface of the stomach would therefore not be corroded while it continues in an active state; but when want of nervous influence interrupts the secretion before the food is digested, when bloodvessels become obstructed by embolic disease, and entire regions of the surface lose their secreting power, when parts of the surface have been replaced by the tissue of cicatrices, the juice acts upon the whole or part of the surface with more or less energy. Thus ulceration is produced and maintained on the inner surface of the stomach, whereby a variety of severe symptoms are produced; when the digestive corrosion proceeds to attack the muscular coats, the symptoms increase in severity, and when it perforates the stomach, it mostly causes the death of the sufferer.

We might fairly question the author why the preponderance of potash salt in the bile of fishes is contrary to his expectations (p. 167). We have read the statement before, and have seen it alleged, that as fishes lived in a medium strongly impregnated with soda salts, their potassed bile was an anomaly. It is probable that the diet of those fishes whose bile has as yet been examined, has more to do with the nature of their bile than the medium in which they breathe. Anyhow, nature was quite competent to separate by membranes potassa from soda-salts, and if we do not recognise the reason of that separation in fishes, we should not think the occurrence paradoxical. The blood of mammals is alkaline and rich in soda-salts: their muscles, pervaded in all directions by this blood, contain a neutral or acid juice, in which potassa-salts prevail. This distribution is striking and remarkable, but it is not unexpected, because we had no right to expect that the contrary or any other condition should obtain. Not to form preconceived notions is the lesson which we desire to abstract from this contemplation.

The section which treats of the bile is perhaps the least distinct in its teachings, and exhibits the fluctuations of the doctrine and the uncertainty of the author more than other sections. The man has yet to arise who is to explain the function of the liver and bile. He must be a devoted chemist and a good physiological experimentalist. He must endeavour to comprehend the idea of Liebig regarding bile, which that great man put forward years ago, and which it has lately been the fashion to despise and ignore. The rational composition of the albuminous substances and that of the biliary substrata are so intimately connected, that we are certain the knowledge of the one will be advanced by progress in the chemistry of the other.

From the recent extravagances as to the function of the liver, science is now happily returning to a sober appreciation of the true secretion of the liver—namely, bile. The chemistry of bile itself has not been proportionately advanced by any of the physiologists who have given so much time and labour to the investigation of the function of the liver, but much advance has been made towards determining the normal average quantity of bile discharged during physiological periods by man and animals. Nevertheless, the author himself assists in showing the great discrepancies to which loose calculations upon the basis of figures found by observation on animals must lead. He quotes the observations of Bidder and Schmidt, Nasse, Arnold, Kölliker, and Müller, and states their conclusion to be that 1000 grammes of dog on an average discharge daily 36·1 grammes of bile, containing 1·162 grammes of solid residue, into the intestinal canal. From these figures he calculates that a man weighing ten stone would daily secrete 2310 grammes, or about five pounds of bile.

Ludwig,* on the other hand, by a similar calculation based upon the physiology of the dog, obtains only a quantity of daily bile in man varying between 80 and 600 grammes. This quantity of liquid is calculated as cystic bile, which, containing between 8 and 16 per cent. of solids, is as concentrated again as the bile issuing from the biliary duct. He allows, therefore, that the quantity of fluid might be doubled, the solids of course remaining unchanged. The largest quantity of bile, therefore, which, according to Ludwig, passes from the human bile-duct in twenty-four hours, amounts to 1200 grammes, or a trifle more than half the quantity assumed by Dr. Day. Here is work for the mathematicians. Let them show how it is that uniform and good observations should lead to such discordant conclusions, and let them find the excuse for the discord which we are unable to imagine.

On gall-stones the author has a slight paragraph, following another

* Physiologie des Menchen, Band ii. p. 328.

one equally slight on the pathological changes of bile. Bolle's analysis of ox gall-stones (1834) might have taught the author that Branson's "pigment-lime," if in existence at all, can only constitute a small proportion of the bulk of a calculus consisting principally of pigment. Late researches published in this country have thrown light upon the composition and pathology of gall-stones. They have demonstrated that these concretions originate in a decomposition of the bile, which must be very similar to putrefaction, or be putrefaction itself; that they contain cholic and choloidic acids as the binding agents of the granules of cholochrome, and that these ingredients are the same as the matters deposited by bile which is allowed to decompose spontaneously out of the body.

On p. 172 there is again the fallacious statement that calomel increases the watery portion, but not the solid constituents of the bile. The administration of this drug to a dog with a well-working biliary fistula has, under all circumstances, the effect of diminishing both the solids and total quantity of the bile, particularly when it produces purging. The common belief that calomel acts upon the liver as a stimulant of its function, and produces more bile than would be discharged without its agency, is a fallacy which has been over and over again exposed by experiments, observations, and discussions. But fallacies are tenacious of life, and propagate with all the means of the most noxious weeds. Tell the practitioner a thousand times that the green colour of the fæces after calomel is due not to biliary colouring matter, but to subsulphide of mercury, he will nevertheless give mercury, and believe it a cholagogue; tell him ten thousand times that no *bile* has ever been found in human fæces, and not enough of the products of decomposition of bile to make an analysis, he will nevertheless *see* large quantities of bile in the dejections after the use of blue pill and other mercurials, and deceive himself and others by the effete dogma of "biliousness" and its treatment.

The section treating of the blood is very elaborate, but does not exhibit any new points. The blood remains as inaccessible to chemical analysis as ever. Schmidt's mode of analysing blood, which is unreliable and unfeasible, we thought had by this time ceased to have any claim for being called a chemical proceeding.

Chyle, lymph, milk, and various other juices bring us on to p. 292. Sweat, we there see stated to contain formic acid. But sweat does not reduce nitrate of suboxyde of mercury on boiling, and the statement is therefore highly questionable.

Chapter XIV., p. 296, treats of the urine. On p. 302 the remarkable opinion of the Rev. Professor Haughton is introduced, that "no uric acid whatever should occur in the urine of man in perfect health, but that all the nitrogen of the urine should pass off in the form of urea, a more highly oxidated product than uric acid." What a divine abstraction, that "all the nitrogen of the urine!" The medical professor actually goes into this snare, and says: "There are several facts which support this view, amongst which we may notice, first, that of the urine of the dog containing no uric acid, while that substance occurred in the spleen; and secondly, that there is certainly no fixed proportion between the quantities of uric acid and urea."

The above emendations are most untenable. Man's urine always contains uric acid; but the Rev. Mr. Haughton says it ought never to contain any. We ourselves have been born with that accommodating spirit which submits to things as they are; rude reality shows us about nine grains of uric acid in a healthy man's urine, and we are glad to get rid of ours daily, for fear it might deposit itself in the knuckles which now wield the pen, and might give our adversaries an easy victory.

The author's statements relative to hippuric acid in the urine exhibit faithfully the contradictions of authors, but do not afford their explanation. This explanation we should have thought him all the more qualified to give, as he had made some experiments upon this subject. Weissmann, when living on a mixed diet, excreted thirty-three grains of hippuric acid daily, but when living on a purely animal diet he passed only twelve grains. Dr. Day thinks that these numbers are much higher than would be generally found, and says that in the course of "numerous analyses in reference to this constituent" made by him several years ago, he could never, unless after the ingestion of benzoic acid, find such an amount of hippuric acid in healthy urine as that given by Weissmann. He does not say whether he found hippuric acid constantly present or not, nor does he state the quantities which he actually found. He therefore has no opinion regarding the assertions of Hoefle, Duchek, and Haughton, that hippuric acid is not a constant ingredient of human urine. He admits Lehmann's assertion, that he found an "excess" of hippuric acid in the urine of diabetes and of fever, as being in opposition to the researches of Weissmann, which exhibited a diminution in these diseases, forgetting that Lehmann possessed no standard of the normal quantity, and consequently could not ascertain either increase or decrease in disease.

The solution of these absurd discrepancies has been given by Weissmann himself, and is simply this. If a large quantity of ether is employed for the extraction of the hippuric acid, and if the concentrated urine is repeatedly shaken with new portions of this solvent, until new portions of ether leave no longer any residue on evaporation, the large quantities of hippuric acid will invariably be discovered. But if the ether is used in a saving way, in miserly ounces, instead of pints, the results of Hoefle, Duchek, Haughton, and Day are obtained.

The description of chylous urine given by the author (p. 332) is very deficient. Chylous urine mostly contains blood-corpuscles, and when fibrine is present it is mostly accompanied with blood-corpuscles. Even when blood-fibrine and fat disappear from the urine of a patient who is subject to passing chylous urine at times, the clear urine continues to coagulate on boiling; and when albumen also disappears, tannic acid still throws down a precipitate of some peculiar albuminous matter. The occurrence of fat in chylous urine, which is the most remarkable feature of this disease, and much more frequent than the presence of fibrine, has not been correctly appreciated by Dr. Day. He has not even quoted the most satisfactory English observations on this head, and confines himself to noticing a most ridiculous and unfounded assertion of Lehmann—namely, "that in the milky or chylous urine the turbidity is not mainly due to fat globules" ("as was generally believed," says Dr. Day in parenthesis, while nobody who had seen chylous urine believed it), but to pus-corpuscles, which, however, contain a considerable amount of fat."

In order that our readers may be better enabled to appreciate this fallacy of Lehmann, we here insert a verbal translation of his original passage.

"Richness in *fat* has been particularly vindicated to the chylous or milk-urine, in which the fat is said to be suspended in vesicles as in chyle or milk. True milk-urine or chylous urine, in which the peculiar turbidity proceeds from fat, Lehmann has not observed; the urine of that kind was made turbid by a great quantity of suspended pus-cells, which in all observed cases took their origin in the kidneys, and not in a catarrh of the bladder. Where in reality such milk-urine has been found rich in fat, the fat may, according to Lehmann's opinion, as in the case of Rayer,* have proceeded from milk added to the urine with the intention of deceiving the physician."†

Nobody ever believed or said that fat in chylous urine occurred in vesicles, as in chyle or milk. The fat in this description of urine is minutely subdivided, so that with the highest powers of the microscope only the merest points can be seen.‡ Lehmann therefore opens his statement with a fallacy. Having admitted that he had not observed true chylous urine, although intending to insinuate that chylous urine did not exist, he is illogical enough to continue the second part of his sentence with "urine of that kind." What kind? Of that kind which he had not seen, and which did not exist? Absurd! He meant to say that urine which looked at all like that described as chylous, in his researches contained pus-globules. The insinuation that purulent urine, or urine fraudulently mixed with milk, had been mistaken for chylous urine, cannot require any further refutation.

On p. 352 we meet with a table exhibiting the average daily excretion of urinary constituents in an adult man of ordinary weight. The table contains an item termed "extractive matters." This very vague term is made to include the colouring and odoriferous ingredients of the urine. Are these matters called "extractive" because they cannot be extracted?

There are yet a great many points upon which we should have been able to offer important remarks. We can make all due allowance for an author's intention, however mistaken he may be in his means. We can admit freely, that Dr. Day has laboured hard to produce this treatise, and that he started with the fairest intentions; but his means

* L'Expérience, 1848, No. 42; and Traité des Maladies des Reins, t. i. p. 159. Paris, 1838–41.

† Gmelin's Chemie, Band viii. 2, p. 379.

‡ Bence Jones, Thudichum, Beale.

were quite inadequate to his purpose. However, until another and better work on the subject of physiological chemistry makes its appearance, Dr. Day's treatise will obtain circulation, and will not be worse by reason of the exhibition which we felt ourselves bound to make of its greatest errors.

REVIEW IX.

1. *On the Air of Towns.* By Dr. R. ANGUS SMITH.
2. *Articles "Putrefaction" and "Sanitary Economy" in 'Ure's Dictionary of Arts, Manufactures, and Mines.'* By Dr. ANGUS SMITH.
3. *Variabilités Normales des Propriétés de l'Air Atmosphérique.* Par M. HOUZEAU. ('Comptes Rendus,' vol. lii. No. 16, p. 809.)

On the Normal Variations of the Properties of Atmospheric Air. By M. HOUZEAU.

4. *Expériences relatives aux Générations Spontanées.* Par M. PASTEUR. ('Comptes Rendus,' vol. l. p. 303).

Experiments relating to Spontaneous Generation. By M. PASTEUR.

5. *De l'Origine des Ferments.* Par M. PASTEUR. ('Comptes Rendus,' vol. l. p. 849.)

On the Origin of Ferments. By M. PASTEUR.

6. *Moyens de rassembler dans un très petit Espace tous les Corpuscules normalement invisibles contenus dans un Volume d'Air déterminé.* Par M. POUCHET. ('Comptes Rendus,' vol. l. p. 748.)

Method of collecting the Corpuscles contained in a given Volume of Air. By M. POUCHET.

7. *Genèse des Proto-organismes dans l'Air Calciné, &c.* Par M. POUCHET. ('Comptes Rendus,' vol. l. p. 1014.)

Genesis of Proto-organisms in Calcined Air, &c. By M. POUCHET.

8. *Études Microscopiques de l'Air.* Par MM. JOLY et MUSSET. ('Comptes Rendus,' vol. l. pp. 647 and 934.)

Microscopical Studies on the Air. By MM. JOLY and MUSSET.

9. *Papers relating to the Sanitary State of the People of England.* By E. H. GREENHOW, M.D. Presented to Parliament by command of Her Majesty.—*London*, 1859.
10. *Reports on the Prevalence and Causes of Diarrhœa at Coventry, Birmingham, Wolverhampton, &c. &c.* By the same Author. ('Second Report of the Medical Officer of Health to the Privy Council.')—*London*, 1860.

THE art of hygiene is not based on any special science of its own; it is merely the application to practical ends of principles derived from sciences which have no necessary relation with it. Its purpose being to protect man from the influence of those abnormal conditions of the world outside of him, which give rise to irregularities in the performance of his vital functions, it is clear that the solution of its problems can require no other knowledge than that of the science of life or physiology on the one hand, and of chemistry and physics on the other. From the latter we learn the *nature* of those causes which pervert healthy functions; from the former, the *mode* of their operation.

It must be acknowledged, that although the applications of science to the art of prevention seem to be more direct and more simple than to the art of cure, hygiene is more empirical, that is to say, less under the control of science, than practical medicine; and this is a natural consequence of the fact that the impulse which hygiene has received during the last twenty years has been communicated to it entirely by the energy and zeal of practical men, and that it owes its present position not to the advance of scientific knowledge, but to the development of social life, and particularly of the social life of great towns. As therefore science has had so little to do in the recent revival and progress of hygiene, it is not to be wondered that she is often unduly excluded in the discussion of sanitary questions, or that she is sometimes unprepared to solve them when submitted to her.

In so far as hygiene is in this position it must be stationary, for an art can only progress by being brought under the dominion of knowledge. In the absence of that control, its history can be no more than that of a succession of trials and failures. But there is no reason why such a state of things should continue; now that sanitary art has taken the form of a system of practical principles or precepts, there is no reason why science should not act upon her evident competency to *judge* of these principles, not only in discriminating between the false and the true, but in distinguishing what is proved and certain from that which is merely presumed.

It is our purpose to show the application of this criticism to a subject which more than any other in the range of hygiene has hitherto been withdrawn from it, by bringing together all those results of scientific investigation during the last few years which bear upon it, and examining in their light the principles which have hitherto guided us. The public, led by the medical profession, has accepted, as established, doctrines relating to the production of diseases, which although not proved to be erroneous, have no certain foundation; such, particularly, as that which attributes the origin of specific morbid poisons to putrescence. To speak plainly what we know on this and similar questions seems to be now more than ever a necessity. We have in London a gigantic scheme in progress for the purification of the river, and the public look forward to its completion in the hope that the millions it has cost may be repaid at least in hundreds of human lives. If, as the most thoughtful among us believe, there is no scientific ground for anticipating such a result, it is surely time that we should say so; and that we should make it plainly understood, if for no better reason than to save ourselves and the science of medicine from the discredit of failure, that the septic theory, although containing a kernel of truth, has not yet received in any definite form the sanction of science.

The external conditions of life which are liable to be modified by the disturbing influences of habitation are exclusively those of air and food.

It is to the modifications of the former, as taught us by physical science, and particularly to those which exercise so unfavourable an influence on the health of town populations, that we propose to confine ourselves.

Chemical and physical modifications of the atmosphere consequent on habitation.—The repeated observations of chemists have taught us to regard the identity of composition of the atmosphere as a fixed law—one to which no exception is to be found in nature, unless it be in the neighbourhood of tropical rivers, where vast quantities of organic matter, the *débris* of a luxuriant vegetation, are rapidly passing into decomposition.* Everywhere, whether collected on the top of Mont Blanc, on the banks of the Seine or Thames, or in the middle of the Atlantic, the two main constituents of the atmosphere are found in precisely the same proportion, and the more perfect the processes of analysis have become, the more firmly has the constancy of this relation been established.† This fact has always, however, been rebelled against by the common experience of mankind; it has been almost an opprobrium to science that, in spite of the manifestly different *feeling* of the air on the Swiss mountains and in the middle of London, the chemist can detect no difference in composition. During the last few years, several chemists have directed their attention to this apparent inconsistency between the organoleptic and physical characters of the air with special reference to the condition of the atmosphere in towns. These researches have related mainly to the quantity of carbonic acid, and other products of combustion, and to the existence of organic matter in suspension. Among the most important are those of Dr. Dundas Thomson and Dr. Angus Smith.

The percentage of carbonic acid usually existing in the air of London was found by Dr. Roscoe,‡ to be 0·037 per volume, a result not differing materially from those obtained by Dumas and Boussingault in Paris. The analyses on which these are based were made by passing a known volume of air over weighed tubes containing alternately pumice-stone steeped in sulphuric acid and potash, a method which leaves nothing to be desired in respect of accuracy. Dr. Smith's estimations of the carbonic acid of the air of Manchester, made by the same method, give somewhat higher results. He found that on a windy day, they averaged from 0·045 to 0·08 per cent., and that on a still day the per-centage amounted to 0·12. When, however, we consider that although London is the greatest city in the world, Manchester is the largest manufacturing town, and that it is the centre of a manufacturing district comprehending many hundred square miles, over which an atmosphere darkened by smoke perpetually hangs, we are not surprised to find that the products of combustion exist in larger proportion than in London or Paris. Dr. Smith has calculated from the quantity of coal burnt in the neighbourhood of Manchester, that

* Daniell: Philosophical Magazine, fifth series, No. 121.

† Dumas and Boussingault: Récherches sur la véritable Constitution de l'Air: Ann. de Chimie et de Physique, troisième série, tom. iii. p. 257. 1851.

‡ Roscoe: On the Atmosphere of Dwelling-houses: Quarterly Journal of the Chemical Society, Oct. 1857, p. 259.

15,000 tons of carbonic acid must be introduced into the atmosphere daily, without taking into account the quantity expired by man and animals.

A much more important product of combustion is derived from the oxidation of the sulphur contained in coal, and the introduction thereby into the atmosphere of sulphurous and sulphuric acids. In the researches undertaken by Dr. Thomson* during the last epidemic of cholera, which consisted in passing large quantities of the air of London through distilled water, it was found that such air invariably possessed an acid reaction, and that this reaction was due to sulphuric acid. Dr. Smith has further investigated this question, and has found that in Manchester the acid reaction of the atmosphere is much more constant and intense than in London. Blue litmus paper becomes red in half an hour, and sometimes in ten minutes when exposed to Manchester rain, and occasionally its acidity is such that a single drop is sufficient to effect the reaction. The actual quantity, however, is exceedingly small; of a solution containing 1000th part of its weight of carbonate of soda, quantities varying from ten to fifty grains suffice to neutralize 1000 grains of such rain; and as much cistern water is found to be neutralized by twenty-five grains; from which results Dr. Smith concludes that the largest quantity of sulphur acids existing in the atmosphere of the town does not exceed 0·004 per cent. by weight, a proportion amounting to not more than $\frac{1}{20}$th part of that of the carbonic acid. As to the share of sulphurous and sulphuric acids respectively in this total, it is of course impossible to arrive at a conclusion; but considering what we know of the rapidity with which the former is oxidized in the air, it is to be supposed that whenever the acidity of the atmosphere is marked, it will be mainly owing to the latter. The impregnation of the rain with a mineral acid must be regarded as rather beneficial to health than otherwise, as tending to retard the putrefaction of animal matter on which it falls.

Dr. Dundas Thomson appears to have been among the first to recognise the importance of *organic matter* as a constituent of the air of towns, and to express the conviction that the gaseous products evolved during putrefaction are not the main sources of danger. Proceeding on this idea, he subjected a large quantity of atmospheric air to chemical investigation, "with a view of condensing any vapour, or detaining solid particles, which might be disseminated." The result was entirely negative.† Further inquiries of the same kind were made, under the sanction of the Board of Health, in 1854, the air being passed, as has been already mentioned, through distilled water, the result invariably being that hyphaceous fungi made their appearance in the water, and in a short time, by their rapid growth, pervaded the whole of it, so as to be evident to the unassisted eye.‡ It

* Appendix to Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of 1854, p. 119.

† Chemical Researches on the Nature and Cause of Cholera: Medico-Chirurgical Transactions, vol. xxiii., 1850.

‡ Report on Cholera, p. 127.

was also found, that on passing the air through sulphuric acid in the same manner, the acid became soon dark-coloured, in consequence of the charring of the organic matter introduced into it. Dr. A. Smith has worked out the idea much more completely. He has preferred a chemical to a microscopical test for the detection of the suspended organic matter. It consists, as most of our readers may be aware, in passing the air through a very dilute solution of permanganate of potash, the strength of which is determined by ascertaining how much is required to decompose a solution of a weighed quantity of oxalic acid, or of uncrystallizable sugar. This test obviously indicates, not the quantity of organic matter, but the quantity of oxidizable matter in the atmosphere, and hence it is only valuable in so far as we may assume that the atmosphere contains no reducing agent. Thus, in the presence of sulphurous acid, it would be of little value were it not that that agent exists, even in town air, in exceedingly small quantity. Many of Dr. Smith's results are of such a nature as to be beyond the possible limits of this source of error. It was found that the same quantity of the solution of permanganate which was decolorized by one bottle of air obtained in a close court in Manchester, required twenty-two bottles to decolorize it on the hills in the neighbourhood. Assuming that sugar and the organic matter of the air are decomposed by the same amount of manganate, "a supposition which cannot be perfectly true, but which, from the minuteness of the amounts, leaves no room for a great error," Dr. Smith concludes that whereas, on the high grounds north of Manchester, there existed but one grain of organic matter in 200,000 cubic inches; in close places in the town there was a grain in 8000 cubic inches. From his most recent observations he concludes that we have, "in different air breathed by people in the same county, a substance the amount of which in one case is twenty-two times greater than in the other, and in air breathed by people in the same town a difference which is as 9 to 22."

The whole importance of these investigations, regarded from the point of view of preventive medicine, lies in their relation to the putrefactive process. To discover a means of seizing upon and estimating putrid exhalations—under which term we include everything not gaseous that is disengaged into the atmosphere from the surface of living animals, no less than the exhalations from dead animal matter —would be certainly a most important step towards acquiring a more satisfactory knowledge of the influence of habitation on health. We have, therefore, to inquire what grounds there are for regarding Dr. Smith's test, or any other founded on a similar reaction, as affording a solution of this problem. It is not difficult to satisfy ourselves that animal matter in putrefaction does disengage from its surface portions of its substance of sufficient tenuity to be suspended in the atmosphere. Without referring to offensive smells, which of course must be material, we have several satisfactory proofs. If a bell-glass be inverted over decomposing animal matter in a moist condition, the inner surface of the glass becomes in a few days bedewed with moisture, which on being examined under the microscope is found to contain the same filamentous fungi to which reference has already been made, and on evaporation it leaves a residue, which is blackened by incineration. Similarly we find that the moisture which is deposited in glutinous drops on the sides and arched roofs of sewers, is rich in organic matter, which must clearly have been derived from the air of the sewer. Dr. Smith has related the results of experiments showing that air kept for a length of time in contact with putrescent matter becomes loaded with oxidizable material, and acquires the power of decomposing a correspondingly large quantity of permanganate of potash.

Another group of facts shows us that the existence of putrescent impurity in the air is a principal, though not a necessary, condition of the induction of putrefaction in bodies susceptible of the change. Thus, for example, I have found that milk which has retained its freshness for hours, will at once turn on being exposed to a putrid emanation. Butchers are familiar with the fact, that meat cannot be successfully dressed in the neighbourhood of a stinking gully grate, or of a stable reeking with ammonia; and for the same reason every intelligent butcher keeps his slaughter-house in a state of scrupulous cleanliness. It is not, however, to be forgotten that other causes, possibly electrical, the nature of which is still involved in obscurity, have a still greater influence in inducing putrefaction. Thus, in this country, the butcher finds that on one day he is able to slaughter and dress even veal or lamb with safety; whereas on another, not differing in temperature, incipient putrefaction may render the carcase unsaleable, in spite of the most careful precautions; butchers are apt to believe that this occurs mostly on calm days when the air feels heavy. Still more remarkable are the facts recorded respecting the slaughtering of cattle in hot countries; the operation can only be safely performed when the air is clear and the sky cloudless. Under such circumstances, we are told that the appearance on the distant horizon of a cloud "like a man's hand," the sure precursor of a storm, is a sign to the slaughterers on the Pampas of South America to desist from their work, for it is immediately followed by rapid putrefaction.

Air contaminated with putrescent matter is for the most part alkaline. Thus, the air of sewers is invariably so, as has been proved by the experiments of Dr. Dundas Thomson, its alkalinity being owing partly to ammonia, partly to sulphuret of ammonium, the form assumed by the sulphur disengaged in the decomposition of fæcal matter. The air of stables and stable dwellings is strongly alkaline, as every one in attendance on the sick poor in London well knows; and the air expired by men and animals, although at first probably acid, rapidly becomes alkaline by putrefaction. The relation between putrefaction and the existence of ammonia in the air is therefore so close that the detection of this body may under ordinary circumstances be regarded as a proof of its existence.

Bleaching power of the air.—It is known to those who are commercially engaged in air-bleaching, that the bleaching power of the air varies very considerably, not only according to the season and time of day, but irrespectively of such periods. It has not yet been determined what is the relation between this reaction and the oxidizing power of the air, as exhibited in its power of decomposing iodide of potassium. But the remarkable experiments of M. Houzeau (*loc. cit.*), made simultaneously in town and country, have shown that country air bleaches much more rapidly than town air, and that in this respect the difference is no less marked than in that of the well-known ozone reaction which has been so clearly shown to be destroyed by urban contaminations.

The *pressure, temperature, and moisture* of the air are but little modified by habitation. The readings of the barometer and hygrometer in town and country do not differ. There is, however, one respect in which the temperature of great towns may be favourably compared with that of the country—viz., in that of equability. This subject has been very fully illustrated by Mr. Glaisher,* in his report on the meteorology of London during the year 1854; and more recently by Professor Hennessy in a paper read last year to the Association for the Promotion of Social Science. Mr. Glaisher found that in the middle of London, the night temperature is much higher, and the day temperature considerably lower, than in the country, and consequently that the range of daily temperature is nearly twice as great in the country as in London, especially in clear weather, when the cloudless sky of the country contrasts with the smoky obscurity of town. This immunity from great variations of temperature must tend to diminish, though probably it does not at all counterbalance, the generally injurious effects of town air on persons affected with chronic pulmonary disease.

In the preceding paragraphs we have reviewed all the differences which are discoverable either by physical or chemical means between the atmosphere of towns and that of the country; and we are in a better position to determine in the light of physiology which of these conditions is likely to exercise most influence on the health of man. As regards the existence of an excess of carbonic acid, it is clearly of no importance whatever, for in many large towns no such excess is met with. Sulphurous and sulphuric acids, if they have any influence, must act as "colytics"—*i.e.*, as agents tending to arrest putrefactive change. The absence of sunlight, on which the more equable temperature of towns depends, has unquestionably an unfavourable influence, but one which is very limited. We are driven then to the only difference which remains—viz., that which depends on the existence of oxidizable matter, as indicated by its power of reducing certain metallic oxides.

Modes of Investigation.—From the preceding considerations, and from others belonging rather to the pathological aspect of the question, we have ground for believing that *periodical observations of what may be called the septicity of the air, that is to say, its power of exciting putrefaction, would lead to most important results as regards the causation of disease.* We cannot as yet identify the existence of putrescence in the air with its reducing power, but considering that the former pro-

* Report upon the Meteorology of London. Report on Cholera Epidemic, 1854, p. 21.

perty includes the latter, it is clear that in the possession of an accurate means of determining the one, we have gained an important step in the investigation of the other. The remarkable researches of Schröder* 'On the Filtration of the Air in relation to Putrefaction, Fermentation, and Crystallization,' seem to us to contain another indication of the method by which this investigation might be accomplished. At all events his discoveries are of much importance in relation to our present inquiry. Schröder found "that almost any organic body, heated to boiling in a flask loosely plugged with cotton while hot, remains for months and years altogether unaltered, although the air, after filtration through the cotton, has free access." Hence he concludes that the atmosphere contains "an active substance, which induces the phenomena of fermentation and putrefaction, and which is decomposed by heat and arrested by filtration." His researches "establish beyond doubt that the formation of fungi in fermentation and putrefaction takes place exclusively by means of germs conveyed by the air, and that these are arrested by cotton."

Pasteur, in researches undertaken with a view to the question of spontaneous generation, has repeated and extended the experiments of Schröder. He filtered a large quantity of air through gun-cotton, which he afterwards dissolved in alcohol containing ether. On allowing the solution to stand twenty-four hours, and carefully collecting the deposit for microscopical examination, he found that it contained, along with a few starch granules, numerous corpuscles, "the form and structure of which indicated that they were organized." The agency of these corpuscles in exciting fermentation and putrefaction was proved by several series of experiments of the following nature. A flask containing a solution of sugar, to which about half a per-centage of yeast had been added, was placed at a constant temperature of about 87°, after having been previously boiled and filled with air which had been passed through a red-hot platina tube. It was found that no change whatever took place in the liquid, even after remaining many weeks at the temperature of fermentation, so long as the flask was hermetically closed, but that as soon as a little bit of cotton which had been employed as an air-filter was introduced, both *mucedines* and infusoria appeared at the end of from twenty-four to thirty-six hours. These experiments were variously modified, with similar results. Thus asbestos was substituted for cotton, and it was found that neither substance produced any effect on the liquid, unless it had been employed as an air-filter. It was further found that a fermentescible liquid could be kept open to the air without fermentation for an indefinite period in a long-necked flask, the neck of which was bent horizontally, provided that the liquid had been previously boiled, so as to expel the air, and destroy the vitality of the germs it originally contained; but that if the neck were cut off below the bend, fermentation would at once commence. This experiment seems clearly to show that the action of the air on fermentescible liquids is exclusively due

* Liebig's Annalen, Band cix. Heft 1.

to its corpuscles. In extending the same mode of investigation to urine, the author found that putrefaction was subject to precisely the same conditions as fermentation, and influenced in exactly the same manner by the presence or absence of the solid particles suspended in the atmosphere.

M. Pouchet has investigated the corpuscles of the air by a different method, and has arrived at somewhat different results. His plan is to direct an extremely minute but rapid current of air against a flat disk of glass. The air corpuscles are deposited on the glass "by precisely the same mechanism that the particles of metal are deposited on a porcelain plate in Marsh's apparatus," forming a little spot not more than a millimetre in diameter. The glass is then placed under the microscope. M. Pouchet denies the existence in the air of any bodies which can be proved to be germs or ova, and endeavours to throw discredit on the researches of Pasteur and others, without, however, advancing any experiments tending to show that their results are erroneous.

Enough has been already advanced to render it probable that the investigation of the property of the air to induce fermentation or putrefaction in liquids susceptible of those changes, or—if we may be permitted to coin a word—its *septicity*, must be of the greatest importance to hygiene. Whatever theory we may be disposed to adopt as to agency of germs or *ova*, it is clear that useful results would be obtained by making continuous observations on the variations to which this reaction is liable, and the effects of such variations on health. Pasteur's and Schröder's experiments suggest the method. A solution of urea or cane-sugar containing a ferment, and maintained at the proper temperature, might be subjected to the action of the atmosphere at different places and periods, and the time required for the induction of alkalinity in the one case, or for the development of carbonic acid in the other, noted.

Another most important line of investigation arises from the fact already stated, that ammonia exists in the air as a result of putrefaction, and will be found in quantity proportionate to that of the organic septic products. This research has been hitherto rendered impossible by the absence of a chemical method applicable to the purpose; but recently a reaction has been discovered which renders it possible not only to prove the existence of ammonia in the air with facility, but to determine its quantity with great accuracy. It is probable that this may afford a means of serial observation on the quantity of ammonia contained in the air at different times and under different local conditions, and may thus have an important bearing on the questions under our consideration.

Sources of organic impurity in the air.—It is evident that, with the exception of graveyards and certain manufactures of which putrescence is the raw material, the insufficiently rapid removal of solid, liquid, and volatile excreta is the exclusive source of the contamination of the air with septic impurities; and that so to accelerate their removal that the putrefactive change shall not have time to commence, is one of the most important problems in the hygiene of habitation.

That part of the problem which relates to the removal of solid and liquid excreta comprised under the general term sewage, has been generally discussed on grounds so entirely apart from the domain of hygiene, to which it of right belongs, that one is disposed to assume an apologetic tone in introducing it. But there is no subject outside of the pale of science, and no question, however practical, which will not be better and more fruitfully discussed in the light of knowledge, and with the desire to attain to truth, than in any other light or with any other object.

The danger arising from any atmospheric contamination varies inversely as the rapidity of its diffusion. Malaria in a valley is more dangerous than on a plain, and so what is harmless in the open country may be perilous in a pent-up court in London. A slight pollution in the streets is worse than the foulest stink in the river, and an almost imperceptible taint *in* the house is far more to be feared than the most offensive open sewer grating outside. It is therefore more important to eliminate sewage and all the products of its decomposition from our houses, than to get rid of it in the streets or in the river; but this is precisely what is most difficult to accomplish. From common experience we know that sewer air very frequently enters our houses, and from experimental investigation* it can be shown that there is during the greater part of the year a constant difference of pressure between the house and sewer atmosphere, and that consequently the air of the sewer, impregnated as it is with organic impurity, passes in a continuous stream by every available chink and crevice from the one to the other.

In Paris and Berlin, where they are so much behind us in their practical appreciation of the value of a wholesome and comfortable dwelling, they have taken time to profit by our experience, and seem to be little disposed to follow our example. The so-called sewers with which one-third of the streets of Paris are provided, are not sewers in the sense in which we understand the term; that is, they contain no "sewage," and are intended merely for the conveyance of liquids, whether excremental or otherwise. From a recent report, it appears that the system now generally adopted in Paris for the removal of solid and liquid excreta is that of moveable *séparateurs*. The *séparateur* is an iron box open at the top, and the sides of which are perforated with numerous holes, the purpose of which is to retain all solid matters, and to allow all liquids to percolate into the sewers, by which they are conveyed into the river. It is so fixed as to be easily removed and reinstated; and it is said that this process occupies only ten minutes, and gives rise to no annoyance whatever, the full boxes being placed immediately on their removal in an air-tight case for transportation.

The *séparateur* system not only prevents in great measure the pollution of the river, but is entirely free from the much more serious

* Quarterly Report on the Health of Paddington, Christmas, 1861.

drawback to which the system adopted in London and other large English towns is liable, arising from the existence of an impure subterranean atmosphere of immense extent, the tension of which is generally greater than that of the houses, so that it tends to make its way into them by every chink and crevice. However perfectly a sewer intended for the conveyance of solid matters may be constructed, it must necessarily be of sufficient size to allow of the entrance of workmen. But in London and other large towns the older sewers are not only much larger than necessary, but from their form and the mode in which they were built, are to be regarded rather as depositories of sewage than as mere channels for its conveyance. So much expense has, however, been incurred within a recent period on works of this nature, that no departure from the present system is to be thought of. It remains to the hygienist, after having pointed out the unavoidable evils connected with it, to devise the best means of diminishing or counteracting them. There are obviously only two ways of preventing the contamination of our houses by sewer-air—disinfection and ventilation. There can be little doubt that the introduction into the house-drains either of substances capable of arresting putrefaction—such as the mineral acids, creasote, and carbolic acid—or of substances capable of rapidly decomposing and destroying putrescible matter, such as chloride of lime, would be attended with the most satisfactory results. When, however, it is considered that the evil to be remedied appears to most people imaginary, there is little ground for hope that an expedient, the use of which could not be unattended with trouble and expense, would ever be so generally adopted as to be useful.

The difficulties of ventilation are equally great. In the ventilation of a house-drain the object we have in view is to diminish the tension of the air which it contains in such a manner that air shall tend to enter it by all existing openings, instead of passing out of it, as at present. How to effect this is a difficult problem. The measure which at once suggests itself is that which is adopted in coal mines—viz., to connect with each system of sewers a heated up-cast shaft of sufficient height to exhaust the foul air, and at the same time disinfect it by combustion. It is often imagined that if this were carried out, the evil would be remedied; but, on more careful consideration, it is evident that a system of sewers, not being a closed cavity like a coal-mine, but one which is interrupted by a multitude of openings through its whole extent, is not capable of being exhausted in this manner, any more than it would be possible to produce a vacuum with the air-pump in a perforated receiver; and experiments have shown that the effect of a ventilating shaft is in fact limited to the sewer with which it is immediately connected.

All that remains, therefore, to be done in the absence of any system either of ventilation or disinfection of general applicability, is to substitute well-constructed drains of impermeable material, in all possible cases, for those of brickwork; and in every instance to provide for the free access of the external air to the space under the house through which the drain passes, so as to reverse those physical conditions on which, as we have seen, the escape of sewer-air immediately depends.

Disease dependent on septic contamination of the air.—We may now proceed to inquire what are the diseases to which the existence of organic matter in the atmosphere gives rise, bearing in mind that our purpose is not to establish its generally injurious effect, but rather to limit and define its mode of operation in accordance with the facts of pathology. Nor, on the other hand, must it ever be forgotten, in such an inquiry, that other causes, rather moral than physical in their nature, are in powerful operation in towns, such as the luxury of the rich and the vicious recklessness of the poor—the neglect of maternal duties, arising from improvidence or the employment of mothers in manufactures; and, above all, the exhausting strain of the energies of the mind in the struggle for existence, all of which are inseparable from the social life of great cities, and tend to depress the vitality of their population.

There is no worker who has thrown more light on the question before us than Dr. Greenhow. He has shown by a large and laborious investigation, what others only surmised, that we have hitherto attached far too much importance to the so-called zymotic disease as indices of the agency of local causes of unhealthiness. The facts which he appears to have established are the following: 1. That there is no constant relation between the general prevalence of disease and the mortality arising from acute specific diseases; that is, diseases dependent on specific morbid poisons. This inference is derived from a comparison of the mortality arising from each of the diseases in question with the total mortality in urban and rural populations inhabiting all parts of England. The comparison yields the same result in every instance excepting that of measles, which owes its danger to pulmonary complications induced by local causes; and that of small-pox, of which the mortality varies with the neglect of vaccination. From scarlatina nearly the same average annual proportion of deaths occurs in the most salubrious districts as in the most unhealthy towns; while as regards fever, there is no marked difference between London and the country, some rural districts being subject to mortality from this cause considerably higher than the metropolis. On this point, however, it must not be forgotten that statistics are rather uncertain, no distinction being made between fever which is specific and contagious, and that kind of fever which has no fixed duration, no capability of reproducing itself, and (as we believe) derives its character as well as its origin from septic impurity, or, to use Dr. Murchison's word, is pythogenic. 2. The second fact is, that the three groups of diseases which Dr. Greenhow has designated as "pulmonary affections, alvine flux, and the nervous diseases of children, are both absolutely and relatively the chief causes of high death-rates." The mortality from each of these causes is subject to enormous variations, and these variations exhibit an obvious coincidence with the aggregation of life in large towns. Thus, in Liverpool these diseases occasion 54·6 in every 100 deaths; while in Glendale, a rural district, they constitute only 34 per cent. of the total mortality. The nervous disorders of infancy exhibit an even greater variation. Whereas only 25·8 per thousand die annually of these diseases in the most healthy rural districts, 383 per thousand die in some manufacturing towns. The variations of the infantile death-rate from pulmonary disease are no less striking, though not so enormous as those above stated. Both are clearly dependent on the poisonous in-door influences which act so powerfully on infantile life in towns.

Since the publication of the work from which I have been quoting, Dr. Greenhow has completed another inquiry relating to the local causes of diarrhœa. The mortality from profluvial diseases varies enormously. The largest, according to Dr. Greenhow, occurs in Liverpool, where it amounts to 6·8 per thousand; the smallest in Aberystwith, where it is only 0·4 per cent. But there are many other places from which these diseases, so fatal in towns, are in fact absent. The inquiry has very clearly established the existence of two local causes as accounting for these enormous differences—"the breathing of an atmosphere tainted with the products of animal decomposition," and "the drinking of impure water. The former of these causes is the more prevalent; and the mortality has, with few exceptions, been nearly in exact proportion to its intensity."

It would be wholly impossible within the limits of this paper to place Dr. Greenhow's facts before the reader in a satisfactory manner—facts which would have been better known to the profession than they are, had they not presented themselves in the forbidding garb of a Parliamentary blue-book. Whatever allowance we may be disposed to make for possible error or uncertainty, the conviction is forced upon us that the supposed analogy between the organic impurity of the air and the diffusible virus of contagious disease has entirely misled us in our attempts to estimate the influence of such impurity on health; that it is not only *not* proved that contagious diseases are called into existence by a septic ferment, but that there is strong reason for believing, on the contrary, that putrescent matter in the air exercises its pernicious influence, not by reproducing itself in the living organism after the manner of a specific virus, but by depressing or impairing the functions of the whole body, and particularly of those organs with which it is brought into contact, manifesting itself constitutionally by fever accompanied by prostration; locally, by affections of the respiratory and digestive mucous membranes.

Review X.

The History of Medicine. By Edward Meryon, M.D., F.G.S. Volume I.—*London.*

The want of a history of medicine which should be at once sound and popular, has been long felt. Le Clerc's history, with Freind's continuation, is obsolete; Sprengel's work is too voluminous for the great mass even of the professional public; and articles in cyclopædias or special monographs on particular subjects are no proper substitute for a summary of facts. Perhaps the want is even greater for the public at large, who are practically reduced to incidental notices in non-professional writers like Hallam and Buckle, than for physicians. A sound view of the growth of medical science would go far to dissipate the gross errors which still prevail among men otherwise educated. That medicine has shared the fortunes of experimental science generally, and has advanced, *pari passu* at least, with our common civilization, is a truth that would scarcely need demonstration if it were not constantly doubted or denied. The mitigation of epidemic disease, in spite of the enormous growth of population, and the greater averages of life at a time when the sickly as well as the strong grow up, are facts which ought to outweigh the many palpable deficiencies that still exist in our therapeutics. But the ravages of the Black Death or the Plague are little remembered by those who witness cholera, and by a singular anomaly, a failure to deal with chronic disorders is regarded more leniently than the inability to arrest those stages of collapse which are rather the beginning of death than disease. An irrational scepticism is commonly associated with a baseless faith; and if the quackeries of our own time are moderated by the public opinion of a powerful professional body, the success they achieve is in a greater contrast with the real advance of the century than it was in the days of Bishop Berkeley and Mead. A belief in tar-water was more rational a hundred years ago than the belief in infinitesimal medicines is now.

These are strong reasons why a book professing to give the history of medical delusions and progress should be welcomed as an addition to literature, if it fairly achieved its purpose. It is unfortunate that Dr. Meryon's book fails to do this. He appears to have read widely, but it is clear that he has not sufficiently thought out his subject. He is ignorant of many points of general history to an extent that is sometimes almost incredible. His style shows want of care in many parts, and the clerical errors still uncorrected are numerous and important. Above all, he starts from no definite idea, and follows no regular plan. His book is neither a history of scientific progress, nor of the lives of physicians, nor a record of disease, but is a patchwork of all these. Names and facts are so accumulated as to weary the eye and mind, and a passage from one authority frequently contradicts or modifies a previous assertion taken from another. We are sensible that there is great reason to excuse defects in a work on so vast a subject. The number of special monographs on medical history (such as Hecker's Epidemics of the Middle Ages) is still too small to be of much real assistance to the historian of the science. An accurate knowledge of the history of mind and of the sources of general history is required to supplement professional erudition. These are all reasons why Dr. Meryon's failure should be judged leniently. But they are also reasons why, in the absence of these qualifications, he should never have undertaken this particular work. As it is, we doubt whether any mere remodelling could make his volume a standard addition to literature. The chapters on discoveries in anatomy are unquestionably the best in his book, but their merit is not so remarkable as to redeem its general worthlessness.

The task of pointing out petty mistakes is one that only extreme necessity can justify. It is, perhaps, the printer who calls Melanchthon Schwarzend instead of Schwarzerd (p. 274); who quotes "Hecker" as "Herder" in three instances (pp. 210, 211, 272); and who transforms the Louis IX. of p. 196 into the Charles IX. of p. 421. But it cannot be the printer who has confounded the two Picos di Mirandola, and extended the life of the uncle from 1494 to 1533 (pp. 384, 387). The legend of Caliph Omar and the Alexandrian library has been disproved by writers as old as Gibbon and Heyne, and more recently by Fournier;* and the story that King Alfred appointed twenty-six professors to each college in Oxford, is a fiction so monstrous that no modern historian has ever thought it worth his while to mention it. Perhaps three out of five historical inquirers still doubt whether any school existed at Oxford, at least before the time of Edward the Confessor. Lepra can hardly have been brought to Western Europe during the thirteenth century (pp. 169, 192), since Archbishop Lanfranc founded an hospital for lepers in the eleventh. Often Dr. Meryon's language would convey a false impression to a reader where our author is perhaps innocent of any worse offence than careless writing. For instance, Hippocrates, who was born 460 B.C., ought not to be called the contemporary of Æschylus (p. 22), who died 456 B.C. No one thinks of Lord Macaulay as the contemporary of Pitt. It is more difficult to understand how Gerson can ever be said to have adopted the doctrines of Wickliffe. (p. 223.) It is the most lamentable point in Gerson's life, that so great and good a man as he undoubtedly was, took a prominent part in procuring the condemnation of Huss and Jerome of Prague, for Wickliffite opinions. Gerson was a reformer in the same way that Cardinal Pole afterwards was. Each wished to see his church purer and more efficient, but each regarded separation from it as a crime against society and God.

Were mistakes of this kind the only or the chief blemishes in the 'History of Medicine,' it would be sufficient barely to notice them, that the author might correct them in a second edition. But Dr. Meryon thinks as well as writes inaccurately. A passage in which he discusses the philosophy of Democritus will serve as an instance of his metaphysics. We give it in his own words.

* Orosius says that the Christians of his own time were accused of destroying the public libraries of Alexandria, and adds, "quod quidem verum est."—Hist., lib. vi. c. 15.

"It may not be uninteresting to observe that during this early period of medical history the doctrine of the immortality of the soul originated as a logical consequence from the speculations of Democritus relative to the composition of body and mind. This first atomic theory and the dogma of indestructibility of matter being admitted, the immortality of the soul necessarily followed; for the soul, according to his theory, being material, or made up of simple atoms, and all material substances being indestructible or eternal, the soul was necessarily eternal or immortal." (p. 19.)

The doctrine that a man's personality does not perish with his body, or that there is a life beyond the grave, miserable and shadowy, but still life, was an article of popular belief long before Democritus was born, and is constantly referred to in the 'Iliad' and 'Odyssey.' The doctrine of metempsychosis, or of a personality that clothes itself in different organic forms, was taught by Pythagoras, who flourished in the sixth century before Christ. It is probably nothing more than the earliest expression of those longings after immortality which haunt every man; and is the transitional phase, as it were, between the vague dread of perishing like the brutes, and the Christian faith that even the bodily type will be restored after dissolution. But Democritus was not troubled by any speculations of this sort. To him, as to Greek philosophers generally, the soul was only the vital principle, whose plastic energy organized matter into form. Assuming the universe originally to have consisted of infinite space, in which incorruptible atoms were ceaselessly whirled about by mutually repulsive forces, the question was to explain how any order and form had been evolved out of chaos. Democritus found the solution in the theory that certain round and fiery atoms attained, through greater perfection of form, to the qualities of motion and thought.* Once assume self-moving and thinking entities to exist, and the genesis of animal life was intelligible. Whether Democritus believed or not, like his countrymen, in spectral forms representing what had once been living men and women, is highly uncertain. His philosophy has commonly enjoyed a bad repute for its supposed tendency to substitute nature for God, and matter for mind. But to assume that he considered the soul eternal, because it was made up of eternal atoms, would surely be false reasoning, even had he meant by the word soul what modern Englishmen mean. Probably Dr. Meryon himself believes sufficiently in the atomic theory to regard the body as composed of primary parts, oxygen, nitrogen, and so forth, that cannot in themselves be decomposed by any known agencies. Yet the body is none the less perishable, because its elements cannot suffer corruption.

It would be tedious and unprofitable to discuss separately the errors of this sort which are scattered through Dr. Meryon's pages, and destroy the value of his labours. We prefer to indicate briefly the causes of his failure, and the direction which inquiry into the history of medicine ought to take. The writer of medical history ought to think clearly, and express his thoughts with precision. An acquaintance with Herschel's 'Natural Philosophy,' or Mill's 'Logic,'

* Aristotelis Metaphysica, i. 4, 9; iii. 5, 5.

would save an intelligent man from such verbiage as "the immutable truths of modern chemistry," as if all truth were not immutable, or as if our present conceptions of chemical science were final. A study of the original sources of history is indispensable. Herein lies Sprengel's great excellence; he has either read the authors whom he quotes, or his notes indicate that his knowledge is second-hand. The misfortune is that, writing in the eighteenth century, when antiquity and the Middle Ages were imperfectly understood, Sprengel's view of the general progress of society was essentially unsound. Hence it is impossible not to see that even where his facts are right, they are judged from the standing-point of a period with which they have no connexion. In criticising Aristotle, he is really thinking of Buffon and John Hunter. Lastly, the analytical method which Sprengel followed is not adapted to a single short volume. If the history of medicine throughout the world during two thousand years can ever be condensed into five hundred pages, it must be in some different way from that of selecting a few names here and there, a few notable events, or a few facts in the history of thought. The mere arbitrariness of such a method may indeed serve to shield the writer from attack, as he can always plead that his omissions were intentional. But the student who desires something complete in itself is terribly perplexed by a sketch of medical progress that does not mention Aristotle's theory of the vital principle, which Bichat thought worthy to be reproduced, and passes by the curious plagues of the sixth century, which Gregory of Tours has recorded.

The early history of medicine in its successes and its failures must start from a clear understanding of the conditions of household life among the ancients, and of the philosophies which influenced physical science. It is scarcely worth while seriously to discuss the myths of Osiris and Isis, or the story of Machaon, unless the historian adopt Plato's view, that the era of the Trojan war must have been a golden time for the constitution, when a warrior could dine heartily off cheese and wine immediately after a dangerous wound had been dressed. If no histories of Greece and Egypt existed, it would still be certain that the science of preserving health must have preceded the science of therapeutics, and it would be highly probable that a people in an early stage of society would shrink with a superstitious dread from human dissections. Fortunately there is ample documentary proof of both these positions. The scrupulous personal cleanliness which was cultivated in Egypt, especially by the priests, and which passed into the Jewish ceremonial law, was the necessity of a hot climate. The discrimination of different kinds of food for the labouring and the sedentary classes, was probably in the first instance derived partly from observation of their digestive qualities, and in part based on the wish to distinguish ranks. It is difficult not to suspect that the Pythagorean abstinence from animal food and vegetables containing a large proportion of fibrin had originated in some wish to subdue the aphrodisiac element in our nature by an appropriate regimen; however later times may have coloured a simple rule of diet with a transcendental theory. The classification which Herodotus gives of special doctors in Egypt is clearly not meant to be exhaustive, but probably gives the chief varieties. He distinguishes physicians of the eyes, the teeth, the head, and the stomach, from others who took the care of obscure disorders. The notorious prevalence of ophthalmic and leprous diseases in the valley of the Nile will explain the first and third class; while the excellent condition of the teeth found in mummies makes it likely, that only the simplest operations of dental surgery were in demand. The practice of monthly purgings explains the duties of stomach doctors, and the practitioners in occult diseases were probably much like the wise men of our country districts. If, therefore, it be really true that the Egyptians were a singularly long-lived people, the credit is no doubt due to their temperate and cleanly lives, rather than to their medical science. It is remarkable that they never seem to have risen beyond simple though sound conceptions of the common rules for preserving health. The system of castes, and their subjugation by the Persians, explain this sudden stagnation of a people early civilized and in constant intercourse with Greece. The influence which their religious and scientific traditions exercised on the highest Hellenic thinkers is the more remarkable. It seems as if the energetic and restless Ionians were subdued by the mere grandeur of the immobility which they witnessed, and found a higher meaning in Egyptian legend than it had ever possessed. Except for the discovery of the diuretic properties of squills, and the alterative of the sesquioxide of iron, the history of Egypt before the Ptolemies would be a blank in the annals of medical progress.

The contrast of Greece and Egypt is all the more remarkable. In Egypt, where hundreds of corpses were daily opened to be embalmed, the knowledge of anatomy was of the slightest description, and the best surgeons in one memorable instance could not reduce a simple dislocation of the astragalus. In Greece the human corpse was buried with all speed, and the prejudices of the age forbade its opening. Yet, in spite of this, partly from dissections of animals, partly from observations in cases of wounds, and partly, no doubt, from the constant facilities for seeing the play of the muscles in boxers and runners under training, a knowledge of human anatomy gained ground steadily. The union of art and science which the best days of Greece exhibited, and which produced such works as the marvellous statues of Venus and the Laocoon, can only be paralleled in the Italy of the Renaissance, when Leonardo da Vinci dissected, and when Titian prepared the sketches for Vesalius. But above all, the idea that health depended mainly on such circumstances as climate and food, was developed into the belief that almost any bodily condition could be produced by appropriate training. The science that fitted candidates for the Olympic games reacted upon all medical theory. The disadvantages of "muscular Paganism" were easily seen. Hippocrates pointed out, that high feeding and a purely animal life tended to produce a state of plethora which easily passed into disease. Plato complained that medicine now was called in for the supervision of all life, instead of being confined to cases of accident, a wound, or mania. He contrasted the brutal indolence of the gymnasts, who slept away most of the time when they were not taking exercise, with the physique appropriate to philosophical speculation. The justice of his censures cannot be doubted. Mr. Hawthorne, in his interesting account of the Communistic Utopia started in America, where the thinking man was to work in the fields, and the working man to discuss philosophy with his fellows, observes that the habits of farm life completely deadened the love of speculation and argument. The philosopher, who was out in the fields six days in the week and ten hours in the day, came home fit for nothing but a hearty supper and bed, and spent the Sunday in lounging about the stables and the yard. Yet it may be doubted whether Greek society lost much in the men who wasted their energies on the palæstra. Men with any mind to exercise would hardly waste their lives upon physical exercises, and the small heads of the boxers and wrestlers in Greek statues are some proof that the fascination of the games was chiefly exercised upon meaner minds. The benefit to art of an improved system of dietetics must have amply compensated the loss of Epeius and Euryalus from the agora.

Among theories of thought that influenced the art of healing in Greece, the first in logical order is the doctrine of the elements. We are apt to smile at the seeming childishness of aphorisms that the All was water, or air, or fire. Yet Thales, Anaximenes, and Heraclitus were giants in their time. They found the world peopled with picturesque deities, who inhabited the mountains and groves, who made the fountain bubble up, or presided over the growth of the tree. Even if the philosopher assumed the river-god and dryad to exist, he was still perplexed by the feeling that there must be some common element which the demigods shaped into form; that Alpheus was distinct from the river in which he flowed down to the sea, and had not called it out of nothing. Their problem was really that of the mediæval alchemist to discover the original matter of which all visible things were a modification and the principle of change. In one form or another they all arrived at the same general result, that change was corruption, and that the original substance had been the pure, perfect element. The life of the gods was conceived as one severe intellectual intuition in cloudless ether, and the eternal order of the stars was contrasted despairingly with the flux and reflux that went on in animal life. These doctrines harmonized with the whole mythology of the times. Plato's dream of "the ancients who were better than we are, and lived nearer to the gods," was derived from a thousand popular legends of the Golden Age, when youth was self-renewed and crime unknown. Of course such a theory as the Hippocratic, that men had arrived, by a gradual process of selection, at a knowledge of their appropriate diet, pointed rather to a struggling upwards out of barbarism; and legends like that of Prometheus represent this side of the question. But such contradictions are inherent in all mythologies. Indeed, many of those who held that knowledge and civilization had advanced with time, regarded that very progress as a proof of degeneracy, and complained that crime had called forth law, disease medicine, and luxury the arts.

From what we may call the chemistry of metaphysics, contained in the doctrine of the elements, the next step in thought was to the comparative physiology of man and the world. What has been naturalized among us in later times as the doctrine of the macrocosm and microcosm, was first popularized by Plato. It seems to rest upon two premisses, that God is to the earth as the soul is to man, and that the soul is fashioned in God's image, and contains in itself types of the laws of the universe as primary ideas. From the first of these principles were evolved a number of fanciful comparisons—such as, that the rivers were the earth's blood, and the rocks and mountain-ridges its skeleton. From the second, it followed naturally that the body would have sympathies with the visible world, and be subject, like it, to changes of the moon and planetary influences. A passage in Aulus Gellius tells us that muscular fibre grew larger, oysters fatter, and the eyes of cats fuller, as the moon increased, and that they dwindled in like manner during its wane.* Moreover, as earth and body were alike matter animated by the Divine Mind, an easy analogy led to regarding the body as a mixture of the four elements. Greek speculation never ventured to dream, like the mediæval alchemists, of resolving these elements again into the one out of which they had originally been evolved, so as to render the body incorruptible. The comparative disregard of chemistry was no doubt one reason of this shortcoming. But another lay in the strong preference for mind over matter which philosophy inculcated. The very men who saw the glorious physical frames of the athletes and the statues of Phidias, regarded the Deity as pure intellect without bodily shape; and aspired, like the Stoics, to subdue pleasure and pain, or, like the Alexandrian Platonists, to lose the sense of personal being in theosophic ecstasies. Christianity redressed the balance, by teaching the resurrection of the body.

The doctrines that man's body was composed of the four elements, and in *rapport* with the universe, were supplemented by the teaching of Aristotle. His distinctions of the different modes of life, nutritional in plants, perceptive in brutes, and rational in men, had a straightforwardness and truth about them that have given them an honourable place in the history of science. But wonderful as are the merits of his treatise 'On the Principle of Life,' and his Zoology, they impressed his contemporaries less than the doctrine of the Mean. That all truth lies between two extremes, excess and defect, is no very profound teaching, but it had a general applicability, which made it popular. Hundreds of men had said something like it without any idea that they were striking the key-note of philosophy. It was especially applicable to medicine. The whole theory of health and disease might now be given with the compactness of a purely logical system. What could be more clear than that maladies arise from the predo-

* A. Gellii: Noct. Att., lib. xx. cap. 8.

minance or defect of one of the elements, and that the province of medicine lay in restoring an equilibrium? If the patient had caught cold, or been frost-bitten, the absence of fire must be redressed by hot medicines. All this is absurd enough, and must constantly have led to deplorable consequences, although no doubt the new discoveries of a sound empiricism could always be adjusted in some way to the old premisses. But in its application to diet, the doctrine of the Mean was sufficiently harmless, as it commonly meant temperance.

The state of health in Greece no doubt differed very much at different periods. The contrast which Aristophanes loves to draw between the sturdy, healthy men who fought at Salamis, and their effeminate descendants who attended the lectures of sophists and meetings in the Pnyx, is chiefly to be ascribed to political rancour. Changes so vast can scarcely have taken place within sixty years. But it does seem probable that a great moral deterioration accompanied and occasioned the fall of Greece. The old institutions had been relaxed and the old faith undermined. Unnatural crimes among men, and drunkenness among women, made the Greek a by-word among nations as his race dwindled in physical stature, lost its old courage, and only retained so much intellect as would serve to flatter and cheat. Probably temperance in food was one of the last virtues lost. That the Asiatic army of Xerxes took only one meal in the day, is scarcely so curious as the observation in a Hippocratic treatise, that many persons in Greece found their digestion unequal to more than one repast.* Yet, if we judge from the comic writers, there was no contempt for the good things of the table. "Everything," says Dicæopolis in the 'Acharnians,' "is prepared—couches, tables, cushions, mattresses, garlands, myrrh, dessert, women, meal-cakes, flat-cakes, sesame-cakes, honey-cakes;" and he goes on to enumerate the more substantial pleasures of salt-fish, forcemeat, cushats, field-fares, hares, and locusts. Physicians and gourmands alike distinguished between bread baked in small pots and that which was prepared in ovens. In fact, bread prepared from unripened corn and green fruit were the chief recognised causes of stomach disease and fevers throughout Greece. Where diet was so important, and the pharmacopœia so slenderly furnished, it is not wonderful if Hippocrates declared it difficult to distinguish between medicine and food. The comparative absence of skin disease was probably due to the frequent use of the bath, and to the fact that salt provisions were mostly a luxury, and not the staple food of the lower classes at any time of the year. It was disreputable as well as dangerous for any man to drink the strong, sweet-scented wines of the highest repute without a plentiful admixture of water; and the common sorts that were retailed at twopence a gallon were of course nothing more than a *vin ordinaire.* Yet it is probable, on the whole, that the Greeks, although temperate in the main, more often sinned on the side of drunkenness than of gluttony.

The transition from Greece in the times of Pericles or Demosthenes

* Hippocratis de priscâ Medicinâ, ed. Kühn, vol. i. p. 33.

to Rome under the Emperors, displays a singular stagnation of medical science and a great change in the habits of society. A larger but less critical acquaintance with zoology, and a larger but scarcely more useful pharmacopœia, testify to the decay of thought and the want of a healthy public opinion. The Greek and Roman conquests in Asia had swamped the small civilized world with a number of barbarous fellow-citizens; Syrians swarmed in the streets of Rome, noble matrons were associated in the mysteries of Isis and Mithras, and beliefs in demons, witches, and vampyres replaced the faith in gods, who were at least human in their forms and frailties. The legends and quack nostrums of every country were collected as data for science by practitioners who were groping in the dark for knowledge. Great excuses must be made for them. In the general want of glass to hold acids or conduct gases, and when coal fires were unknown, no real advance could be looked for in chemistry, and most of our mineral medicines were impossible. But it is difficult not to think that better observations might have been handed down, and the discoveries of the sixteenth century in anatomy anticipated. Galen's great systematizing power seems really to have been fatal to the profession. The encyclopædic character of his works furnished them with a solution for almost every difficulty, and if on the one hand they made the best science of the times universal, on the other hand they established a universal church which tolerated no difference. Religion aided the general apathy. The fathers of the Church proscribed philosophy on account of its heathen associations, and were prone to see miracles in the most ordinary events of life. Apuleius standing his trial for witchcraft and writing a book to show that it was possible and frequent, is a proof of the degradation which the highest Pagan intellect might reach. But there is not much to choose between the two faiths in their influence on physical science. Gregory of Tours gives a striking story of medical practice in Gaul. He had fled with some of his companions to avoid what was called the groin pestilence (lues inguinaria) to the neighbourhood of St. Julian's tomb. One of the party fell ill, and a soothsayer was called in who murmured charms, threw lots, tied ligatures to the neck, and so left his patient with a promise of recovery which was never realized. Indignant at this superstition, Gregory put some dust from the martyr's tomb into water, and gave it to the next of the party who sickened. The boy drank it, and at once the fever left him, and he recovered.*

While medical theory exhibited a decline rather than an advance in the powers of original thought expended upon it, the habits of society had changed signally for the worse. Without placing implicit faith in the Golden Age when Roman matrons were chaste, and when Roman patricians lived upon bread and a few vetches, we may readily believe that from the days of Sylla downwards luxury made fearful progress among the upper classes of the Republic and Empire. The extravagances of sensual passion that the pages of Catullus, Juvenal, Suetonius,

* Greg. Turon. de Mirac. S. Juliani, cap. 45.

Petronius Arbiter, and a host of others exhibit, may be barely alluded to. That a demoralized people cannot be healthy is self-evident, and vice under the Empire was carried on to the extinction of many patrician families. But it must be remembered that there was also a total want of healthy habits in the lives of the later Romans. A people who left the task of defending themselves to foreign mercenaries, naturally gave up the martial exercises which had trained the Curii and Camilli, and which strengthened Cæsar's constitution in spite of his profligacy. To wrestle, to race, and to plunge hot and dusty into the Tiber were requirements with which the courtiers of Caligula and Nero never troubled themselves. Such a man as Otho, who poulticed his face to preserve his complexion, and fitted up a room with golden pipes to scatter liquid scents, was the type of the young patrician. His courage was wasted on midnight brawls, in which he insulted women or maimed inoffensive passengers, or perhaps showed the last flashes of generous fire in voluntary death to avoid disgrace. Nero probably lengthened his life by his fondness for gymnastic exercises; but these, although not unknown in Rome, were scandalous, and cannot have been largely adopted. Hence the bath came more and more into use as a substitute for healthy exercise, and Galen devotes a whole book to its use. The prevalence of eye-diseases and the great use of collyria seem to indicate frames weakened by sexual indulgence and general indolence. But above all, apoplexy, gout, indigestion, and all the miseries that attend overloaded stomachs, were rife in the Empire. Seneca speaks of stomach-ache as one of the three evils whose intensity excuses suicide in a philosopher. Men so crippled with gout that they literally could not stir a finger, are mentioned by Juvenal and Galen. Some of this intemperance was probably attributable to the low tone of women's morality, and the general disgust for marriage that had come in. Lucullus, one of the first famous epicures, seems to have found relief from the infamy of his wives in sumptuous eating. Then, again, frivolous men were glad of an occupation that consumed several hours of the day and admitted of being studied and varied scientifically. The Catius of Horace, who regarded gastronomy as a grander science than the teaching of Socrates, and the Montanus of Juvenal, who could tell at a taste in what part of the sea oysters had been fattened, were pretty certainly not imaginary characters. It would have been well if Roman gourmandise had always been equally refined. It is sickening to read how Claudius would eat until he lay on his back exhausted but not satiated, and was then restored to fresh efforts by having his palate tickled with a feather that he might vomit. It is certain that opening medicines and clysters were used freely by the Roman upper classes, with the view of multiplying their meals.

Melancholy as this picture of society may seem, it scarcely changed for the better during several centuries. The vice of the Middle Ages was a little less refined and inventive, but it was equally abominable. In fact, the new generations of barbarian conquerors entered with untamed animal vigour upon the inheritance of every sin which the

Roman world had elaborated. The mere fact that nightmare was personified as a deadly goddess speaks volumes as to the temperance of our Germanic ancestors. "Si quis ex ebrietate eucharistiam vomuerit; Si mater cum filio suo parvulo fornicata fuerit; Si voluntarie semen in ecclesiâ fudit malâ cogitatione clericus," are some of the headings of Anglo-Saxon Church laws which witness to the perpetual contest between rampant animal passion and Christianity. We must add to these continued causes of disease those which arose from a lower civilization. Famines became more frequent and terrible as roads were broken up, and reservoirs of grain disused. Although the warm bath continued in use in England, and probably throughout Europe, long after the Roman times, the habits of the people were less cleanly, and skin diseases increased. Intramural interment was gradually revived, and the splendid drains which the conquerors had constructed in every town were left uncared for, and choked up, and replaced by cesspools and dunghills. Only those who study our early annals in their original sources are aware how largely disease enters into them.

While plagues became more frequent, and chronic illnesses more terrible, the appliances of medicine were diminished. The upper classes of the Roman world had been cut off by the sword of the Goth and Hun; philosophy had been proscribed by the Church; and Galen and Pliny were not easily understood by men who were not on the intellectual level of Galen and Pliny's contemporaries. Moreover, the drugs of the Greek and Latin pharmacopœia could not always be procured in Gaul or England, and perhaps lost some of their efficacy under a Northern sun. The more fanciful parts of the old theories were often those which most attracted half-educated men. Two treatises ascribed to Bede have a certain connexion with medicine. One discusses the relation of thunderstorms to events, and especially to health; for instance, thunder from the west is a presage of pestilence, and thunder in January an indication of sterility. The other treats of bleeding, a tolerably universal remedy in days when it was used to cure gout, sciatica, and anæmia. But the writer's chief object is to indicate the *Egyptian* days when all bleeding was dangerous. What principle determined the canons "luna secunda non est bona, luna tertia bona," we cannot now know; but both this and the theory about thunder clearly indicate a revival of the old view that the body of man sympathized with the universe. Of other remedies besides bleeding we know comparatively little with certainty. Epileptic patients seem to have been regarded as possessed with devils, and therefore rather subjects for the priest than for the physician, though mugwort kept in the hand was a prophylactic against attacks. Probably "fiend-sickness" was some similar disease, as it was to be treated with a drink of herbs in holy water and ale taken in a church bell, with psalm-singing and prayer. The remedy of melted butter against "pocks," perhaps varicellæ, must have been devised in sheer audacity of invention. But it is easy to understand the somewhat heroic treatment of a mustard cataplasm for headache, and a paste of strawberrystalks, pepper, and wine for weak eyes.* We dwell upon these facts partly from their interest in English history, partly because Dr. Meryon, and more unaccountably Sprengel, seem ignorant that any records of medicine exist for England during the Anglo-Saxon times. But if the facts we have instanced give a low idea of the art of healing during these centuries, they must be qualified by some considerations. There is evidence that Aristotle, Pliny, the 'Herbal' so-called of Apuleius, and one or two minor treatises were known in England. The preparation of philtres, poisons, and emmenagogues must almost certainly have been continued from Roman times, if we may judge by the efficacy of the effects that were thought to be produced in the two latter cases, where delusion is difficult. Again, there must have been some popular device for escaping the ordeals of boiling water and hot iron, as criminals sometimes passed them by dozens at a time. The operation for stone was practised in Flanders in the tenth century, and was probably, therefore, not unknown in England. But the silence of our histories about surgical operations at a time when wounds and accidents were frequent, is remarkable. Except the bare facts that amputations were practised, and the limbs bandaged, not cauterized, we know nothing certainly.

The revival of letters in the eleventh century produced important results on the study of medicine. Men began again to read and understand the great authors; Galen and Paulus Ægineta replaced the worthless compilations which had done duty for them; and an acquaintance with the best Arabian authors made alchemy popular. It would be easy to make out a case either in proof that there was real medical knowledge during the Middle Ages, or to show that all science was a farrago of baseless principles and quack practice. On the one hand, we might set the numerous copies of medical authors to be found in monastic libraries, the introduction or extended use of preparations of ammonia, antimony, and mercury, and the gradual rise of the medical profession in estimation which little facts indicate; the court physician in England being feed by suitors at law, and Chaucer's doctor of physic going clothed "in sanguin and in perse." On the other hand, it would be sufficient to quote a few of the meaningless remedies employed, and to show how little the greatest minds understood the secret of their failure or success. In judging of medical theory, however, some allowance must be made for the peculiar phraseology of the schools. Raumer cannot restrain his astonishment at a passage in Hugo de St. Victor, enumerating the six circumstances (*occasiones*) by which medicine acts upon health, temperature, change of quality or quantity in food, sleep, exercise, and mental quiet or emotion. Yet, although the classification is somewhat arbitrary, and the term "occasiones" a little pedantic, the scheme was not a bad one for practitioners to remember, and Sylvius reproduced it in the sixteenth century. Speaking generally, we may say that in the five hun-

* Wright's Biograph. Brit. Lit. Anglo-Saxon Period, p. 98. The remedy of a mustard cataplasm for headache is ascribed by Galen, who argues against it, to Archigenes.—Galen, ed. Kühn, vol. xii. pp. 565, 566.

dred years preceding the Reformation, two great steps were made in advance on the times immediately subsequent to the fall of the Roman Empire. In the first place, medicine was again regarded as the science of preserving health, and this was of some importance when the means of healing disease were so imperfect. And again, immense advances were made by the alchemists in the knowledge of minerals.

Perhaps more curious reading than John of Milan's metrical rules for health could hardly be found. Side by side with the calculation that there are three hundred and sixty-five veins, no doubt to match the days in the solar year, and with a warning not to eat goose on the 1st of May, are to be found a number of excellent practical hints for diet and daily life. Cleanliness is repeatedly enforced; the morbid influences of passion are pointed out, and Liebig's recommendation of bread with the bran unbolted is anticipated. The effects of different wines in making blood and aiding digestion are discriminated; and cheese, salt meat, venison, and hare are denounced as unsuitable for the dyspeptic. After all, these canons are little but a *memoria technica* which the unlearned might remember, but which the physician was expected to complete from Galen and Avicenna.

The writings of Roger Bacon are rather remarkable in the history of thought than of medicine. Since the publication of his minor works, no doubt can exist as to his real character. He was preeminently a mystic, desiring to see the kingdom of God realized upon earth. Believing that the triumph of the Gospel could only be secured when right reason should have prevailed over error, he did his part as a scholar in promoting inquiry. One great object of his works is to recommend a sounder study of philology, that the text of the Bible may be better understood. Another is to simplify the elements of knowledge, and codify all thought by reducing it to its first principles. In order to do this he distinguishes three kinds of knowledge. The first, which he calls mathematics, is a curious combination of Platonism and geometry. He seems to have confounded the generic term "species" with its common meaning "appearance," or "phenomenon," and to have imagined that a science like optics might be constructed which should deal with the classification of the natural world. The second kind of knowledge, "alkimia," answers to chemistry and parts of physiology, embracing the knowledge of elementary substances, and of those organic functions which depend on the chemical constitution. Experimental science, to which Bacon seems to assign the highest place, ought rather to be called constructive science, for he clearly regards it as demanding the highest powers of speculation, and only owing its name to its power of establishing all its premisses by experiment. Hence it differs, for instance, from astrology, which is purely speculative, and from such a science as botany, which is merely the knowledge of plants. The art of constructing burning-glasses is the one instance which Bacon gives of experimental science, and there can be little doubt, therefore, that he would have included under that head almost all the inventions which he discovered or anticipated—the camera lucida, carriages that would go without horses, and ships that would sail against the tide.

In the 'Opus Minus' he refers the task of lengthening life to experimental science. But his views on this subject will be better derived from his treatise on the 'Secrets of Nature and Art.' Starting from the doctrine that a general decay is going on, he observes that "every man would be able to remedy his own decay if he would subject his health from youth up to a complete regimen, embracing food, drink, sleep, waking, motion, rest, evacuation, constriction, climate, and habit of mind. For if any one should follow these rules from birth, he would live as long as the nature derived from his parents would permit, and would reach the extreme limit of that nature which has fallen from original righteousness, but he could not transcend this, for this regimen hath no remedy against the old corruption of our first parents. . . . . And at first, when the life of man began to shorten, the remedy would have been easy, but now it is difficult for a cure to be provided after six thousand years and more. Yet the wise, moved by the aforesaid considerations, know how to devise means not only against the defects of any special remedy, but against the corruption of our first parents, not so that man be brought back to the life of Adam or Artephius, inasmuch as the causes of decline are now so strong; but that life may be prolonged a hundred years or more beyond the common age of men now living, in so far as the passions of old age can be retarded and mitigated when they cannot be removed, to such an extent that life may be preserved healthily beyond all opinion of men, yet always within the extreme limit of our nature."* He then answers the argument from authority by observing that physicians then knew more than Galen and Aristotle, and that their sons would know more still. This confidence in the advance of science is remarkable in a singularly modest man, who admired and recommended the study of the ancients even in moral philosophy. It is clear, too, that in spite of his chemical studies, Bacon still regarded hygiene as the principal branch of medicine.

In quitting the Middle Ages we are unwilling to leave unnoticed one singular error into which Dr. Meryon has fallen. He says (p. 202), "So conspicuous was Raymond Lull as an adept, that he suffered martyrdom in the cause of his art." If by this phrase is meant alchemy, no statement can be more unfortunate. Lull died a martyr to dialectics and Christianity. Following the same track as Bradwardine, with even more stolid resolution, he had constructed a complete mechanism of thought. It is difficult to convey a notion of this "Ars Lulliana" to any who have not seen its scheme, but it may be compared to the tables for finding the fares between different stations, with this difference, that the object was to supply answers to every conceivable question. Given any subject of thought, the Lullist referred it to its appropriate category, and then glancing his eye down a column, along a line, or aslant the checks of the parallelogram, read off all the attributes, and was answered. It was doubtless the symmetry of this system that recommended it to its inventor and a host of disciples. Lull himself

* Bacon: Opera Inedita, pp. 374, 540, 541.

was convinced that he could convert all Islam, if he were allowed a fair hearing. At first he was quietly dismissed the country, and for some years occupied himself with the congenial pursuit of alchemy. Then his first faith returned; he visited Africa again, and was stoned.*

The changes in thought that accompanied the Renaissance and Reformation powerfully influenced medicine. Unfortunately for the profession, while the surgical movement was headed by Vesalius, the chemical fell into the hands of Paracelsus, a mystic and almost a man of genius, but a charlatan and a scamp. The natural consequence of this dualism ensued. At once favoured and persecuted, patronized by Philip II. and the friend of Titian, but forced to prowl by night about the cemeteries of Montmartre that he might pick up bones, and finally driven into exile on a frivolous charge of dissecting a patient alive, Vesalius succeeded nevertheless in raising anatomy to a level from which it has never sunk. The ingenious arguments of Sylvius, that the structure of the body had been modified or become monstrous since Galen's time, are now best known to us from their parody by Cornelius Scriblerus. "The moderns have perhaps lengthened the channel of the guts by gluttony, and diminished the liver by hard drinking. . . . I question not but plausible conjectures may be made even as to the time when the blood first began to circulate." Such arguments did not seriously retard progress. Even assuming that Galen had been right once, it was clear that a different description was required for an altered frame. The doctrine of the circulation of the blood, conjectured by Servetus, and perhaps, as Dr. Meryon surmises, by Fra Paolo, no doubt floating dimly before many men's minds as a possibility, was at last demonstrated by Harvey.

It is disheartening to contrast the slow advance of therapeutics. Yet, although we do not care to break a lance for the personal character of Paracelsus, we think injustice has been done to his ability. A mere quack could not have left such profound traces on his time, especially discredited as he was by his morals, and in antagonism with the whole medical profession. A man who ascribed the short lives of princes and nobles to their employment of physicians, had thrown away the scabbard. It was easy enough to attack him. Let alone his life, his writings are often like those of a madman. Among his works is a treatise on the curative properties of earthworms, in which he takes care to explain the fact that they have never yet been employed by the avarice of doctors, who like to dispense costly remedies. He professes to believe in the efficacy of incantations, and recommends that a wax likeness of yourself be kept to catch those directed against you, as charms are used in Naples to attract the evil eye. Even in his cures, the employment of heroic remedies constantly shattered the constitution they relieved for the time. But three great merits recommended Paracelsus to the public. At a time when the world had been system-ridden, he proclaimed revolt against all systems, ostentatiously imitated Luther's conduct, and associated him-

* Hegel: Gesch. der Philosophie, Band iii. ss. 175–177. Raumer: Gesch. der Hohenstaufen, Band vi. ss. 599–605.

self with the Protestant movement. He once says that he knew of no one who opposed Luther, except from bigotry or interest. He burned Galen and Avicenna, as Luther had burned the Papal Bull. In the next place, Paracelsus indicates the transition from hygiène to therapeutics. He regarded rules of health as of secondary importance. The great question was to purify anew the degenerate vital principle, and transubstantiate man into an immortal body, as the leprous or baser metals were to be transmuted by alchemy into gold. Wild as this conception was, it led men on the path of experiments which resulted in larger knowledge of the mineral medicines. Lastly, Paracelsus had a vein of mystical poetry in his composition. Belonging to the school which rejected the old mythology of saints, and unable as yet to understand the operation of natural laws, he conceived the order of the universe as a gradual hierarchy of organic beings, rising from the lowest upward to man, and connected through him with God. Man was the mediator of the elemental spirits, as Christ had mediated for man. In this way again he explained the old mythology. The giants and sirens of old time were monstrous offspring of the sylphs and undines respectively; and Venus herself was an undine.* It is curious to notice how the essential beauty of these thoughts has caused them to be embalmed in such widely different works as Pope's 'Rape of the Lock' and La Motte Fouqué's 'Undine.'

We cannot follow Dr. Meryon through the whole of his book, which, properly speaking, does not extend beyond the sixteenth century, though the Rosicrucians and Lavoisier are incidentally introduced. In an appendix a chronological table of special facts contributing to establish the science of medicine is given. It is of course a mixture of discoveries in materia medica, improvements in therapeutics, and advances in anatomy. It is imperfect. The vesicatory use of cantharides is mentioned, for instance, in the Hippocratic treatises, 'De Naturâ Muliebri,' which are probably anterior to the first century, to which Dr. Meryon refers its introduction; and the book 'De Superfœtatione,' which recommends its employment as a purgative, and Galen's allusion to it as a diuretic, at least show that Dr. Groenveldt was not "the first to employ cantharides internally as a remedy," as Dr. Meryon elsewhere states. (p. 458, note.)† Pancirollus has pointed out that the rhubarb spoken of by Galen was used as an astringent,‡ and the first mention of its purgative properties is ascribed by Pereira to Paulus Ægineta. The use of immediate auscultation in pleuro-pneumonic and phthisical cases is noticed in the Hippocratic treatise 'On Diseases;' and the 'Prognostics' mention the indications of health and disease derived from watching the pulsations of the heart.§ The Coacæ Prænotiones show that the pulsation of the temporal artery was attended to.|| Facts such as these at least deserve to be recorded, especially as Sprengel has awarded the merit of founding pulsatology

* Paracelsi · Opera, vol. i. p. 345; vol. ii. pp. 42, 393, &c.

† Hippocrates, ed. Kühn, vol. ii. pp. 553, 560; vol. i. p. 479. Galen, ed. Kühn, vol. xiii. p. 313.

‡ Pancirolli: Nova Reperta, ed. Salmuth, vol. ii. pp. 275–282.

§ Hippocrates, ed. Kühn, vol. ii. p. 318; vol. i. p. 94. || Ibid., vol. i. p. 250.

to Praxagoras. The art of inducing sleep by waving or strokings of the hand, such as are now practised in mesmerism, is alluded to by Plautus in the 'Amphitryon.' (Act I. sc. i. l. 157.)

"Quid si ego illum tractim tangam ut dormiat."

Hints of this sort are, perhaps, not very important, but a picture of ancient medical practice is imperfect without them, and merely as archæological notes, they ought to find a place in a new history of the science. Sprengel's work is wonderful in its way, but the additions to our general knowledge in the last sixty years have been enormous. Especially is this the case as regards the Middle Ages. Even in the darkest periods amputations were constantly performed, and attempts were even made to replace the medulla, where it had been much injured, by the marrow of animals. The details of Richard Cœur de Lion's death show that his surgeons, unskilful as they were, did not fear to cut deep in order to extract an arrow-head from the shoulder, and did not injure any important bloodvessel. Instances of this sort ought to be collected, that the actual state of medicine and surgery at any given time may be understood. Hitherto whole centuries have been condemned on the strength of one or two anecdotes. Bacon's story of the Jewish physician who scoffed at Christian doctors as only having the power to loose and bind, would, perhaps, apply to later centuries than Bacon's; and the story in Hoveden, how Archduke Leopold of Austria struck off his own leg with an axe for want of a surgeon to do it, loses much of its point when we read in the original that his body-surgeons, between grief and awe, were fearful of touching him,* and when we remember that Austria was then, as now, relatively an uncivilized country.

A separate work might well be written on the History of Epidemics, or indeed of disease generally. The Plague of Athens would be an admirable starting-point. Dr. Collier, in a thoughtful little book, has advocated the view that it was scarlatina maligna. Probably most men would be tempted rather to view it as a special form of zymotic disease, bearing much the same relation to typhus that yellow fever does, with due allowance for differences of climate and race. The description by Hippocrates of the epidemic in Thasus, and that by Galen of the pestilences in the second century, leave little to be desired. Even the fragmentary notices that occur in non-professional writers like Gregory of Tours might be pieced together into some intelligible shape; and the character of the yellow death in the sixth century would, perhaps, not defy scientific investigation. Hecker's 'Epidemics of the Middle Ages,' in spite of his fanciful theories and wild language, is a good specimen of what can be achieved by honest industry. Again, we are far from thinking that the question of syphilis has been satisfactorily treated. Without pressing a passage in Apuleius, in which one of his characters speaks of contracting a withering and pestilent disease from sexual intercourse with a woman of the worst character (scortum scorteum), there is a remarkable statement quoted by An-

* "Non enim ausus est aliquis vel potuit pro dolore in dominum suum manum imponere."—Hoveden, Savile. p. 426.

tony Wood from Gascoigne's 'Theological Dictionary,' which was written before the discovery of America: "Johannnem de Gaunt pudendis suis miserum in modum ulceribus exesis occubuisse."* Gascoigne regarded this death as the vengeance of God on a prince of loose habits who favoured Wycliffe. But the story is at best a link in the chain of evidence which seems to prove that the fearful scourge of the sixteenth century had been known to doctors long before it broke out among Columbus's companions, or in the French army, and existed, possibly, as an alternative form, with leprosy in men of dissolute lives, just as scrofula and phthisis often alternate. Anyhow it is clear that a wide field is open in this department alone to any careful writer.

We regret to have found nothing in Dr. Meryon's book that can entitle it to take the place of a manual. The interest of the subject and the portions that treat of discoveries in anatomy are redeeming features, but we are sorry to say that we see no reason why it should displace Sprengel, in spite of his length and deficiencies.

Review XI.

Statistical, Sanitary, and Medical Reports of the Army Medical Department for the Year 1859. ('Parliamentary Paper,' 1861, pp. 438.)

Few among us will take up this book without thinking of that good man and wise statesman to whom its publication is chiefly owing. This is not the place to review Lord Herbert's political life, or even to chronicle what we owe to one whom Providence enriched with almost everything man can desire, and who worthily used those beneficent gifts. When it was ordered that he who exalted even his great place, and made more illustrious even his far descent, by personal qualities of the highest order and by motives most pure and noble, should be removed from among us, we felt this could only be because the work was accomplished for which he was so richly endowed. On looking back at Lord Herbert's career, we fancy we can in fact perceive the complete circle of a finished work. At his first entrance on political life, it might have been supposed his career would have been in almost any channel than that in which it was actually destined to run. Had we predicted his course, we might have chosen diplomacy and foreign affairs, or the management of colonies, as best suited for that wide-grasping and polished mind, and for that just and equable disposition. But circumstances threw him early in another direction, and made him the great War Minister of his day. Perhaps no man in modern English history has so completely mastered his subject, and short of the technicalities of actual military movements, no man probably possessed a more thorough insight into the working of the great machine which we are obliged to keep ever ready while we wait for that far-distant time which the eye of the poet can alone see,

> "When the war flag will be furled
> In the parliament of man; the federation of the world."

* Wood's Athenæ Oxonienses, A. 1384.

Among the several divisions which compose our great military system, none occupied more of Lord Herbert's attention than the medical department. It was perhaps at first chiefly the kindness and tenderness of his disposition which led him to look closely into the arrangements for the relief of the soldier when sick or wounded. But he very soon recognised that the care of the soldier, while it is for all countries a duty, is for this nation a matter of vital moment. In the face of the great armaments of countries which equal us in intelligence and far surpass us in population, we can only maintain our station, and even our liberties, by keeping at the highest point the vigour and efficiency of our soldiers. Our small army must be a *corps d'élite*, and superiority both in mind and body must balance the immense excess of force and numbers which can be arrayed against us. Lord Herbert's constant object was to encourage in all branches of the army the development of mind, and to provide to the utmost for the care and strengthening of the body.

This work he in great measure accomplished, or at least has so prepared the way that little remains to do except to follow him with loyalty and vigour. He stamped a policy and initiated an action which must be followed, for it is firmly based both on truth and expediency. So far, then, we believe that this great administrator's work in life was accomplished, and that on that work is set the stamp of completeness and durability.

This is not the place or time to record all that has been done for the medical department of the army by Lord Herbert and by those who have been associated with him. We will only now say that he acted with equal justice, liberality, and wisdom. He clearly saw that the interests of the soldier and of the surgeons of the army are identical, and designed all his plans to be of equal benefit to both. The tone in which Lord Herbert always referred to the medical profession indicates the extent to which he understood its claims and action. He not only perceived its utility and appreciated its action on some of the deepest problems of this mysterious life, but he entered profoundly into its principles and even its technicalities. Lord Herbert was one of perhaps some twenty laymen who have mastered the principles of physiology and hygiène; and physicians who were brought into contact with him were often surprised to find him as well acquainted with many professional details as themselves. In this particular, indeed, no succeeding War Minister can hope to equal him; and herein his death will be an irreparable loss to the Army Medical Department.

It has been impossible for us to notice the work which heads this review without these few remarks, for the publication of an Annual Report by the Army Medical Department was part of Lord Herbert's scheme. It is, we believe, an essential point that such a report should be yearly issued, and we think that no one will read the present volume without admitting the expediency, and indeed the necessity, of Lord Herbert's proceeding.

The Report is intended to give to Parliament and to the nation a yearly statement of the health of the army in all parts of the world, and of the working of the medical department. It consists of three sections, each section being the report to the Director-General by the head of one of the three divisions into which his office is divided—viz., of the statistical, the sanitary, and the medical branch.

The Statistical Report naturally commences the work. It contains a complete statement of the sickness, mortality, and invaliding of the army, during the year 1859, in all parts of the world except India. It was found impossible to obtain the returns from India for the year 1859, but in future years they will be given, and then at one view we shall be able to see the exact condition of every regiment of our scattered army.

This Report is by Dr. Balfour, and is to be considered as a continuation of those able army reports which, initiated by Henry Marshall, and worked out by Sir Alexander Tulloch and Dr. Balfour, have had so immense an effect not only on the measures taken to improve the health of the soldier, but indirectly on the progress of the great subject of public health.

We remember reading somewhere that Napoleon the First, in his early, ardent days, when the scheme of universal conquest first dawned upon him, used to say that no book was so interesting to him as the dry details of figures which showed him the efficiency or sickness of his troops. That great calculator of forces was no despiser of statistics, but knew well the deep meaning which underlies the formal and uninviting calculation. But what Napoleon in his vaulting ambition wished most to know, is also that which peaceful and contented England has chief need to learn. A conscientious and provident care of the lives of those who serve the State has need of the same system as the lawless lust of conquest which regrets only an unproductive expenditure of force. It is indeed surprising to us that annual reports of this kind should not have been sooner ordered.

The earlier statistical reports were productive of great good, and led at once to vast improvements, but they were published at such distant intervals, that their exposures of evil or statements of benefit were long after date. It is the very basis of good administration to know quickly, in order to act quickly. We look upon this annual statistical report as the most important of the changes effected by Lord Herbert. It is, as it were, the keystone which locks all the improvements together. The various arrangements by which accurate sanitary supervision is carried on throughout the army seem to be extremely well devised, but without this annual digest there would be danger that they might become mere forms. But now it is impossible that anything can go very wrong, or any duty remain unperformed, without early detection.

With respect to the Report itself, we defer its analysis to a later period, as we hope to present to our readers in a future number a full account of the statistical inquiries into the health of the army and navy which have been lately published. But we may observe that the Report does the greatest credit to Dr. Balfour; it must have cost him great labour, and has evidently been most accurately prepared. There is a little difficulty in comparing it with the former army reports, as the classification has been altered, in accordance with the improved returns now used in the army. Although there cannot be a doubt that the new classification and nomenclature are much better than those formerly in use, we question whether the arrangement might not be still improved. The classes are now too large and heterogeneous, and one table sometimes requires a second before its meaning can be seen. Thus the heading, "Miasmatic Diseases," which includes not only malarious affections, but eruptive fevers, continued fevers, dysentery, cholera, tonsillitis, ophthalmia, and rheumatism, does not sufficiently express its meaning; Dr. Balfour has been obliged to distribute the diseases under the separate headings in a subsequent table. For statistical purposes the generalization has, we conceive, been carried too far, though Dr. Balfour has, by additional labour, to a great extent remedied this inconvenience.

We will only observe, before leaving this part of the Report to be more fully dealt with on a future occasion, that the health of the troops almost everywhere was very good in 1859. There is a most gratifying decrease in the mortality of all arms and at almost all stations. The causes of this are probably complex and dependent on better selection of men, better treatment of them, and more individual care on the part of the men themselves. Although it is too soon to express ourselves confidently, we have a strong impression that future years will tell a very different story of the health of the soldier from that recorded in the former reports and in the celebrated Blue-book of the Sanitary Commission.

The second section of the work contains the Sanitary Report, by Dr. Logan, Inspector-General of Hospitals. This is, as it were, the natural supplement to the Statistical Report; that shows the results, whilst this indicates the causes of the results, and what is doing to modify the results of future years. Dr. Logan takes a rapid survey of all the stations of the army, referring to their sanitary condition, and the measures taken to improve them. The following extract will show how this is done:

"INDIA.

"In the *Bengal* Presidency this year circumstances were very exceptional throughout most of its upper provinces, and efforts were urgently continued to provide barrack and hospital accommodation, of permanent or temporary character, to meet the requirements of a largely increased European force separated into cantonments after field service during the mutinies. The period required the active supervision and constant care of the medical officers for preservation of the health of the troops at many stations, especially when they, unavoidably, were under insufficient shelter from climate during the hot season. The monthly sanitary reports received from the officers evinced the great activity and close attention they must have devoted to all matters of drainage, cleanliness, and sanitary police of cantonments, camps, and their vicinities; ventilation of barrack dwellings, means of ablution for the soldiers, and the many other details connected with the health of the men and the duties of sanitary officers. The senior medical officer present at each station having been appointed to the special and responsible charge, as sanitary officer, his diary of transactions was systematically submitted to the inspector-general of hospitals.

"*Madras.*—At many of the stations occupied by the British regiments the barracks were in a transitional and improving state. A new cavalry barrack was in process of construction at Bangalore on the race course, and was reported as being placed on high ground, free from any obstructing circumstances as to ventilation. The principal medical officer, the deputy-inspector-general of hospitals, gave his opinion that the duty and employment, in cantonment or garrison, exercised no injurious effect on the health of the troops, many of whom, however, had been subjected to severe exposure during the late mutinies, and may have thereafter suffered more or less. It was thought the vice of intemperance was diminishing, and hope was entertained that, with increased sources of employment and recreation as afforded by gymnastics, racket courts, reading rooms, &c., the taste for the debasing vice would be found to wear out. Many such proposed means, however, were negatived for the present on financial grounds.

"The newly-established red serge frock was found to answer well as a substitute for the cloth tunic, the only objection to the former being its non-durability; an improved manufacture might remedy this particular defect. The shako was no longer in wear, and the wicker helmet with white cotton cover was much approved of and in general use.

"In the diet of the troops an improved quality of meat, and, at many stations, a more abundant supply of vegetables, would have been desirable, could they have been procured.

"In the furniture of the soldiers' bedding and bedsteads, generally defective throughout India, little improvement has taken place in this Presidency, although pressing representations appear to have been made on the subject through its highest military authority. Iron cots were recommended for more general use.

"*Bombay.*—The annual sanitary report of the principal medical officer had been received up to the 31st March, 1859, only. He states that due means were adopted, during the term of his report, by the Government and military authorities, to remove, if possible, any causes of disease among the troops. Careful surveillance appears to have been exercised in regard to the quality and quantity of the soldiers' dieting, to proper adaptation of their dress in relation to season, climate, period of day or night, in which they were employed, and to circumstances of field duties.

"In respect of the troops not employed in the field (including some regiments newly arrived in the country), their sanitary condition, from their not having been exposed, as others had been, consequent upon the mutinies, was generally satisfactory."

It is certainly very satisfactory to be assured that so much is now being done in India, and we hope the statistical returns of next year will begin to show the influence of all this care. But we must observe, that matters will soon relapse into the old dreary round of negligent routine, unless the Indian officials know that some action will be taken at home in consequence of the reports transmitted from foreign stations. Thus, in the above extract it will be observed that at many stations in the Madras Presidency "an improved quality of meat and a more abundant supply of vegetables would have been desirable, could they have been procured." But we will be bound to say, there is scarcely a station in India where good meat and plenty of vegetables may not be procured if the authorities are in earnest on the subject. And we conceive that such a paragraph as the one above should be followed at once by an order from the Home to the Madras Government to pay attention to this point. If a constant surveillance is kept up in this way, there can be no doubt that idleness and routinism will disappear. The fatal belief which has heretofore damped the energies of almost all officers, has been that whatever they represented, and however they represented it, nothing would be done; and that the man who was most active in suggesting improvements only got the character of a troublesome fellow who must somehow be put down. But now no representation can be a dead letter, it will always receive attention; if it is bad, it will be thrown aside; if good, it will be acted upon in the proper way, and at the proper season. At least, we infer that this is the course contemplated by the Government and by the able officer at the head of the medical department.

At p. 177, Dr. Logan gives an account of the suggestions made to the Minister of War by the Director-General at the outbreak of the China war, and of the orders sent to the sanitary officer of the force in China. We regret we have not space to quote these documents, but we hope that some official account will hereafter appear of the expedition to Pekin, as it was a most remarkable instance of the way in which the health of an army can be maintained under no very favourable circumstances by judicious management. If such a report should be issued, we shall then return to the documents contained in the work before us.

In addition to Dr. Logan's general summary, this part of the Report contains six able sanitary and topographical papers—on Deesa, Mount Aboo, Umballa, Ghazeepore, Columbia, and British Guiana, by Messrs. Hanbury, Ogilvy, Kendall, Fleming, Seddall, and Reade.

We presume that gradually a complete series of such reports will be published. The Indian journals—especially the old 'Madras Medical Journal,' the 'Bombay Transactions,' and the 'Bengal Indian Annals'—contain a number of valuable reports of this kind; but there will be a great advantage in giving them of later date, and in a single and accessible work.

Of these essays, that of Mr. Hanbury on Deesa, and of Mr. Ogilvy on the neighbouring hill station of Mount Aboo, are the most immediately interesting from their connexion with the great question of the location of European soldiers in India—a question which will receive its answer from the commission now engaged in considering the sanitary condition of the army in India. From the character and knowledge of many of the members of this commission, we believe there can be little doubt as to the result of their deliberations. For our own part, we conceive that one answer only can be given, and that is, that Europeans must occupy hill ranges, and that there is no military or political reason which is sufficiently important to set aside this cardinal measure. Mr. Ogilvy's report on Mount Aboo shows not only the advantage of the station, but also how much could be done, even in these chosen places, to improve the location, housing, and diet of the troops.

These topographical essays will evidently be a very valuable and important part of the new report.

The third and last section of the work is the Medical Report, by Deputy-Inspector-General, Dr. Mapleton, the head of the medical branch. In it are given all the details of the department, the admission into the service during the year of medical men, the deaths and retirements, the circulars issued from the office, and other particulars. The numerical strength of the department will probably surprise our readers; at the end of 1859 it was 1075, and this was exclusive of the great Indian service, which is even more numerous, and which is now incorporated with the royal army. The department must now number more than 2000 officers scattered over all parts of the globe.

The contents of this part of the Report are very various. Essays on sunstroke and dysentery; an account of the fittings of the hospital-ships *Mauritius* and *Melbourne*, employed in the China war; surgical reports by Dr. Savage, Mr. Lee, and from the General Hospital at Chatham; observations on the composition of the air in the wards of Fort Pitt; and two lectures by Professor Longmore, compose the bulk of this section of the work. A list of all the medical officers of the army; the questions asked at the examinations for entry and for promotion, and a full specification of the fittings of the hospital-ships just referred to, are appended.

It will be observed that a new system has been thus inaugurated—viz., the publication of essays on medical and surgical subjects by army surgeons. It has been long urged upon the Government that, both for the sake of the department and of science, it would be highly expedient to issue periodically a work similar in character and aim to the well-known 'Recueil de Mémoires de Médecine et de Chirurgie Militaires,' which has been published in France since 1766. The late Inspector-General, Henry Marshall, one of the most acute and philosophical of the army surgeons of our time, very strongly advocated this measure, and justly argued that it would be productive of the greatest benefit. We are truly glad to see the plan at last in action; medical officers will now have open to them a ready and easy mode of publication, and we do not doubt that there will soon be in English a collection of essays not inferior to those which have so long conferred such credit and honour on our brethren in France.

The essays in the present Report, although good and interesting, can hardly be taken as a sample of what will be forthcoming when the army surgeons become fully aware of the new system. The papers on sunstroke contain some good observations, and Dr. Massey's essay on dysentery will be read with interest, as affording additional evidence of the way in which the ipecacuanha treatment of dysentery is becoming the fashion in India. Large doses of ipecacuanha now appear to be employed, almost to the exclusion of other plans, and apparently with marvellous results.

Two papers on the determination of the carbonic acid in the air of the wards at Fort Pitt supply some valuable facts for the hygiènist;

and the careful surgical reports from the same hospital by Dr. J. R. Taylor and Mr. Matthew, will no doubt be studied by all surgeons.

In this volume is commemorated the opening of the Army Medical School at Chatham, in October, 1860. Two lectures delivered by Mr. Longmore, Professor of military surgery, on the occasion, and before alluded to, contain an admirable account of the objects for which the school was established by Lord Herbert, and of the relations of the medical department to the other departments of the army. They are extremely well written, and we beg to direct the particular attention of our readers to them. We had intended to have given an account from the first lecture of the new Chatham school, which we believe will prove to be of very great importance to the Army, but the space at our disposal is at present so limited that we must defer our notice to some more favourable opportunity.

In conclusion, we have only to say that if we have spoken favourably of this volume, we have not said one word more than it deserves. It is a good commencement of a most valuable system, and will, we trust, be worthily followed up. We congratulate the Director-General on the issue of his Report, and we are sure he may regard it with unfeigned satisfaction.

But as we close our short notice, the unbidden regret rises again to our mind, that he is gone whose sagacity suggested and whose influence ensured the publication of this work. May its successive numbers ever embody his spirit: that earnest desire to do good and to press on to higher and higher aims which so emphatically marked the noble and generous mind of our great War Minister.

PART SECOND.

Bibliographical Record.

Art. I.—1. *On Epilepsy and Epileptiform Seizures; their Causes, Pathology, and Treatment.* By E. H. Sieveking, M.D., Physician to St. Mary's Hospital., &c. Second Edition, revised and enlarged.—*London*, 1861. pp. 336.

2. *Epileptic and other Convulsive Affections of the Nervous System, their Pathology and Treatment.* By C. B. Radcliffe, M.D., Physician to the Westminster Hospital. Third Edition (incorporating the 'Gulstonian Lectures for 1860.')—*London*, 1861. pp. 312.

These two works, though like in subject and size, and therefore so far capable of being brought together within the compass of this single bibliographical notice, are, in their individual characters,—in style and matter, very unlike. Yet each is excellent in its way, and the one may be considered complementary to the other. The one is highly imaginative, permeated by a bold hypothesis, redolent in argument and in apt confirmatory illustrations. The other evinces a more cautious disposition, has a highly practical character, utilizes present knowledge, has little of the romance of hypothesis about it, and will be found a valuable and safe companion. Indeed, the treatises under notice are both of them well known to the profession; one has reached a second, and the other a third edition, and both in this progress have gained much in size and in fulness of detail and illustration.

Dr. Radcliffe advances his hypothesis of the dependence of spasm and convulsion on the deficiency of blood, of vital power, and of nerve force, with great intrepidity and with such an appeal to physiological laws and pathological deductions, that no lover of truth, even if it fail to convince him of the correctness of the principle asserted, can withhold his respect for the author's views.

"The additional thought and experience of the last three years (writes the author in his preface) have only served to strengthen my own convictions in the truth of this principle, and to show that my argument, as set forth previously, was a very imperfect statement of the grounds of these convictions." It is a principal object of this present edition to place the author's arguments at large before the public, and to this end he has incorporated the Gulstonian lectures he delivered in 1860, and has also "so re-written and re-cast the whole as to make a new book rather than a new edition."

The first one hundred and thirty pages are devoted to "physiological preliminaries respecting muscular motion," and this prelude complete, the pathological consideration of epilepsy and other convulsive affections of the nervous system is taken in hand, and the author's practical experience appealed to in elucidation of their treatment. Yet, although the second part of the work recommends itself by its own merits to the medical practitioner, and serves as a practical commentary to the first, yet it is the latter which will probably be perused with most interest on account of the originality of its matter. Indeed, if we accept its principles, much of our nervous pathology must be reversed.

This is not the place to enter into a critical examination of Dr. Radcliffe's views; indeed, this has been done in a previous volume of this Review, and we have now therefore only to commend them to the careful study of our readers, with all the increased light and extended argument the author has been enabled to add to this last promulgation of them. Although the leading hypothesis of this book has become particularly and practically associated with the name of Dr. Radcliffe, this physician has in a very fitting spirit taken pains to show that though he arrived at it as an independent thinker, it was anticipated by others in a greater or less extent. The first writer who shadows forth the idea is Dr. West, of Alford, Lincolnshire, in 1832; and Sir Charles Bell, and Professor Dugès of Montpellier, appear to have imperfectly conceived it a few years later. About the same time Matteucci broached some notions akin, and Professor Engel, of Vienna, almost contemporaneously with Dr. Radcliffe's own first publication, advanced some very similar opinions regarding muscular contraction. Subsequently Professor Stannius has expressed views closely analogous, and therefore the hypothesis pressed forward by Dr. Radcliffe is now presented to the public backed by some good names; a circumstance, indeed, sufficiently desirable, but valueless without the support of substantial arguments.

Dr. Sieveking contributes a few pages to the theory of epilepsy in his eighth chapter, but he otherwise keeps strictly to the intention stated in his preface, "to avoid the introduction of hypothetical conclusions and vague statements." In fact, so stringent has he been that he has omitted all reference to the opinions of Dr. Radcliffe; not, perhaps, from a desire to ignore them, but simply to avoid physiological discussions in a work it was especially wished to be practical. In thus restricting himself to the pathology of epilepsy, he has been able to produce a much more complete account of that terrible malady and of its treatment, than is furnished by Dr. Radcliffe in his more discursive treatise on convulsive affections generally.

Experience is quoted frequently, but as the author tells us, the summary of cases given in the former edition is omitted, and instead of it numerous illustrative cases, detailed more at length, are interspersed through the body of the work. Dr. Sieveking gives much attention to the subject of the medical treatment of epilepsy. He is distinctly opposed to antiphlogistic measures; of many drugs recommended he has had no experience, and of others with a wider reputation, such as turpentine, indigo, and cotyledon umbilicus, which he has tested, he can record nothing satisfactory as curative agents. Of the mineral tonics he places the salts of iron and zinc first; the preparations of silver have not answered his expectations, and of copper and its salts he can say nothing in their favour.

In the pages devoted to the consideration of the medical treatment of epilepsy, Dr. Radcliffe evinces a strong leaning towards the use of phosphorus, as well on physiological grounds as from experience, and he gives an excellent formula from the Prussian Pharmacopœia for a compound of oil and phosphorus; but as yet his experience with it has been too limited to do more than lead him strongly to hope that in it he has found a potent remedial agent for this terrible malady. In conclusion, we may cordially recommend each of the treatises, of which we have given so brief a notice, to the best attention of the reader.

ART. II.—*Clinical Surgery. The Surgical Diseases and Injuries of the Nose, Larynx, Thorax with its contents, and of the Organs of Circulation.* By THOMAS BRYANT, F.R.C.S., Assistant-Surgeon and Lecturer on Operative Surgery at Guy's Hospital. Part II.—*London*, 1860. pp. 75.

THIS pamphlet is a reprint from the last number of the 'Guy's Hospital Reports.' It contains a brief notice of the cases, illustrative of the subjects mentioned in the title-page, which have occurred in Guy's Hospital from October 1, 1853, to June 30, 1860, together with an abstract of some of the more interesting cases, followed by a general commentary. We cannot say that, in our opinion, Mr. Bryant has done well to transfer his paper from the modest form of a contribution to a periodical, into the more ambitious guise of an independent book. The surgical literature of the present day is so overlaid with ill-digested cases and incomplete observations, that an addition to the number of such productions is anything but desirable. In the instance before us it is quite clear that Mr. Bryant is somewhat hampered by the fact that the majority of the cases spoken of were not under his own care, and probably were under the care of his senior colleagues—hence a certain hesitation and reticence in commenting on the treatment, which goes far to deprive the commentary of its value. Again, although the title would lead the reader to expect a treatise on the subjects named, which if short should aim at something like completeness, nothing of the sort is attempted; only those affections are touched upon of which Mr. Bryant happens to have good illustrations: thus in surgical affections of the thorax nothing is said about the operation of paracentesis and its indications, nor about wounds of the large vessels and their treatment. Nor is Mr. Bryant always so "well up" in the matter on which he is discoursing as he might be. Thus (at p. 119) he quotes a case of "laceration of the lung, the result of an accident, but unassociated with any fracture of the ribs." He goes on to say, "As far as my experience goes, the case is a unique one, as I am unable to point out any work in which a similar instance is recorded." Now, besides numerous cases scattered through English and foreign journals, there is, in a work so well known as the 'Mémoires de la Société de Chirurgie de Paris,' a long treatise on this very injury, in which the mode of its production, its diagnosis, prognosis, and treatment are elaborately discussed.

We would not be understood to depreciate Mr. Bryant's work, or to represent it as destitute of merit. On the contrary, it will be easily imagined that a collection of cases by so accomplished an observer in so advantageous a position, must contain many of much interest. We would mention especially a very interesting, although somewhat obscure case of tumour connected apparently with the thyroïd body, in which Mr. Bryant performed tracheotomy on account of dyspnœa, and with the effect not only of saving the life of the patient, but also, as it would seem, of causing the gradual wasting of the tumour and the final cure of the disease. (p. 106.) The cases also of aneurysm, and of foreign bodies in the air-passages, are of much interest.

Our objection to this book is rather that it is not so complete or so original, and therefore is not likely to be so useful, as a book from a man of Mr. Bryant's calibre, deserved reputation, and high position ought to be. If he would take the advice which we were bold enough to insinuate to him in our notice of his last work, 'On the Joints,'* and would have the patience to wait until he could produce a complete and readable work on some important subject, based not only upon the experience of his own hospital but on all the available materials at his command, we are persuaded that he would do more for his own reputation, as well as for the advancement of practical surgery—which no one is better qualified to promote—than by fugitive pamphlets like the present.

There is one thing, however, which we feel bound again to notice in Mr. Bryant's writings: we mean the strange contempt which he evinces, not only for elegance of composition, but even at times for the plain rules of grammar. We could quote passages from the present production worthy of the best days of Mrs. Gamp, and others in which all the concords are set at defiance, and which should not have been allowed to pass through the hands of any respectable readers of the press without correction. It will not do to say that this is a minor matter. Such delinquencies irritate an educated reader, and predispose him against the good concealed beneath.

ART. III.—*Statistiske Undersögelser om Dœdeligheden i Drammen i Femaaret* 1856–1860, *og om de sanatœre Forhold sammesteds.* (*Meddeelt i Sundheds-commissionens Möde den* 20*de Februar*, 1861. Af Landphysicus Lic. Med. F. BLICK.)

Statistical Report of Mortality in the Town of Drammen during Five Years, viz. 1856–1860, *and its Sanitary Condition in that Period.* Communicated to the Health Commission at their Meeting, held 20th February, 1861. By F. BLICK, Rural Physician and Licentiate in Medicine. pp. 24.

THIS brochure is a reprint from the Norwegian 'Medical Magazine,'

* See vol. xxv. p. 81 of this Review.

and refers to a town of some commercial importance situated on the river Drammer, nearly twenty miles to the south-west of Christiania. The place now contains almost 10,000 inhabitants; consequently its sanitary statistics, regarding the period embraced in Dr. Blick's publication, becomes interesting to foreigners really desirous of knowing something about that locality, viewed in a medical aspect. Within the five years specified, the author states that the total deaths in the town above named amounted to 1113, being at an average of 222 annually, or only one death during the year in every 45 residents; which hence supplies strong evidence as to the salubrity of that Northern European district. It is likewise equally worth mentioning that 40 of the aggregate fatal cases occurred in old people who had passed their eightieth year, and farther, six ranged over ninety, all being females; but none were centenarians. Respecting the influence of seasons upon public health in this particular precinct, it appears, by appended official returns, that the second quarters of most years proved less lethal than the first and fourth, during which deaths exceeded the comparative number; thus indicating that cold weather here as elsewhere acted inimically on man's physical frame. Again, with regard to those diseases which then caused the greatest mortality, reference may be made to a detailed statement of the different casualties characterizing 1860, and contained in the publication brought under review. According to that document, 24 patients died of hooping-cough, 18 of broncho-pneumonia, 16 of croup, 12 of cerebritis, 12 of cramp or convulsions, and 9 by pneumonia; besides others by some acute maladies, but whose respective figures being small, need not be added, while 5 persons died from typhus. Lastly, as to affections of longer duration, it also appears noteworthy, that 63 deaths followed tubercular phthisis; but what is rather different from analogous results observed in England, the largest proportion of such victims comprehended males; and as the ratio exceeded one-fourth of the whole amount, consumption seems therefore very destructive among Drammenians. By organic diseases of the heart 9 fatal instances are reported, and also 9 by chronic bronchitis; while 8 individuals died of apoplexy, 7 of diarrhœa, and 4 of albuminuria, without comprising gout, paralysis, hepatitis, scrófula, or various complaints of an ordinary type, whereof one or two respective examples having only been recorded, they require no special notice. Several additional instructive data might be quoted advantageously from Dr. Blick's valuable pamphlet, which we have here briefly introduced to the acquaintance of English medical readers. However, as the present limited space at our disposal prevents an extended allusion to its contents, we can now merely remark, that the report may be fairly considered a good illustration of the mode Norwegian practitioners discuss sanitary or similar questions, in order to bring them before their national brethren, at the same time that they thus contribute to diffuse more extensively many useful facts which materially promote scientific practical knowledge generally throughout the profession.

ART. IV.—*Diagrams of the Nerves of the Human Body; exhibiting their Origin, Divisions, and Connexions, with their Distribution to the various Regions of the Cutaneous Surface, and to all the Muscles.* By W. H. FLOWER, F.R.C.S., Assistant-Surgeon to, and Demonstrator of Anatomy at, the Middlesex Hospital.—*London,* 1861.

THIS work, consisting of six folio diagrammatic plates, with a certain amount of letter-press, "aims at placing the knowledge of the anatomy of the nervous system in a form easily accessible, as well to students as to those whose avocations no longer afford time or opportunity for elaborate anatomical investigation." The nomenclature used in the sixth edition of 'Quain's Elements of Anatomy' is adopted, and the diagrams themselves are the results of fresh dissection on the part of the author, in addition to assistance derived from the works of Quain, Swan, Herschfeld, and Leveillé. We are quite sure that the work is calculated to be of very great assistance to the advanced student in the dissecting-room as well as in his own private study, clearly remembering as we do to have experienced in our own case the want of a lucid exposition of the various nerves of the body, so arranged and presented to the eye as readily to be remembered. Considerable facility in fixing the uses and destination of various nerves upon the mind, results from the names of the muscles to which the nerves are affixed being printed in red letters. On each plate we have columns pointing out, as regards the nerves described, their "origin from the surface of the encephalon," their "foramen of exit from the cranium," and their numerical designation according to the respective plans of Willis and Sömmering. We think that Mr. Flower would have done well to have added a column giving the "*real*" (deep-seated) encephalic origin, as well as the "*apparent*" one of each nerve, as that is physiologically of much more importance.

For practical common-use purposes we should be glad to see these diagrams stiffly mounted and varnished, so as to be hung up against the wall and freely handled; and we hope that we may hear of their being heartily recommended by our various anatomical demonstrators to their pupils.

ART. V.—*A Manual of Botany, including the Structure, Functions, Classification, Properties, and Uses of Plants.* By ROBERT BENTLEY, F.L.S., M.R.C.S.E., Professor of Botany in King's College, London; Professor of Botany and Materia Medica to the Pharmaceutical Society, &c. Illustrated by nearly 1200 Woodcuts. —*London,* pp. 811.

CLASS-BOOKS, text-books, outlines, manuals, rudiments, introductions, elements—the cry is still they come! In looking over the work whose title stands at the head of this notice, we are naturally induced to make comparisons with similar elementary works of Lindley, Henfrey, Asa Gray, Balfour, and others in common use in our schools, and the result is, we regret to say, not altogether favourable to the new manual.

Originality of matter is of course not to be expected in such a book, but on the other hand, much freshness and interest may be imparted to a work, the compilation of which is effected and regulated by the judgment and experience of the compiler, rather than by the mere mechanical transference of matter from the pages of another writer, himself, perhaps, a compiler. Slips of the pen and inadvertencies, such as the describing of the tuber of an orchis as a form of root, or the erroneous way in which the dehiscence of the fruit of *Digitalis* is described, may be remedied in a second edition, when we should be glad to find also some more extended attempt to systematize the multitude of details which usually prove so repulsive to the student, but which become much more attractive when their import is explained by the theories of vegetable construction and the great principle of unity in diversity.

We cannot but think it an important omission that Professor Bentley has not devoted a chapter to the method of describing plants, as this is a point on which so much stress is now laid by examiners, and rightly so, for nothing tends in a higher degree to promote the faculty of accurate observation than the process of describing fully, yet tersely and with accuracy, the common plants that a student is likely to meet with. In no other way can the cramming process, with its attendant cerebral dyspepsia, be more effectually treated. The student for whom the present work is intended is in no position to make the comparisons which have led to the foregoing remarks, in spite of which, we do not hesitate to say that there are many points in Professor Bentley's Manual which will ensure the popularity of the book among students; thus it is compact, in clear type, abundantly illustrated, arranged in a manner which Mr. Bentley's long experience as a teacher of medical and pharmaceutical students has taught him to consider the best adapted to the purpose in view. Last, not least, it is provided with a double index.

ART. VI.—*Transactions of the Obstetrical Society of London.* Vol. II. For the Year 1860. With a List of Officers, Fellows, &c. —pp. 368.

THE Obstetrical Society, yet in its infancy, is evidently enlarging its borders, and its fellows are to be congratulated on the merits of this their second volume of Transactions, which contains as many as thirty-four communications. Besides a number of instructive separate facts and "cases," we have several papers on general questions connected with obstetric inquiries, diseases of women and children, &c. Thus we find papers On the Influence of Belladonna on the Mammary Glands;* On

* We would direct the reader's attention to a highly interesting case related in the American Medical Monthly Journal, Sept. 1860, in which arrest of milk, which had been secreted continuously for three years, was effected by the outward application of fluid extract of belladonna and glycerine.—[ED.]

Midwifery in the East; On Concealed Accidental Hæmorrhage at the latter end of Pregnancy, &c.; On Aids to Impeded Parturition; and On the Exigencies Connected with Preternatural Labour. Among the most interesting are to be mentioned the lengthy paper by Dr. Granville on the "Power and Act of Propagation in Females of the Industrial Classes in the Metropolis;" an "Inquiry into the Correctness of the Doctrine of William Hunter in regard to Retroversion or Retroflexion of the Gravid Uterus," by Dr. Tyler Smith; two papers by Dr. Tilbury Fox on Phlegmasia Dolens; one by Dr. Barnes, showing the influence of shortening of one leg in inducing Distortion of the Pelvis; and one on the value of Anæsthetic Aid in Midwifery, by Dr. Charles Kidd.

As in the case of the Royal Medical and Chirurgical Society, papers are published on the recommendation of "referees." We think it is of vital importance to the prosperity and long life of the Obstetrical Society that the "size" of their volume of Transactions should be subordinated to the quality of the contents. In the long run, these yearly volumes will be much more worthy of the Society if the prevalent and pressing temptation to make a goodly appearance be rigidly withstood, and only really good matter admitted. We would also advise the council of the Society to see well to their illustrations. Nothing is more worthy of being done well than engravings or woodcuts, and the more such a book as the one under notice has of them the better. We regret that we cannot commend the quality of those in the present volume; excepting one by Mr. West, illustrating an interesting case related by Dr. Graily Hewitt, they are commonplace, coarse, and somewhat unsightly.

ART. VII.—*Third Annual Report of the General Board of Commissioners in Lunacy for Scotland.—Edinburgh*, 1861.

WE have had occasion to notice the two previous Reports of the Scotch Lunacy Commission in preceding numbers of this journal, and to award them a high meed of praise for the extent and value of the information they conveyed. The present Report may, by its excellence, rank with those of past years, although its contents may not strike the reader so much, because the state of the insane in Scotland has already been well described, and the supply of novel material has been somewhat exhausted by the comprehensive details furnished in the previous Reports. The picture of misery and neglect unfolded in the Report of the Special Commissioners, and in the first annual Report of the present Board, excited the public like a moving tale; its broader shadows are now toned down, but sufficient are left to exhibit how much still remains to be accomplished by the labours and influence of the Commissioners. We may in illustration call the reader's attention to the Report (Appendix F) on the condition of single patients; to the residence of many in turf-huts, neither wind nor water-tight, with earthen floors, or in huts of such a sort as to remind the reporter by their rudeness of the wigwams of the North-American Indians.

The total number of the insane in Scotland on the 1st of January, 1860, including the private patients as approximatively estimated, amounted to 8084. Of these 2858 were supported by private funds, and 5226 by parochial rates. On 1st of January, 1859, the corresponding numbers were 2898 and 4980. The number of private patients has therefore decreased, whilst that of paupers has increased. The augmentation in the number of the latter, moreover, is remarked not only in asylums, but also in workhouses; there was, however, a decrease of 30 in the course of the year, of lunatic paupers placed as single patients.

The statutory definition of lunacy is still a sore impediment to the Commissioners in their attempts to secure asylum supervision and care for patients. The sheriffs have great powers assigned them under the Act, and assume still greater. They constitute themselves judges of appeal to determine on cases of lunacy which shall or shall not be drafted to an asylum; and having their determinations left unrestricted by the definitions of the statute, and being so far left to their own judgment or fancy, it is not surprising that their decisions are often very unsatisfactory to the Commissioners, and those of different sheriffs opposite and conflicting. Thus it happens that sheriffs' orders are granted on certificates containing scarcely any evidence of insanity, and at other times refused in instances where such evidence is very clear.

It stands to reason therefore that this power of the sheriffs must be abated; for by its existence legislative provisions the best considered in other respects, may be rendered null and void, and the interests of the insane sacrificed. On what principle should a sheriff's opinion respecting the presence and form of mental disorder override and set at nought the convictions of one or two medical men who certify to insanity upon their own observation and personal examination of a patient? Yet this is done by sheriffs who have never even seen the patient concerned.

The Commissioners make this very proper remark (one well worthy consideration by those concerned in litigation respecting the value of certificates of lunacy and medical opinion):

"A medical man may, from the manner, appearance, and conduct of a patient, be thoroughly convinced of his insanity, and may nevertheless fail, by any statement of facts, to convey the same conviction to another person; and this difficulty will be greatest in the incipient stages of the malady. No careful observer can have failed to notice that almost all the murders and suicides committed by lunatics take place at the outset of the malady, before the symptoms are sufficiently developed to enable medical men to grant certificates in such a form as might satisfy the sheriffs."

The practical suggestion the Commissioners would make to meet the difficulty noticed is

"That the sheriff should, as a matter of course, grant his order for the admission of patients on the simple certificates of two qualified medical men that the patient is insane and a proper person to be detained in an asylum; and that it should be the duty of the Commissioners to examine the certificates, and to call for their amendment when defective; or to require the discharge of the patient when the evidence of insanity appeared imperfect and could not be substantiated."

This suggestion is assuredly simple and moderate enough; for we take it for granted that the Commissioners in Lunacy are the legal protectors of the interests of the insane, bound to satisfy themselves that every person confined as a lunatic is actually insane and in need of protection. And if this be so, the intervention of any other authority or opinion, except of a proper court of appeal, can only be mischievous.

ART. VIII.—*Stammering and Stuttering, their Nature and Treatment.* By JAMES HUNT, Ph.D., F.S.A., Honorary Secretary of the Ethnological Society of London, &c. 1861.—pp. 182.

THERE are some disorders and vicious habits of the body the treatment of which, by reason of their comparative rarity and from their requiring for their alleviation as much a moral as a physical method, is ordinarily removed from the sphere of the medical man. The habits of stammering and stuttering are among such; and thankful must a parent be who can turn to any quarter whence guidance in curing a tendency thereto may be expected. We have already* had occasion to notice Mr. Hunt's 'Manual of the Philosophy of Voice and Speech,' a work wherein the author shows himself so familiar with his subject that we are not surprised at his applying his knowledge of the natural conditions of speech and voice to the cure of deviations therefrom. The present work is in reality a fourth and enlarged edition of his 'Treatise on the Cure of Stammering,' and contains much interesting and discursive information gathered from all countries and all ages, as regards the history and statistics of the subject in question, and the numerous theories, modes of treatment, &c., which have prevailed respecting it.

As respects the method of cure adopted by Mr. Hunt (the same upon which the deserved reputation of his father rested), it is confessed that no precise account is given in the work. The method is by no means an esoteric one apparently, but, provided that no organic defect exist, simply consists in the patient's being made to understand what elements enter into the processes of vocalization and articulation, how the lips, the tongue, the jaw, and the organs of respiration are therein concerned, in finding out which of these organs is from habit wrongly worked, and teaching the sufferer how to regulate the faulty action. For the execution of this method no exact rules can be laid down; experience on the part of the attendant, following upon suitable physiological knowledge, must guide the treatment, along with aptitude on the part of the patient, and careful attention to mental tranquillity and self-control. The author has done well to relinquish the former title of the book.

* See No. xlix. of this Review.

ART. IX.—*The Causes and Treatment of Imperfect Digestion.* By ARTHUR LEARED, M.D., Physician to the Great Northern Hospital, &c. Second Edition.—*London*, 1861. pp. 218

THIS little work is a useful one, and though not containing much that is actually new, places what is already known before the reader in a pleasant, practical, and suggestive manner. We will allude to one statement made by the author (pp. 126 and 193), to which we cannot at all assent—viz., that sarcinæ ventriculi almost invariably indicate the presence of organic disease of the stomach. We are aware that this opinion prevails, but we are quite sure, from actual observation, that in a great number of healthy stomachs these vegetable organisms are to be found. This fact was made known four or five years ago to the Pathological Society of London. In the appendix, some interesting observations are given on the proximate cause of copious flatulence, on heartburn, and on the use of pepsin as a remedy.

ART. X.—*On Acute Ophthalmia as it occurred in the Left Wing of H.M.'s 37th Regiment, during* 1851–52, *whilst stationed at Colombo, in Ceylon.* By J. W. FLEMING, F.R.C.S., Surgeon to the Regiment.—*London*, 1861. 8vo, pp. 16.

THE symptoms noted by Mr. Fleming were vascular injection, commencing in the palpebral and extending over the ocular portion of the conjunctiva, slight at first, but afterwards so great as to obliterate all trace of vessels; a feeling as if of a foreign body in the eye; lachrymation; intolerance of light; a hot, burning sensation when the eyelids were separated; occasional headache, especially at night; with phlyctenæ, followed by one, two, or more small transparent ulcers round the edge of the cornea. The acuteness of the symptoms subsided generally in two or three days. Not one man, with the single exception of an old worn-out soldier who had previously lost an eye in the West Indies, left the hospital with a speck on the cornea, or impairment of vision. Nor did Mr. Fleming encounter a single instance of granular conjunctiva.

The causes of the disease assigned by the patients were, catching cold, exposure to the night-air, exposure to the sun, sitting in the wind or currents of air, dust blown into the eyes, and dust from breaking stones.

Mr. Fleming speaks of the cases which he met with as a "sudden outbreak of ophthalmia." The whole number of cases amounted to seventy-six, six of these were remaining over from December, 1851. The numbers occurring in 1852 were, in January, 3; February, 7; March, 6; April, 15; May, 6; June, 10; July, 6; August, 6; Sept., 5; October, 3; November, 2; December 1. The progress of the disease through the four companies forming the left wing, the numerical strength of which is not stated, scarcely vindicates the names of a "sudden outbreak," and the occurrence of 70 cases in twelve months is trifling, when compared with what has happened in other instances. Edmonston, for example, in the course of sixty-five days of February, March, and April, 1802, met with 133 cases in the 2nd Regiment of Argyleshire Fencibles; while Vetch, in the 2nd battalion of the 52nd Regiment, which numbered somewhat above seven hundred men, from August, 1805, till August, 1806, had 636 cases, including relapses.

In regard to the treatment, Mr. Fleming tells us that he made it his great object "to reduce speedily the *local* inflammation at as little expense as possible to the *general* strength. To attain this, lowering or antiphlogistic measures were employed with the greatest caution, and merely long enough to overcome the more acute symptoms." (p. 11.) One, two, or three leeches to the inner canthus, a solution of acetate of lead dropped on the eye night and morning, a collyrium of sulphate of zinc with opium, extract of belladonna round the orbit, warm-water fomentations assiduously used, and occasional laxatives, made up the general treatment. With the exception of the acetate of lead and the sulphate of zinc, which are or ought to be discarded, as we think, from ophthalmic practice, all this was judicious; as was the occasional exhibition of rhubarb, carbonate of soda, quinine, and muriated tincture of iron. We have not much faith in solutions of iodine or iodide of potassium applied locally. We believe that in such cases of ulcers of the cornea as Mr. Fleming met with, the plan he adopted of bringing the eye under the anodyne influence of belladonna is the safest and the best; and that saline solutions, especially those of lead and zinc, are apt to produce only additional irritation, and to lead to opaque cicatrices. The instillation of pure cod-liver oil, which he used in one case in which the ulcer assumed a scrofulous character, is comparatively innocent. "All evacuants," says Mr. Fleming, "emetics, indiscriminate use of mercurials, violent purgatives, general bleedings, blisters, setons, irritating ointments, and such like "heroics," were avoided." (p. 13.) We have no doubt that his success "may be fairly attributed to the careful consideration of the requirements of each individual case, and the absence of the common routine practice too often followed in the management of this class of diseases." (p. 9.)

On taking up this pamphlet, we were naturally led to suppose that under the name of "Acute Ophthalmia," we should meet with an account of an occurrence of the contagious inflammation of the conjunctiva, commonly called the Egyptian ophthalmia, and which has so often proved the scourge both of British and of Continental armies. Of this disease every one knows that a purulent discharge from the inflamed membrane is an early and a leading symptom. As no mention, however, is made by Mr. Fleming of this symptom, nor of the chemosis by which it is attended, and as he expressly states regarding one of the most remarkable, as it is the most intractable of the sequelæ of purulent ophthalmia—namely, granular lids—that it did not occur in any of his cases, we must conclude that what he met with in the left wing of the 37th Regiment at Colombo, during 1851–52, consisted merely in catarrhal, phlyctenular, and traumatic conjunctivitis. This conclusion is supported by the symptoms mentioned by Mr. Fleming, and to which we have already referred. Fifty or sixty years ago, military surgeons might have been forgiven for such a title as "An

Account of an Ophthalmia," or "On Acute Ophthalmia," but now that the nosology of the eye has been so carefully studied, and so many treatises and manuals published on eye-diseases, the distinctions observed in which are founded on anatomical diversities of texture and on pathological phenomena of easy recognition and indubitable importance, we should as little excuse the surgical writer who should reject the precision of modern ophthalmology, and go back to the vague nomenclature of bygone days, as we should some dabbler who, ignorant of the technicalities of botany, should attempt to describe to us a tree by saying it was of a middle size, its fruit nearly as big as an orange, containing seeds extremely bitter and poisonous, and that it grew in Ceylon; all which description he might avoid by using, if he knew it, the scientific name of the tree he meant, while he would convey to us a meaning which could not possibly be mistaken.

ART. XI. — *The Anatomist's Vade-Mecum: a System of Human Anatomy.* By ERASMUS WILSON, F.R.S. Eighth Edition.—*London,* 1861. pp. 732.

IN issuing this edition, the author takes occasion to thank friends for several suggestions and contributions. Additional illustrations, rendering this volume more complete than the preceding edition, will contribute to the extension of its popularity.

ART. XII.—*The Natural History Review:* a Quarterly Journal of Biological Science. Edited by Professors CARPENTER, BUSK, HUXLEY, and others. Nos. 1, 2, and 3.—*London.*

CONSIDERABLE attention has been attracted by the new series of this review, which has lately made its appearance under the above distinguished editorship. The general character of the critical and original papers contained in the numbers which have already appeared augurs well for its future position in scientific literature, and promises a fulfilment of the design of the editors, who announce it as intended to stand in the same relation to naturalists, and other persons interested in biological inquiries, as that which is occupied by the ordinary quarterly reviews in respect of men of letters and the general public. Amongst that portion of the contents which bears more directly on human anatomy and physiology, is a series of three papers from the pens of Professors Huxley and Rolleston, and Mr. J. Marshall, "On the Cerebral Characters of the higher Apes as compared with those of Man." The great interest which this subject has lately excited induces us to lay before our readers some of the conclusions arrived at by the authors. Professor Huxley's essay is avowedly an impugnment of one of the propositions enunciated by Professor Owen in his Classification of Mammalia founded on cerebral characters. It will be known to most that the latter eminent naturalist has placed man in a separate sub-class "archencephala," because his psychological powers, in association with his extraordinarily developed brain, entitle the group which he represents to equivalent rank with the other primary divisions of the mammalian class of vertebrates. The structural difference which Professor Owen considers the main point of distinction between the brains of the ape and man, is the presence in the latter of a third or posterior lobe, which not only overlaps but extends backwards beyond the cerebellum, and the internal anatomical structure of which is characterized by the posterior horn of the lateral ventricle and the hippocampus minor. Professor Huxley cites a number of authorities to prove that no one of the three characters thus specified is distinctive of, or peculiar to, the genus *Homo;* and he further asserts that the points of differentiation between the brains of man and the higher apes are of far less importance than those cerebral characters which separate *Pithecus* or *Troglodytes* from the lowest quadrumana—e.g., the *Lemuridæ.* As Mr. Huxley's paper is not so much one of original research as a compilation of previously existing evidence, having indicated its tenor, we proceed to notice the two more important essays by Professor Rolleston of Oxford, and Mr. Marshall of University College Hospital; that by the former observer giving an account of the examination of the brain of a young orang (*Pithecus Morio*); that by the latter of the dissection of the brain of a young chimpanzee (*Troglodytes niger*). On what may be called the external anatomy of the orang's brain, Dr. Rolleston's examination has brought to light the following facts: When the brain is *in situ*, before the removal of the dura mater, the superior surface of the cerebellum is entirely covered by the cerebral hemispheres, which moreover project a little beyond it posteriorly. In the instance examined, the cerebral mass did not project laterally beyond the boundaries of the cerebellum, as in man; but that this is at least not a constant point of difference the author proves by reference to the brain of another orang, of a chimpanzee, of several *Cercopitheci*, and an *Inuus* preserved in Christ Church Museum. His investigations, however, have led him to the belief that the cerebral hemispheres in the apes are, as a rule, more wall-sided—that they bulge less laterally than in man. A greater importance is due to the curtailment in the simious brain of the downward growth of the posterior lobes, a line drawn along the edge of the cerebral hemisphere where that hemisphere overlies the cerebellum being much less horizontal than in the human subject, the cerebrum appearing to a certain extent encroached upon by the cerebellum. Diminished upward extension of the posterior and anterior lobes, shown by the line bounding the superior edge of the hemispheres, describing a much more even curve than in man,—the diminished antero-posterior growth of the frontal lobe evidenced by the more vertical direction of the Sylvian fissure and its parallel convolutions,—the smaller antero-posterior extent of the corpus callosum, which permits the corpora quadrigemina to come into view on the removal of the brain from the skull, are also most important points of difference.

Professor Rolleston considers that the difference in the absolute weight of the orang's and human brain is a more sharply differentiating character than is the difference in the proportion which the weight of the brain bears to the weight of the entire animal. We may, however, observe that with regard to the question of the difference between the absolute and relative weights, satisfactory results cannot be obtained until adult specimens of the larger species, *Troglodytes gorilla* and *Pithecus satyrus*, have been obtained for examination, as not only does the weight of the body in these species more nearly accord with that in man, but according to a well-known physiological law, the brain is always relatively large in immature and small warm-blooded vertebrates. A convolution, bridging across the external perpendicular fissure dividing the parietal and occipital or middle and posterior lobes, which, according to M. Gratiolet, exists only in man, the orangs, and *Ateles*, is shown by Dr. Rolleston neither to be constant in man nor in the orangs, and sometimes to be found in the brain of the chimpanzee. A second connecting convolution, the *deuxième pli de passage* of Gratiolet, is present in man, never in the Catarhine Quadrumanes, but it is found in the American monkeys *Cebus* and *Ateles*. The lobule of the marginal convolution surmounting the upper end of the fissure of Sylvius is, according to Gratiolet, peculiar to man; but Dr. Rolleston observes that in human brains it varies much in development, and is frequently asymmetrical. Of all the convolutions, the superior frontal, both by their simplicity and symmetry, mark the comparative degradation of the simian structure. The internal anatomy of the brain closely corresponds in *Pithecus* and *Homo;* in the former the existence of the posterior cornu and hippocampus minor is asserted. A photograph appended to Mr. Marshall's very valuable paper on the brain of the young chimpanzee demonstrates beyond cavil the existence of these structures in *Troglodytes.* Amongst the more remarkable observations contained in the latter essay, is the demonstration of the far inferior development of the corpus callosum in the chimpanzee. This important commissural structure is more than twice as large, proportionally to the size of the brain, in man as compared with the ape. It will be remembered that the corpus callosum is entirely absent in the marsupial division of mammalia. The posterior overlapping of the cerebellum by the cerebrum in the chimpanzee is also proved by Mr. Marshall. With regard to the height of the human brain as contrasted with its upward extension in the chimpanzee, if we take the centre of the medulla oblongata as an arbitrary central point, the superiority of development, although everywhere great, is greatest in the vertical and parietal regions, next in the frontal, and least of all in the occipital. It would, then, appear that the main differences between the brain of man and the brute are those of degree, and that every essential portion of the human cerebral mass has its homologue in the brain of the higher apes. It must, we think, be allowed, nevertheless, that the association of psychical powers so extraordinary, with so great an increase in the development of the nervous centre, constitute an ascensive step of equal importance to the gradation between the Lissencephalous and Gyrencephalous sub-classes. We do not, therefore, regard Professor Owen's proposition as untenable, although he has apparently been led into error in choosing ground whereon to base it. But be this as it may, we fully accord with Dr. Rolleston in the belief that the highest and truest distinction between man and the brute is not one that can be weighed, measured, or anatomized, and with him we would draw from their admittedly close resemblance in material structure a powerful argument for the immateriality of that principle the agency and phenomena of which so immeasurably separate the one from the other.

Our space will not permit us to do more than mention a paper by Dr. Schaaffhausen, translated by Mr. Busk, on a remarkable human skull found beneath a deposit of mud four or five feet thick, in a cave in the southern wall of the gorge of the Neanderthal. The skull, which is in a very imperfect condition, exhibits a remarkably low forehead and an extraordinary projection of the frontal sinus. The importance of the peculiar conformation of this particular skull as a race-character is insisted on by Dr. Schaaffhausen, but, as it appears to us, on insufficient grounds. Its high antiquity, also, although most probable, is unsupported by any direct evidence.

ART. XIII.—*Obstetric Aphorisms, for the Use of Students commencing Midwifery Practice.* By JOSEPH G. SWAYNE, M.D., Physician-Accoucheur to, and Lecturer on Obstetric Medicine at, the Bristol General Hospital. Second Edition.—*London*, 1861. pp. 130.

THE reading of this useful and convenient book much reminds one of that constant occupant of the medical student's pocket in days of yore, good old Dr. Denman's Aphorisms (with its "discharge of the waters," and other quaint expressions); and we can honestly say that we know of no book of the kind which at the present day would prove a more valuable acquisition to the student when first launching upon the responsibilities of untried practice. The intention of the author, as may be inferred from the title, is not of course to present anything like a systematic guide to midwifery-knowledge: it is briefly to furnish such intelligible practical rules and common-sense trustworthy admonitions as may be required by the young obstetric practitioner on emergencies, or by the student when left for the first time to his own resources, or placed in any real or supposed difficulty. The wants experienced in such a position are well pointed out and cautiously met by the author, whose familiarity with his subject and whose duties as a teacher of midwifery render him thoroughly conversant therewith; and we are glad to see that another edition of these Aphorisms is demanded by the appreciation of the public. This second edition, besides other alterations, contains an account of Dr. Marshall Hall's "ready method" of performing artificial respiration, a process spoken of in the highest terms of approbation; as also a notice of the use of belladonna, whose claims have been lately brought forward as an anti-lactescent. Eleven woodcuts do serviceable work as illustrations, and suitable formulæ for prescriptions and recipes are given in various places.

PART THIRD.

Original Communications.

Art. I.

Some Observations on the Urinary and Alvine Excretions, as they appear within the Tropics. By Robert Lawson, Deputy Inspector-General of Hospitals.

A knowledge of the conditions of the excretions in health and disease is of great importance in the practice of medicine. These have been subjected to extensive and laborious investigation in temperate climates, but as yet little seems to have been done towards describing their peculiarities within the tropics. Having given some attention to the subject since 1857, in Jamaica, I have elicited some facts which do not seem to be generally known, but which appear highly interesting.

To determine the amount of the urinary secretion, with reference to the quantity of fluid drank, and under the varying conditions of temperature and moisture, a series of observations was made, extending over nearly four months. The individual under observation was forty-four years of age, five feet eleven inches in height, and weighing 186 pounds (85 kilogrammes), deducting the weight of clothes. He had been much in the tropics in the previous twelve years, and though subject to aguish feelings, was otherwise in good health. He rose at daylight, was variously occupied with business during the day, and usually studied till eleven or twelve at night. Moderately active, and temperate.

The urine, including that passed at stool, was collected to half-past eight A.M. daily. Its specific gravity was determined by a glass urinometer, the errors of which were known, and which kept its adjustment throughout; the solids were calculated by Christison's formula. The amount of uræmatine was ascertained by Vogel's scale, as given by Thudichum,* a constant thickness of four inches being employed, the containing-vessel being of clear white glass; and when the specimen presented the least haze, it was filtered before testing its colour. It was also examined daily for uroglaucine and urrhodine; traces of these were always present, but never to such an extent as to influence the colour derived from the uræmatine. During the observations small quantities of purpurine were occasionally seen, but impure nitric acid gave no indications of bilious impregnation.

* Pathology of the Urine, p. 135.

The following table (Table A) contains the results. The quantities of fluid are given in cubic centimetres, and the weight of the solids in grammes. The amount of uræmatine is expressed in units of Vogel's scale; the other columns need no explanation.

These results show that when the vapour in the atmosphere approaches saturation, much more of the fluid imbibed passes off by the kidneys than when the air is drier; the remainder of course is thrown off by the lungs, skin, and bowels, but unless there be diarrhœa, the last may be disregarded. The quantity of solids also is materially affected by the amount of hygrometric moisture in the air, increasing with the urine, but not proportionately.

This table (A) gives the averages for considerable periods only. Table B, which contains similar facts for four consecutive periods of three days each, shows that the changes are even more remarkable from day to day.

The period preceding the 3rd July was fine; that between the 3rd and 6th July was very rainy, with squally windy weather; that from the 6th to the 9th was much more settled; and that from the 9th to the 12th dry, warm, and agreeably fresh. The influence of these changes on the urinary secretion is very marked, and embraces the solids as well as the watery part of the secretion. Similar variations are constantly taking place with changes of weather, though within the limits of these observations there was no other instance in which the extremes of dampness and dryness came so close together.

While these observations were going on, it was found that moderately active exercise (other things remaining the same) diminished the quantity of urinary fluid and solids, while it increased most materially the action of the skin and lungs. As it is well established that exercise increases the waste of the tissues, the elimination by the skin and lungs must have increased sufficiently, not only to throw off a quantity of effete matter equal to that produced by the exertion, but, in addition, a portion of that which, without it, would have passed off by the kidneys.

It seems the general opinion that the unpleasant feelings many persons experience in damp weather, and their proneness to disease during it, are owing to the rapid removal of electricity from the surface. Without questioning such removal, the facts stated indicate that impeded function of the skin and lungs, while the vicarious action of the kidneys supplies their place but imperfectly, are intimately concerned in producing the malaise and proneness to disease observed at such times. The influence of exercise, too, is beneficial, not only by causing a greater waste of tissue, but also by ensuring a more perfect elimination of the effete materials, by exalting the excreting functions of the lungs and skin.

The large quantity of uræmatine habitually formed, deserves particular attention. Vogel estimates the uræmatine excreted in twenty-four hours, by a healthy adult, in Europe, at from three to six units of his scale, and on the average at 4·8.* These observations give a

* Thudichum's Pathology of the Urine, p. 187.

TABLE A.

Period—1859.	No. of Days.	Mean temperature.	Mean dew point.	Moisture required to saturate the air.	Average quantity of fluid taken.	Average quantity of urine passed.	Fluid exhaled by skin, lungs, &c.	Per-centage passed in urine.	Per-centage exhaled by skin and lungs.	Solids in urine.	Uræmatine, by Vogel's scale.
		Fahr.	Fahr.		c. c.	c. c.	c. c.			grammes.	
June 14th to 26th.	12	78°·5	71°·4	·204	2091	1120	971	54	46	58·5	14·73
July 1st to 31st (2 days omitted)	29	80°·2	70°·4	·269	2182	833	1349	38	62	52·1	13·02
Aug. 1st to 31st (2 days omitted)	29	80°·1	70°·8	·257	2350	863	1487	37	63	49·4	13·10
Sept. 1st to 24th	24	79°·4	72°·0	·211	2252	1010	1242	45	55	56·1	12·53

TABLE B.

To July 3rd, 8½ A.M.	3	78°·8	71°·4	·211	2100	800	1300	38	62	51·7	13·1
„ 6th „	3	75°·2	68°·5	·195	2100	1500	600	71	29	66·8	14·3
„ 9th „	3	78°·6	71°·2	·211	2100	809	1291	39	61	53·8	13·2
„ 12th „	3	81°·5	70°·3	·301	2100	682	1418	32	68	50·6	14·4

steady average of about two and a half times greater; though some of this might be attributed to individual peculiarity, yet the difference seems more than can be accounted for on that supposition, and may be fairly referred to the effect of climate. The alvine discharges present characters somewhat analogous, which will be noticed below.

If a few drops of urine be added carefully to hydrochloric acid in a test tube, a colour varying from deep blue, through different shades of purple, to red, may often be seen along the line of junction. This arises from uroxanthine, or rather its derivatives, uroglaucine and urrhodine, in varying proportions. Nearly every specimen of urine examined for the purpose, whether from sick or well, presented traces of these, though at times the quantity was both larger and more constant. Thus, up to the beginning of 1859, urrhodine particularly attracted attention from its frequency and quantity, but at that time it diminished rather suddenly, and continued scanty in most cases until the early months of the present year (1860). Soon after this diminution cases of fever presented an early and unusual degree of tenderness over the liver, and depth of bilious suffusion, characters they began to lose only in 1860. These colouring matters were not met with in very large quantity at any time, unless hepatic derangement were present, but when that led to deep bilious impregnation of the system, they were never found to be copious.

Hippuric acid is another constituent of the urine in Jamaica, which is found in large quantity in both the white and dark races, whether sick or well. I first detected it in August, 1848. I have since examined the urine for it in one hundred and ten instances, and found it in every one. These cases embraced Europeans, and creoles of all colours, and persons who were healthy as well as others who were labouring under different forms of disease. It was found as constantly in the urine of healthy European soldiers stationed at Newcastle, nearly four thousand feet above the sea, as of those who resided in the low ground. Few, if any, of these persons could have taken benzoic acid, unless it were contained in the yams, or other vegetables composing part of their food, but I am not aware that it has ever been detected in these.

The quantity of hippuric acid is usually so considerable, that if the urine be concentrated by evaporation over a water-bath to one-eighth of its bulk, then acidulated with hydrochloric acid, and set aside for twenty-four hours, a pretty copious deposit of crystals of hippuric and uric acids will be found on the bottom of the vessel. The crystals are usually in the form of elongated plates, or needles, derived from an oblique rhombic prism, though, by solution in water and slow evaporation, more distinctly prismatic forms are obtained. These crystals presented a brilliant appearance with polarized light; they were sparingly soluble in water, but readily so in alcohol and ether. When dissolved in lime-water, and the fluid allowed to evaporate, many of the varieties of crystals figured by Robin and Verdeil as hippurate of lime (Atlas,* planch xxi. fig. 2) were seen. With polarized light these

* Traité de Chimie Anatomique et Physiologique.

presented a very brilliant yellowish-white colour. These characters sufficiently indicate hippuric acid.

The easiest method of determining the presence of this acid in any specimen of urine (supposing it to be present in sufficient quantity) is to place a couple of drops on a slip of glass, and add a drop of hydrochloric acid; the mixture is then allowed to evaporate, when the crystals of hippuric acid may be distinguished, with more or less uric acid, according to circumstances, scattered through the specimen. Sometimes, however, the specimen remains fluid, though exposed to a dry atmosphere for days; if a drop of nitric acid be added to such, a rapid crystallization in general takes place, and the hippuric acid appears in another form.

If a drop of nitric acid be added in the first instance, instead of the hydrochloric, the crystallization presents a different, though very characteristic appearance. As the fluid becomes concentrated, crystals of hippuric acid form at various points round the extreme margin, from whence needle-like prolongations shoot out nearly parallel to the margin, and crystals of nitrate of urea are quickly deposited among them. These extend on either side of the point from which they started, until they meet with those from the neighbouring centres of crystallization, when they stop, leaving a well-defined separation between them; and as the fluid in the centre evaporates, the crystallization extends towards that, the greater part of the solids being deposited round the sides, and the centre being left nearly bare. Needle-like crystals of hippuric acid may be seen on the surface of the mass, with a close resemblance to a bundle of the leafless branches of broom, while the intermixture of that acid and nitrate of urea bears a considerable resemblance to plumes of ostrich feathers. A microscope is necessary for distinguishing these characters; they may be seen with a power of 60, and with one of 100 they are quite distinct. With polarized light the appearance is very brilliant. In every case in which urine was examined for hippuric acid, one or both of these methods was employed, and the quantity was always sufficient to give decided indications of its presence, and often was copious. In the majority of instances, crystals of uric acid were found also, but occasionally none were visible.

It was observed in the course of the examinations from which the above facts were obtained, that portions of urine acidulated with hydrochloric acid remained fluid for days, even though the temperature was high and the air dry. This seemed owing to the medicine taken in some instances, but at other times every specimen examined, whether from a healthy or sick individual, and for many weeks in succession, presented the same character. This peculiarity was obviously attributable to the presence of some of the more deliquescent salts, and from its being so general, was in all probability connected with some of those influences called epidemic, of which as yet we know so little. Its mention here may induce others who have the opportunity, to prosecute the investigation.

The colour of the alvine discharges is usually considered to be derived from bile, and though doubts have been from time to time thrown out as to the accuracy of this opinion, it still maintains its ascendancy. Holland, in his 'Medical Notes and Reflections' (second edition), suggested that the colour of the discharges from the bowels was much more owing to the secretion of the colon than to the bile. Osborne* supposes that the mucous membrane of the intestine can secrete dark bile, though the bile-ducts be obstructed. Frerichs states in his sixth observation, that

"The colour of the stools was somewhat brownish, not owing, however, to the admixture of bile-pigment, but, as shown by the microscope, to numerous epithelial cells containing pigment derived from the mucous membrane and glands of the intestine."†

The following observation is more to the purpose. A gentleman in Kingston, Jamaica, had an attack of diarrhœa at five A.M., on the morning of the 10th February, 1860. This was followed by much irritation and soreness along the transverse and descending colon, which continued during the day. There was another loose motion about eight A.M., of a deep dark brown colour, like very thin fæculent matter, a portion of which was obtained without admixture with urine. Under the microscope this was found to contain a large quantity of glandular epithelium, either separate or contained in distinct tube-casts; there were a few pieces of the inner husk of wheat, and some of the pulp of fresh figs, eaten the previous day. The epithelial cells, whether separate or in casts, were almost all deeply tinged with a brown colour; no blood globule was seen among them. The colour of the evacuation was altogether owing to the epithelium it contained. These facts are conclusive as to the possibility of colouring matter being obtained from the intestine, and they indicate pretty clearly the extent concerned.

If a portion of an ordinary formed fæculent evacuation be examined, it will be found to contain portions of partly digested articles of food, such as muscular fibre, ligamentous substances, starch granules, with quantities of other matters which seem little changed, such as vegetable cells, inner husk of wheat, &c.; besides these, however, a large quantity of granular matter, of glandular epithelium more or less granular, and even of portions of tube-casts filled with a similar epithelium, are to be seen throughout the specimen. The tube-casts are generally in short fragments, and many portions may be detected, some sideways, others presenting an end only. This epithelium and granular matter is all impregnated with a brown pigment, and is usually the only colouring matter to be seen, and nitric acid does not produce in it the characteristic reaction of bile.

The colour of the alvine evacuations of the white races who have resided for some years in a warm climate is habitually much darker than in colder countries, and this though the food contain no colouring matter to affect them. On first proceeding to the tropics the change comes on gradually, and perhaps may not be fairly developed for several

* Dublin Journal, Feb. 1853.

† Frerichs on Diseases of the Liver (Sydenham Society's edition), vol. i. p. 138.

years, nor until the individual may have undergone one or more attacks of fever; and, on his return to a cold climate a retrograde change occurs, the colour becoming perceptibly lighter in the course of a few months, though it is probable the change may continue for much longer.

It is a matter of common observation, that those portions of the surface which are exposed to the light become in a few years very swarthy in the white races in warm climates; the same is observed to a minor degree in warm weather in temperate climates. In the former, however, residents of some standing often present a change of colour in portions of the surface not exposed to light, if there have been any irritation applied to them; thus, cicatrices of ulcers, parts which have been blistered, or even slight abrasions, become covered with a cuticle of a more or less deep brown, quite distinguishable from that of the neighbouring skin. If a portion of such cuticle be examined microscopically, the colour is found to be owing to numerous dark or blackish-brown pigment granules in the epithelial cells, in nowise distinguishable, save in their being less numerous, from those which give the colour to the skin of the dark races. This tendency to the deposition of pigment in the surface epithelium continues, in persons who have resided long in warm climates, often for years after their return to colder countries.

It seems now generally admitted that uræmatine and the other dark pigments are derived from the hæmatine of the blood. The facts above detailed show that much more of this is thrown out of the system in a given time in a warm climate than in a colder one, but that the capability of throwing it out is only acquired after a somewhat prolonged residence; and, after a return to the colder climate, the habit is only got rid of gradually, and perhaps never completely.

Acclimatization consists of such a change in the actions going on in the system as enables the individual to live in any given climate, with less liability to disease, or less chance of being injuriously affected by it when contracted, than on his first immigrating to that climate. A profuse excretion of hæmatine is one of these actions, and a very striking, though by no means the only one; yet, while acclimated persons rid the system of this by one or more of the methods described, the more important diseases of those unacclimated generally present it in some form of morbid deposition or discharge.

To commence with the black races, they present an extensive outlet for the hæmatine in the pigment-loaded epithelium of the skin; and, so far as my observation goes, when in health, neither their urine nor alvine discharges are nearly so dark as in acclimated Europeans. In sickness, however, these often become much darker, and if long-continued, or if there be much purulent or mucus discharge, the colour of the surface becomes much lighter. On the functions assuming their natural character, or the morbid discharge ceasing, the original depth of colour of the surface soon reappears, from the pigment resuming its natural outlet.

On the inhabitant of a temperate climate proceeding to the tropics, neither the urine nor the alvine discharges afford a sufficient outlet at first for the altered hæmatine, and there is usually much more irritation of the skin and feeling of oppression from the sense of heat, even though the individual may live moderately, than he experiences a few years after, when the evacuations are more highly charged with pigment. The more serious diseases of the recent immigrant, too, such as yellow fever and dysentery, are characterized by excessive elimination of the colouring matter of the blood; while in remittent fever the hæmatine is often deposited largely in the spleen and liver, and sometimes in the brain, or elsewhere.

In females, the uterus is an organ for the elimination of a material rich in hæmatine. In warm climates they are particularly subject to excessive menstrual discharges and affections of the uterus, of which no satisfactory explanation has been offered hitherto. When yellow fever is prevalent, too, the mortality among women is generally much less than among men; and when the female is affected with tropical fever, every practitioner must have observed that when the menstrual discharge came on, the fever almost always disappeared. The facts detailed in this paper offer so far a reasonable explanation of these peculiarities, though confessedly an incomplete one.

I have detailed above some of the results of my tropical experience, and have attempted to deduce from them conclusions bearing on the question of acclimatization, with what success others must determine. Though so much prominence be given to the removal from the system of the products of hæmatine, it must be distinctly understood that that is to be regarded only as one of a series of actions, and in all probability as one of the least important of that series; but being within our cognizance, it was advisable to develope its relations. Should these remarks induce others to follow up the subject of acclimatization, or to use their opportunities to determine the effect of epidemic influences on the system, my object in laying them before the profession will be attained.

Art II.

Unusual Cases of Poisoning. By Francis Ogston, M.D., Aberdeen. Part I.

Case I.—*Fatal poisoning by lucifer-matches : suicide.*—On the morning of the 9th of March, 1858, Margaret McB——, a mill-worker in Aberdeen, swallowed, with a suicidal intention, an unascertained quantity of the matter scraped off from the points of lucifer-matches in her apartment. When seen by Dr. Nicol, of the Aberdeen Royal Infirmary, on the evening of the same day, she was suffering from nausea and retchings of watery fluid, and complained of thirst and pain in the epigastrium. There was marked pallor of the face, her extremities were cold, and her pulse was weak and almost imperceptible. Next day, in addition to the above symptoms, she had at times cramps of the legs. The sickness, pain, and vomiting continued with little intermission, without any movement of the bowels, till the 13th, when stupor came on, and after a succession of piercing screams, and strong clenching of the hands, she sank in the course of the afternoon.

At the inspection of the body on the 15th of March, the following appearances were noted by us: Its non-dependent parts generally, including the lips, pale; its dependent parts and the nails, livid; the joints rigid, the countenance placid, the eyes sunk, the pupils dilated. There were greenish patches on the sides of the belly; the scalp was natural; the fornix and thalami nervorum opticorum were somewhat softened, as was the cerebellum generally. The grey matter of the middle lobe of the brain was redder than usual; spine, natural. The mucous coat of the larynx and upper part of the trachea was dyed of a uniform deep red, without perceptible vascular injection. The air-tube, on being laid open, exhaled a distinct phosphoric odour, and when removed and carried into a dark corner of the room gave out a white light. A large fibrinous clot occupied the right cavities of the heart and the bloodvessels connected with it; lungs much congested with dark fluid blood; rounded patches of effused blood, varying in size from the breadth of small shot to that of a horse-bean, in the loose tissue outside the œsophagus, and on the upper surface of the diaphragm. On opening the stomach, the phosphoric odour was again perceived, as in the trachea; liver, spleen, and kidneys congested with dark fluid blood, the blood here and elsewhere being almost black.

CASE II.—*Poisoning by "phosphor paste:" suicide: fatal.*—Janet R—— or M——, an Aberdeen pauper, aged fifty-three, living alone, was found in bed by a neighbour, on the 22nd of December, 1858, in an almost insensible state, with a matter of an indigo-blue colour oozing out at one corner of her mouth, which this person wiped away with a bit of white cotton rag. Dr. Jackson was then called in, who found the woman unable to give any account of herself, and death followed in the course of a few hours, without any marked symptoms presenting themselves.

At the post-mortem inspection (thirty-six hours after death), for permission to assist at which I was indebted to Dr. Jackson, we found the countenance placid, the pupils natural, the lips blue, the joints rigid, the front of the body pale, and its dependent parts livid. The scalp was bloody; the encephalon and top of the spinal column natural; the pharynx and upper part of the gullet were of an uniform deep red; the larynx, trachea, and the left bronchial tubes were filled with frothy mucus; the blood, in a clotted state, was accumulated in greater quantity in the right than in the left cavities of the heart; the interior of the stomach uniformly reddened, the redness deepest towards the pylorus; the duodenum intensely reddened, with minute injection of its smaller arteries; the other viscera presenting nothing unusual. On laying open the cavities of the chest and abdomen, as well as the stomach, a distinct phosphoric odour was at once perceived. The stomach contained about four fluid ounces of a thick brownish pulpy liquid, and small patches of bluish, soft semi-solid matter adhered to different parts of its walls. When introduced into a bottle, and taken to a dark corner of the room, the surface of the fluid from the stomach was perceived to be distinctly luminous. The blue solid taken from its wall corresponded on examination with that on the cotton rag with which the mouth had been wiped before the woman's death, and with the remaining contents of a jar, almost empty, of the substance sold as "phosphor paste," found in her apartment when first entered before her death.

CASE III.—*Poisoning with bichromate of potass: suicide: fatal.*—Mary Ann N——, an unmarried woman, aged twenty-three, in charge of a wool-mill at G——, in the county of Aberdeen, about six o'clock on the morning of the 21st of November, 1859, called up the servants of the establishment to their work, at which time she appeared to them in her usual health and spirits. About half an hour after, however, she complained of illness, and stated that she had swallowed "a good piece" of a stuff used at the works (the bichromate of potass). Some train oil was then procured and given to her, the ingestion of which was speedily followed by severe retchings and vomiting. To this succeeded abdominal pains and coldness of the extremities, the vomiting continuing, with frequent dejections of dark coffee-coloured stools, till a little before her death at three P.M. of the same day.

At the post-mortem inspection, undertaken on the 24th of November, along with Dr. Reid, of Aberdeen, and Mr. Davidson, surgeon, of Old Rayne, the following particulars were noticed: The countenance calm, the pupils natural, the joints rigid, the forepart of the body generally pale, the nails blue, the dependent parts of the trunk and limbs reddish, abdominal wall depressed. The scalp was bloodless; the cerebral veins and sinuses deeply loaded with dark fluid blood; blood, in greater quantity than usual, in the interior of the brain; nothing unusual in the mouth, throat, or air-passages; right cavities of the heart gorged with dark fluid blood, with a little blood of the same character in its left auricle and ventricle; lungs a good deal congested; slight vascular injection of the lining membrane of the œsophagus near its lower end; sixteen fluid ounces of a thick reddish-brown liquid in the stomach, the inner coat of which was wanting over its cardiac half, portions of its middle coat being also absent here and there at the same part of the organ; the lining of the remaining half of the stomach, here almost entire, dotted with irregular patches of a cherry-red or reddish-brown colour; the whole of this viscus, viewed from within, presented a uniform red colour, and the same uniform redness existed along the entire inner surface of the smaller intestine, large patches of which were also seen to be minutely injected; a bloody fluid, in some quantity, occupied several of the bends of the duodenum and jejunum; kidneys a good deal congested; a large clot of blood in the left ovary; a cavity, lined with a firm white membrane, in the right ovary; lining of the uterus of a uniform deep red.*

* We were informed in this case that, though known to have had suppression of the menses for fourteen or fifteen months, this young woman had her shift stained with menstrual blood at the time of her death, and with this the state of the genitals at the inspec-

The fluid from the stomach of N——, on a subsequent chemical examination by two of us, was found to be saturated with bichromate of potass, while crystals of the same salt had settled to the bottom of the vessel into which it had been transferred; and that of this poison, taken altogether, it was estimated that the stomach after death must have contained above half a drachm. Bichromate of potass was likewise detected by us in some vomited matters which had been preserved, in which we were rather surprised at detecting the portion of the mucous coat found wanting in the stomach, and this almost in an entire state.

CASE IV.—*Accidental poisoning by corrosive sublimate: fatal.*—On the evening of the 24th January, 1860, William H., a small farmer, at B. of L., in the county of Aberdeen, carried home with him what he supposed to be a "Physic powder" (compound powder of jalap), but for which a grocer and druggist in his neighbourhood had substituted by mistake what was afterwards discovered to be two drachms of corrosive sublimate. This H. soon after swallowed, and had no sooner done so, than he called out that "he was burned," and threw himself on a sofa in great pain. Warm water was drunk by him freely, and two hours after, the whites of two eggs were given him. The diluents brought on copious vomiting. At noon on the following day, when seen by a medical gentleman, he complained of burning pain in his stomach and bowels, and seemed much depressed. His death, however, did not take place till the 28th of January, at 3 o'clock P.M. For the last twenty-four hours the urine had been wholly suppressed.

The deceased, it was learned, had suffered occasionally from dyspeptic symptoms, but otherwise was known to be of active habits, and in the enjoyment of tolerably good health.

At the inspection of the body on the 30th of January, in company with Dr. Manson of Banff, the features were observed to be calm, the lips and forepart of the body generally pale, the sides of the belly greenish, the dependent parts of the trunk and limbs reddish, the pupils natural, the joints rigid. The scalp was bloodless; the cerebral mass generally firmer and paler than usual; there was a bluish line, inclining to purple, on the gums close to the necks of several of the teeth; the tongue, palate, and isthmus of the fauces, pale and glazed; the larynx, trachea, and bronchi pretty uniformly reddened, the redness increasing in intensity as the latter subdivided within the tissue of the lungs. Frothy mucus, with minute air-bubbles, in the lower end of the trachea. Both lungs œdematous, the fluid in the right lung bloody; right cavities of the heart filled with dark fluid blood. Left cavities of the heart almost empty; portions of the outer covering of the œsophagus injected, mucous lining of the same tube at its lower end

tion corresponded. In connexion with this it was observed by us that though she presented none of the indications of a recent or earlier pregnancy or delivery, the brown line so generally met with in pregnant, and so rarely in virgin females, was very distinctly marked along the centre of the belly, from the umbilicus to the pubis.

wanting. Deficiency of the mucous coat of the stomach at its great "cul de sac." The wall of the stomach at the smaller "cul de sac" considerably thickened from a deposit of altered blood of considerable tenacity interposed betwixt its inner and middle coats. The first fifteen inches of the smaller intestine considerably reddened and injected; its inner coat softened; and portions of it absent. The cæcum thickened to the same extent as the smaller "cul de sac" of the stomach; but without the same distinct or coloured deposit. Interior of the rectum reddened, with rounded patches of effused blood of the size of a hemp seed under its lining membrane.

A few days subsequently, with the able co-operation of Mr. Brazier, of the Aberdeen University, by the employment of gold and zinc wires, mercurial deposits were obtained from portions of this person's liver, stomach, and smaller and larger intestines; as well as, though less distinctly, from a couple of ounces of his blood; deposits which, after sublimation, took the characteristic red when exposed to the vapour of iodine.

CASE V.—*Poisoning by strychnine: suicide: fatal.*—At 7 o'clock on the evening of the 15th of March, 1860, James H——, aged thirty-two, an innkeeper at H——, was seen in bed in what was described as a fit, which after subsiding, speedily returned, during which time he was said to have "struggled a good deal." About an hour after the first seizure, he was attended by Drs. Wilson and Grant, of Huntly, who succeeded in obtaining the action of an emetic, and who, from the condition they found him in, considered that he was labouring under symptoms of poisoning by strychnine. The fits after this continued to recur with increasing severity till half-past 10 P.M., when he died. The first fit occurred five minutes after taking a powder in his possession, on which he exclaimed that he was "right strange," and on its recurrence that "he was ill again."

At the inspection forty-eight hours after death, the undernoted particulars were observed by us: Face reddish; nails, front of the neck, shoulders, and the dependent parts of the head and trunk, livid. Features placid. Pupils natural. Joints stiff. Elbows half-bent; on their forcible extension, continuing relaxed for some time, but afterwards contracting and stiffening. Instep more arched than usual. Fingers and thumbs slightly flexed. Scalp bloody. From two to three drachms of blood, partly fluid, partly clotted, in the left cerebral ventricle. Sinuses at the base of the brain unusually loaded with dark fluid blood. Fluid blood in the mouth and throat, and in considerable quantity in the trachea. About a third of the upper lobe of the left lung, and a smaller portion of its lower lobe, in an apoplectic condition; and over the latter part of this lung the superficial cells ruptured, and air effused under its pleural investment. A similar apoplectic condition of the lower lobe of the right lung. A large quantity of dark fluid blood in the right, and a much smaller quantity of the same fluid in the left cavities of the heart. Mucous coat of the stomach softened and of a dark-brown colour, at its larger and smaller "culs de sac;" the dark colour caused by a thin layer of blood effused betwixt its inner and middle coats. Eleven ounces of a thick greyish fluid in the stomach. Slight redness of the mucous coat of the duodenum at its commencement.

On a subsequent chemical examination with the same assistance as in the last case, distinct indications of strychnine were readily detected in the contents of the stomach, and in the matters vomited during life; as also, though less distinctly, from the blood and from a portion of the liver.

CASE VI.—*Poisoning by strychnine: accident: recovery.*—Early on the morning of the 21st of last October, William H——, a boy of nine years, on getting out of bed, laid hold of some bread and butter on which a part of one of those powders sold as "vermin killers" had been spread on the previous evening by his mother, with the intention of destroying rats, and was in the act of eating it when the circumstance was observed and his mouth forcibly emptied. The remainder of the bread and butter was got rid of by an emetic speedily afterwards, previous to which, however, he suffered from convulsive twitches of the face and arms, the pulse being at the same time small and wiry; these symptoms ceased on the full action of the emetic.

CASE VII.—*Inhalation of quicklime: œdema of the glottis: tracheotomy: recovery.*—On the afternoon of the 25th of August, 1860, a mason's labourer, in Aberdeen, annoyed at the gambols of some youths around a heap of newly-slaked lime at which he was at work, seized hold of A. M——, a boy of seven years, and buried his face for an instant in the lime. The consequence was that the boy's nostrils, mouth, and throat were filled with the quicklime, and he seemed suffocating till the speedy removal of it from his mouth and throat afforded relief.

This, however, did not prove permanent, as the boy's breathing became soon after again difficult, apparently from closure of the glottis, the mouth and throat being intensely red and a good deal swollen. About five hours after the injury, the late Dr. J. Williamson, who had charge of the case, fearing fatal asphyxia if relief were not obtained, called in Professor Pirrie to perform the operation of tracheotomy, which was effected under chloroform. It was not, however, till six days after the operation that the breathing was so far established through the natural opening, as to permit of the removal of the canula, after which the wound healed readily.*

* It may not be generally known that quicklime acts on some of the lower animals as a speedy poison. Some years ago I was called on to determine the character of a moist mass of oatmeal and quicklime, the eating of which, laid down for the purpose, had, in a few hours, destroyed eleven domestic fowls. It may not be out of place to add here, that table salt is used for the same purpose, two cases of which have lately occurred in this quarter. In one of these the animals were observed, after partaking of the salt, to run to the water, and soon after to drop down dead. Two of them, when examined, had their mouths, gullets, and crops distended with a highly saline semi-solid alimentary matter, and these parts of the alimentary tract exhibited a deep diffused redness over their whole extent.

Case VIII.—*Throwing vitriol over the person: recovery, with the loss of an eye.*—An old man of the name of Tawse was tried at the last Autumn Circuit Court of Justiciary in Aberdeen, and, on conviction, sentenced to five years' penal servitude, for the crime of throwing sulphuric acid over his wife's face with the avowed object of blinding her. When seen soon after, beside the woman's clothes being wetted with the acid to a large extent, the face, hands, and wrists were intensely reddened and painful, the eyelids much swollen, and the conjunctivæ injected, and blisters in wheals had already appeared on the hands. After neutralizing the acid and carefully washing the parts affected, the woman was removed to the Aberdeen Infirmary, where, after some weeks' stay, she recovered from the burns, but only after serious injury to one eye, and the loss of the other from the bursting of its globe.

Art. III.

An Analysis of Two Hundred and Twenty Cases of Pulmonary Consumption. By William Robinson Hill, M.D. Edin., Physician to the Eastern Dispensary, Bath.

In publishing the following remarks, it is not my object to bring forward any original theories, or even any new ideas, on the subject of pulmonary consumption, except so far as deductions from the observation and collection of groups of facts, independently of the opinions and statements of others, may be called original; but I may be able by this analysis either to add my testimony to that of others on some points, or to indicate erroneous conclusions to which the adoption of certain views may lead; and by recording facts in connexion with a particular disease, I may thus contribute one small item to the cause of medicine, inasmuch as the true principles of the treatment of disease will ever be more and more developed according to the correctness and extent of our acquaintance with the laws of its etiology and pathology, and with the course which particular diseases ordinarily run.

To this end, as in other scientific investigations, there are two methods open to the student and investigator, by which he may seek to arrive at the truth. These are the methods of *induction* and *deduction;* the latter undoubtedly the nobler of the two, inasmuch as it is the shorter, and bespeaks a thinking mind of such an order as is not often met with; but for these very reasons it is the least safe, as a slight error in the data on which the reasoning is founded may lead to results very wide of the truth, and if persevered in, in its application to the practice of physic regardless of practical results, may do incalculable mischief; as an instance of which, I would merely mention here the theory of the treatment of phthisis pulmonalis by the hypophosphites, which, grounded on a captious process of deductive reasoning, has had the effect of causing many to swallow drachms of the salts of hypophosphorous acid without deriving a reasonable amount of benefit, as will be more particularly referred to by and bye.

The inductive method seems peculiarly adapted to the science of medicine; a method less brilliant, it is true, than the former, and requiring principally diligent application and an unprejudiced mind for the unbiassed record of facts; whilst its results, if grounded upon a sufficiently extensive and comprehensive collection of such facts, may be regarded as tending towards the goal of truth so far as they go, and will at least not necessarily involve the recorder in groundless speculations.

It is on this principle, then, that any generalizations that may follow are made. A careful study of the disease during a period of twelve months at the Brompton Hospital for Consumption, with an accurate and indiscriminate noting of all facts in connexion with the state of the patient, results of treatment, &c., enables me, by collating the notes of the histories of 220 patients, to form an analysis of their cases, illustrating some points in the disease, the record of which I presume to think may at least afford some assistance to others who endeavour to develop ideas upon the subject of pulmonary phthisis, by yielding a larger field for the basis of their generalizations, which must always be more or less correct according to the amount of well-ascertained facts on which they are founded; and hence it is more than probable that some of my own conclusions will not be found to accord exactly with those of others who have observed a larger mass of cases, or even with those of others who have watched a smaller number of patients under different social circumstances, for it must be remembered that the following remarks being deduced from the observation of patients in the lower walks of life, with only a small proportion of what may be called the lower middling class, would be essentially modified on some heads—such as duration of the disease, probabilities of amelioration, influence of hereditary taint, &c., if they were combined with an equal number of cases under more favourable circumstances for the treatment of the disease.

They would also be found to differ from the results of others in some measure, according to the stage of the pulmonary lesion at the time the patients came under observation, and according to their age, sex, and various other conditions, which will occur to the mind of every one.

Of these 220 cases, 152 were of the male sex, and 68 were females. Of 150 of the male cases, 21 were in the first stage, 57 were in the second stage, whilst in 72 there was evidence of the formation of cavities; and out of 65 of the female cases, 2 were in the first stage, 28 were in the second stage, and 35 were in the third stage. It will thus be seen that a very large proportion of the cases were in what is usually called the third or last stage of consumption. The general result of the treatment of the 220 cases will be seen by the annexed tabular view, but this subject will be again noticed more particularly further on.

		per cent.
Decidedly improved	104	47·2
Received no *marked* benefit	25	11·3
Getting worse	41	18·6
Died	50	22·7

I do not pretend in this analysis to write a treatise upon pulmonary consumption, but merely to indicate the results in the way of generalizations upon a few points in the history and treatment of the disease to which the observation of these cases has practically led me, and before entering upon these subjects would make a few remarks upon the pathological anatomy of tubercle.

Tubercle is by most authors described in a general way as being a "peculiar deposit," possessing a very low form of vital organization, having no capability of further development, and always showing a tendency to degeneration and disintegration.

There are, as is well known, two great classes of theorists on the morbid anatomy of tubercle, the one class holding that the "deposit" is an *exudation*, which has struggled into a certain stage of organic development, but remains in a deformed, unfinished state from its low power of vitality; a description which, to say the least, is sufficiently vague to mystify the casual observer, and is decidedly unphilosophical when we remember that in animal physiology most developments are vouchsafed a cellular origin, and when we further remember the cellular theories of vegetable physiology and pathology. Those holding this exudation-theory generally speak of tubercle as a substance which is deposited in a fluid or semi-fluid state, and that it soon assumes the solid form—a statement which has been handed from one to another without the shadow of a foundation, for in the tubercles of serous membranes, where it may be examined in its most uncomplicated form, nothing but tolerably well-formed nucleated cells can be discovered.

The other class of theorists hold that it is merely a retrograde metamorphosis and degenerated condition of pre-existing tissues or elements—a doctrine sufficiently comprehensive, probably, to include the truth, but incapable of conveying much idea of the real state of matters.

The error of observers generally seems to have consisted in taking for examination as the type of tubercle the cheesy masses found in the lungs and elsewhere, which are in reality either masses of tubercles undergoing a process of degeneration and disintegration, such as any morbid product is subject to under similar circumstances, or they may be thickened and metamorphosed purulent or cancerous matters; hence tubercle has been described microscopically as consisting of molecules, granules, and ill-formed irregular cells; whereas, any one who has examined the tubercles in tubercular meningitis can testify that the cells have a most legitimate rounded appearance, and contain nuclei, bearing, in fact, a strong resemblance to pus-cells, which circumstance led Robin to doubt the propriety of considering it a tubercular disease.

For these reasons, then, seeing that the cellular development of an amorphous exudation has not been proved to demonstration, and appears as improbable a theory as that of spontaneous generation, inasmuch as they both involve the notion of the spontaneous origin of life; and believing as I do in the idea of the cellular origin of pathological products, the only theory of tubercle that seems to me to be true, rational, and philosophical, is the one proposed by the master-mind of him who gave out, and has so ably maintained, the doctrine of the cellular pathology. I refer, of course, to Professor Virchow, of Berlin, probably the greatest thinker that Germany can at present boast of, combining what is so rarely seen in one person—the faculty of observing minute details, and of making large and important generalizations, at once the workman, the builder, and the architect—the workman, laboriously and patiently collecting and preparing the materials; the builder, arranging them and massing them together in their respective places; and the architect, devising in his far-seeing expansive mind the dim outline of a noble edifice.

Without entering into an exposition of the cellular theory generally, I cannot do better than give M. Virchow's views on the origin of tubercle, as they are found in his work entitled, 'Die Cellular-Pathologie,' amplifying them, where possible, by my own notes of a lecture which I heard delivered on that subject by himself.

In the first place it is important to know what we understand by the term "Tubercle." Originally the name was applied to anything in the form of a little knot or little lump, as the Tubercula carcinomatosa, Tubercula scrofulosa, and Tubercula syphilitica; and Laennec was the first to distinguish two different forms of tubercle in the lungs, analogous in themselves but differing in their development: 1. The isolated tubercle, or miliary tubercle; and 2. Infiltrated tubercle. This was the first step in departing from the original idea of tubercle; and later in its history the cheesy nature of the production gradually came to be considered as its ordinary specific character. If a lung in a state of tubercular infiltration is examined, its component elements are found to be the same as result from the changes that take place in the products of inflammation, hence Virchow is of opinion that what is generally called "infiltrated tubercle," is in fact, with few exceptions, to be referred in its origin to an inflammatory, purulent, or catarrhal product, which by degrees, through a process of imperfect resorption, has passed into a shrivelled condition.*

The isolated tubercle, or tubercula granulata, he considers may have three different sources of development; first form, where it is situated in the parenchyma, confined perhaps to a dozen alveoli, the principal part of the morbid product being in the alveoli as in pneumonia, in which disease this condition is termed *hepatization*, and hence Virchow names this form *cheesy hepatization:* it may be lobular or lobar.

The second form shows itself as really interstitial, situated in those points where a certain amount of connective tissue exists. It occurs as little knots, which may afterwards seem to form one mass, but can always be distinguished as composed of separate grains, called miliary tubercles.

* See Die Cellular-Pathologie, in ihrer Begründung auf physiologische und pathologische Gewebelehre, p. 425. Second edition. Berlin, 1859.

The third form begins on the small bronchi entering the alveoli. A simple chronic inflammation of the bronchus takes place, causing purulent or muco-purulent secretion, which on cross section appears as a yellow spot in the centre. These granulated bodies have been taken for tubercle, because observers never thought that they had sections of small bronchi before them. This form may be easily distinguished from the real tubercle granule with the naked eye by simply rubbing a little blood over the cut surfaces of the granules, when the inspissated mucus contained in the lumen of the minute bronchial tube becomes reddened, whilst the thickened wall of the bronchus remains uncoloured. This appearance is of course not seen in the real tubercular granule.

Professor Virchow, therefore, in his theory of tubercle holds to the original idea of its being a grain or little knot, and the tubercular granule he considers as a new formation, which from its first development onwards is necessarily of cellular nature, and which, as a rule, proceeds from the connective tissue (*Bindegewebe*) exactly as other new formations do, appearing as a little knot when in the interior of the tissue, and as a projecting knob when situated on the surface, consisting in its entire mass of one- or many-nucleated cells.* These cells often bear a very close resemblance to pus-cells, and are only to be distinguished from them in possessing larger and more numerous nuclei, with a comparatively smaller cell-wall than the pus-cell. They are also distinguished from cancer and cancroïd cells by the latter being much larger formations, with strongly developed nuclei and nucleoli. He also insists upon the desirability of retaining the name "tubercle" for this form of morbid development, because he believes that a tubercle never increases to any size, never, as he says, becomes a "tuber;" and he regards the large tubercular masses found in the brain and other places as consisting of thousands of little tubercles, which by their close apposition appear as one mass.

In describing the further history of tubercle, M. Virchow says that usually an imperfect process of fatty metamorphosis soon commences in the centre of the tubercle granule, after which, all trace of moisture disappearing, the elements begin to shrivel, and the grey semi-translucent granule presents a yellowish opaque spot in its centre, and thus the cheesy metamorphosis is established, which, though the usual, is not to be considered the necessary and characteristic issue of tubercle; for, on the one hand, if the fatty metamorphosis is complete the tubercle may in a few cases be absorbed, and on the other hand, other cellular new-formations may pass into the condition of cheesy metamorphosis; hence the necessity of examining tubercle in its mode of origin rather than in its result, if we wish to have a correct notion of its pathology.

Supposing this, then, to be the true theory of tubercle, it is clear that the term at present used, of tubercular "deposit," is meaningless and incorrect; and perhaps the more exact expression in speaking of tubercular affection would be to call it the "tubercular form of inflammation," as we have always spoken with hitherto unfounded correct-

* Loc. cit., p. 427.

ness of tubercular *meningitis*, synonymous with "tubercular inflammation of the meninges."

Having thus treated of the morbid anatomy of tubercle in what I believe to be its essential nature, I shall not notice further at present the other pathological changes or conditions of the lungs incident upon the presence of tubercles in their substance; I refer to the process of softening and ulceration of the pulmonary tissue, with expectoration of the same, and the formation of cavities, which sequelæ are to be regarded merely as the result of the presence of a disintegrating, disorganized substance, giving rise to local irritation, and are not a necessary issue of tubercle. Therefore to speak of the disease being in the "first," "second," or "third" stage, according to the condition of the lungs, is also erroneous both in a general way and anatomically; firstly, because the general condition of the patient and the physical signs do not always accord with the state of the lungs, as is occasionally seen by the sudden death, from hæmoptysis or otherwise, of a patient pronounced to be in the first stage of consumption, when a small central vomica would entitle him to be classed amongst those in the *third* stage; neither is it anatomically correct, because the second and third conditions are not really stages of the disease in its essence any more than the metamorphoses of an apoplectic clot are stages of apoplexy; however, the terms as applied to the condition of the lungs will be used in this paper for the sake of brevity and clearness.

I shall now pass on to the causes of pulmonary consumption, which may be considered under the usual heads of predisposing and exciting; and under the former we shall inquire into the amount of influence which the sex, age, hereditary tendency, and social condition of the patient may seem to exert in the development of the disease.

Sex.—Whether the disease is as a fact more common in males than in females I am unable to state, for the circumstance of my having observed most cases in the male subject may be regarded as accidental.

That such is the case is rendered probable by the fact of a much larger proportion of males applying for relief at charitable institutions; for example, at the Brompton Hospital for Consumption up to the year 1849, 2679 males and only 1679 females had been under treatment;* but this, on the other hand, may to a great extent be explained by remembering that it is often more convenient and necessary for a sick father of a family to leave his household than for the mother to do so, which is proved by the fact that of my 152 male cases one half were married men, whilst of the females less than a quarter were married.

The sex, however, will be observed to exercise a considerable influence in the *age* at which the disease attacks the individual, and on the development of the disease in those in whom there exists no hereditary predisposition, inasmuch as the female sex is in a much larger proportion attacked before the age of thirty, and in a still greater proportion before the age of twenty, whilst those affected without any hereditary taint of the *male* sex will be found to predominate over the

* See Cotton on Consumption, p. 57. Second edition.

tainted in inverse proportion to that which obtains in the female cases.

Age.—Pulmonary consumption is proverbially a disease of youth, which is notably confirmed by the statistics of these 220 cases, and I must here mention that these remarks are entirely independent of cases of infantile phthisis, none of my cases having been under the age of eight. There is of course the greatest difficulty in ascertaining the date of the commencement of the pulmonary lesion, but as great care and trouble was taken in these cases to ascertain the time of the commencement of the *slightest* cough (a point so difficult to ascertain from patients, as they are apt to take no account of it for a long while), as well as to note any other symptom which might indicate the date of attack, it is hoped that the statistics may, at any rate in a general way, be considered reliable. The most common period for commencement is between the ages of twenty and thirty, in which 43 per cent. of the 220 cases commenced; the second most frequent period is before the age of twenty, in which 28 per cent. were attacked, thus leaving a very small proportion in whom the disease commenced after thirty years of age. These statements agree so far with Dr. Cotton's observations, in that he states* "that the disease is most frequent between the ages of twenty and thirty," to the amount of more than 39 per cent. They disagree in this point, that Dr. Cotton finds the period of thirty to forty the next most frequent age. Now, in investigating this subject, I took great pains in each case to endeavour to ascertain the age at which the disease seemed to *commence*, and as Dr. Cotton makes no remark on this head, I think it is more than probable that his deductions are founded on the ages of the patients taken as they came under his notice, which, in examining my own cases, would, I find, materially increase the per-centage of cases between thirty and forty, as also decrease the number of those under twenty.

The number of females attacked before the age of twenty predominates over the number of males attacked before that age as 38 to 24, and before the age of thirty as 78 to 68, thus showing that the number of females attacked before the age of twenty exceeds the number of males attacked before the same age by a very large proportion. Why such should be the case will, I think, be amply apparent from the facts stated below, which prove that the disease is developed much more frequently in the male, by what may be called accidental and extraneous circumstances, and to which he is almost as much exposed from the age of thirty to fifty, as from the age of ten to thirty.

Hereditary predisposition.—This is popularly recognised as of great moment in the development of the disease, and not without good cause, as I think, though at what rate its influence must be valued is uncertain, and what the chances are in favour of an individual being attacked who is a member of a consumptive family, independently of other causes, cannot be ascertained with any precision until numerous

* Loc. cit. p. 54.

observations have been made as to the number of members of such families who, *cæteris paribus*, are not attacked.

Of 213 cases in which a note was taken with accuracy regarding the family history, 100 or 46·5 per cent. had consumptive relatives, whilst 113 or 53·5 per cent. had none.

This per-centage differs from that given by Dr. Walshe, who says that 26 per cent. of phthisical patients come of a phthisical parent, thus including only cases of direct hereditary transmission, which corresponds with my own *direct* cases which numbered 49, or 23 per cent.

The majority, therefore, in my own statistics, being in favour of those free from family predisposition, it would seem at first sight as if the hereditary element was one of which much account should not be taken, but it must be remembered that there are other predisposing causes, and that the exciting causes must therefore act in numerous instances independently of the one we are now considering, which will be plainly seen by a minuter examination of the hereditary influence, inasmuch as in the sex most removed from the influence of other predisposing causes, that of the hereditary taint will be found largely to predominate; thus, of the above quoted 213 cases, 151 were males, in 63 of whom, or 41·72 per cent., the hereditary tendency was marked, whilst in 88, or 58·28 per cent., there was none; the rest of the number, 62, were females, of whom 37, or 59·7 per cent., had a consumptive family history, whilst only 25, or 40·3 per cent., were free from it; thus showing in females, who may be regarded in a measure as comparatively exempt from some of the causes of phthisis to which men in that class of life are exposed, that the predominance in favour of hereditary influence is as great as 3 to 2 of the male cases.

It may therefore, I think, be safely argued that, *cæteris paribus*, the fact of family taint increases the probabilities of an individual being attacked to a somewhat considerable extent; and further, that it increases the probability of individuals being attacked at an earlier age than would otherwise happen, for of an equal number of cases in which there was direct parental disease, and in which there was no family history whatever of consumption, 73·2 per cent. of the former were attacked before the age of thirty, whilst of the latter, the disease commenced before the same age in only 58·6 per cent.

Social condition.—The influence of marriage is said by some physicians to be important in the development of consumption, but on this head I have nothing to offer: 1st, because I have made no observations on the subject, and 2nd, because in hospital patients the medical attendant is acquainted with only a very small portion of the pathological life of his patient; and, therefore, I conceive that this subject would be more accurately investigated by the combined experience of medical men in practice who have the opportunity of watching their patients through a series of years.

That the social condition in respect of the want of adequate food and clothing, the breathing of a confined and polluted atmosphere, and the exposure to the vicissitudes and inclemencies of the weather, exercises a considerable power in the development of pulmonary disease, is proved, amongst other things, by: 1st, the more frequent occurrence of the disease amongst those so situated; 2nd, by the fact, which I think I have observed in a general way, that those patients in whom these conditions are mainly the cause of the disease are most markedly benefited by removal therefrom, and by hospital treatment; and 3rdly, the facts which have been above used to show the amount of influence which hereditary tendency exerts, may inversely be applied for proof here; inasmuch as, granting the hereditary influence, the disease is seen to occur idiopathically, if I may use the expression here, with much greater frequency in males, who necessarily are more exposed to the vicissitudes of weather, than in females, a frequency represented by 3 to 2.

The exciting or proximate cause of tubercular affection of the lungs is very vague, very difficult to grasp with accuracy, and, in the present state of our knowledge, must probably be attributed to that very comprehensive and indefinite term of "catching cold," which popularly accounts for such a large majority of human ills. This seems to be almost the only thing to which amongst those attacked, the rich and poor, the well-fed and the ill-fed, the well-protected and the ill-protected, are alike exposed; but it must be associated in our minds with other circumstances, inasmuch as all catarrhs and coughs do fortunately not take the form of confirmed pulmonary disease, and among these other circumstances, the most generally recognised classes seem to be those which include all circumstances that are mentally depressing, and all causes that are of a physically debilitating nature.

Why different effects should be produced under apparently the same conditions is at present not discoverable, and the reason probably lies in the secrets of the vital and organic constitution of the human system, with which we are unacquainted, and for the revealing of which a correct idea of the pathology of the disease is evidently one of the most important indicators.

The physical signs of pulmonary consumption.—There are no physical signs which, taken alone, can of themselves be said to indicate unmistakeably the presence of tubercular disease of the lungs in the first stage, the so-called stage of deposit. For the morbid signs which we recognise in these cases are merely a modification or alteration in character or intensity of the normal sounds yielded by examination of the healthy chest, the alteration being due to a greater or less amount of consolidation of the pulmonary organ, and hence influenced by the general laws of acoustics, so that the same abnormal sounds may be produced under similar circumstances by consolidation of any nature, and not necessarily tubercular. In the diagnosis of early pulmonary consumption, it is therefore plainly necessary to take into consideration other facts in connexion with the physical signs. Of these one important circumstance is the *position* of the consolidated portion, as it is a well-known fact that in nearly every case of consumption, the disease commences at the apices of the lungs, and if the consolidated portion is of a circumscribed character at the apex of one or both lungs, the argument is strong in favour of tubercular affection.

Other circumstances of a general nature which it behoves us to take into consideration may be enumerated, as the comparative long duration of possibly only a slight cough, the occurrence of hæmoptysis, of night sweats, of slight pains described by patients as being of a "dragging" character, the loss of flesh, the commencement of shortness of breath when exertion is made, &c. Any or most of these may be either absent, or, if present, from a variety of causes, beyond our power of discovery.

To consider now the physical signs themselves more in detail, I will first mention that of forty-two lungs in which the disease was observed in the first stage, in male cases, nineteen, or nearly one-half, presented marked conditions of a degree of dulness on percussion, of bronchial breathing, and of more than usual resonance of the voice, and I may remark that in at least sixteen of these cases the correctness of diagnosis was put beyond all question, either by personal post-mortem inspection or by observation of the cases until they passed unmistakeably into the second or third stages. These signs, then, when found in conjunction with one or more of the above-mentioned concomitant general symptoms, may be considered as positive proof of the presence of tubercular affection in an early stage. It will be seen, however, when more closely examined, that each of these signs is, in its expression, of a relative nature only, and, further, may from various causes be absent, as I can show from actual cases, without in any way detracting from our certainty of the presence of tubercular disease.

In judging of the percussive resonance of a given portion of the pulmonary region, we must bring our general knowledge of the normal resonance to mind, in addition to comparing it with the same situation on the opposite side of the chest, and with other portions of the same pulmonary organ; nothing is therefore more probable than that a slight amount of comparative dulness may escape even a tolerably experienced ear.

The absence of dulness on percussion compatibly with the presence of tubercular consolidation may be accounted for in one of three ways: 1st, from the presence of emphysema of the lungs, which modifies also other abnormal physical signs, as in one case, where there had been cough for two years, with loss of weight to the amount of at least forty pounds, whilst there was no dulness on percussion, no increase of vocal resonance, and deficiency in the respiratory sounds. Post-mortem inspection eight months after the first examination, showed cavities in each lung, with much emphysema. Under this head I omit all such great deviations from the normal physical condition as pneumothorax, hydrothorax, &c., which more or less preclude the possibility of ascertaining the condition of the lung at all.

2nd. The presence of some degree of dull percussion may not be perceived, on account of great disease and absolute dulness on the opposite side, in comparison with which the percussion of the doubtful side appears of normal resonance. This I have seen in five or six cases, where a suspicion of the presence of tubercle from other decided physical signs, notwithstanding the absence of all perceptible shade of dulness, has been converted into a certainty a few weeks later, either by autopsy or more decided physical signs.

3rd. Dulness may be really absent when tubercle is present in sufficient quantity to produce harsh, interrupted, or even slightly bronchial breathing, but is scattered through the lung substance, leaving unaffected, sufficiently crepitating portions between the nodules which produce the resonance on percussion. This is common in those cases of miliary tuberculosis, where the physical signs are often so obscure as to render it uncertain until the decease of the patient, whether the disease was tubercular affection of the lungs with accompanying fever of a typhoïd character, or typhoïd fever with concomitant pulmonary complication.

Of these cases I have seen one where the too unmistakeable symptom of perforation of the intestine alone decided the diagnosis on the side of enteric or "pythogenic" fever, and another where post-mortem inspection alone yielded positive evidence of the correctness of the alternative diagnosis of tuberculosis.

The bronchial breathing of auscultation is a sound tolerably fixed and definite in its character, in consequence of being derived from the normal condition of the respiratory sounds when listened to in the neighbourhood of a large bronchial tube. But it may vary widely in its intensity, and therefore more attention should be paid to its essential character than to its loudness. Its essential character, to my mind, consists in the increase in length and harshness of the expiratory sound in its normal comparison with the sound of inspiration; or, in other words, it consists in the expiratory sound resembling the inspiratory sound more than is observed under normal conditions.

This physical sign, when existing in a region where naturally only vesicular respiration should be heard, is of great value in indicating pulmonary consolidation; but only when observed under conditions above-mentioned can it be interpreted as evidence of *tubercular* consolidation, neither can its absence be considered always as negative proof of the non-existence of tubercular affection, as the emphysematous case above cited proves, whilst I have notes of other cases in which, with dulness and increased vocal resonance, the respiratory sounds could be described only as "rough" or "deficient," which cases, in the course of their history, after the lapse of a few weeks, were found to present undeniable signs of tubercular affection.

Finally, the increase in intensity of the vocal resonance is, when distinctly present, a very strong evidence in favour of pulmonary consolidation, but is comparatively often absent when other signs leave no doubt of tubercular affection; and reversely, there are fallacies attending its presence which detract from its value, inasmuch as under normal conditions its intensity varies in similar situations on the opposite sides; and again, in health it varies considerably in different subjects.

These, then, being the conditions under which we may with confidence declare the existence of pulmonary tuberculosis in an early stage, it becomes a subject of great interest as well as of great importance in some cases to consider if there are not certain less decidedly well-pronounced deviations from the normal condition of auscultation by which we may be justified in expressing our conviction of a diseased state. This knowledge can manifestly be obtained only by the careful observation of a large number of cases for a period of time, noting any slight deviation, and comparing the result after the lapse of a longer or shorter period of time.

Following this plan, I have met with several cases in which certain slight deviations from the normal character of the respiratory sounds, when taken in conjunction with sundry other circumstances favouring the probability of tubercular disease, such as manifest affection of the opposite lung, gradual wasting of the body, long-continued cough, &c., have given rise to a suspicion of tubercle, which at a later date has been confirmed by marked physical signs, or by post-mortem examination.

These deviations it is rather difficult to indicate in words, and have been described by different authors in different ways, because the same sound will suggest to the minds of different auscultators a similarity to a variety of other sounds. Avoiding, therefore, comparisons with other sounds, I would describe them according to their character and intensity in relation to the normal respiratory sounds, and speak of them as a *deficiency* in intensity, a *roughness* or unevenness in the character, and a *harshness* in character maintaining the evenness of normal respiration, but to be distinguished from exaggerated healthy or so-called puerile respiration, being rather a very mild and early expression of the bronchial breathing.

In addition to these there is the divided respiration, in which one or both of the respiratory sounds are divided into two, three, or more distinct and separate parts. As instances, I will mention some of the cases in which these deviations were noticed.

M. L. Harsh respiration, generally over right lung, with slight dulness and little increase of vocal resonance at the apex, and distinct evidence of a cavity in the left lung. Five weeks after this note was made the patient died from the occurrence of pneumothorax, and tubercle was found *scattered through* the right lung, but by no means densely so.

F. A. Slightly harsh respiration under right clavicle without dulness. Evidence of a cavity on the left side. On examination five weeks afterwards, there were dulness, crepitation, and loud vocal resonance at the right apex.

A. B. Deficient respiration and slightly impaired percussion at the right apex, with softening in the left lung; after two months presented the following physical signs—dulness, crepitation, and bronchophony.

T. H. Two months before his death a little harsh respiration was the only abnormal physical sign at the left apex; whilst at the time of the post-mortem examination there was a large vomica in that situation.

R. B. Rough respiration with dulness at the left apex was changed into cavernous respiration a fortnight later, the change being verified two days afterwards by post-mortem inspection, showing the upper lobe of the left lung quite dense with tubercle, with two or three small cavities in the apex.

G. B. Divided inspiration at the right apex, with some other signs of tubercle, was found after two months to be developed into the second stage.

Several similar cases might also be given, but these are considered sufficient to show that such slight deviations are worthy of attention, and may give good reason for a doubtful, if not unfavourable diagnosis.

The second stage of phthisis pulmonalis is said to commence when we find accompanying or superadded to the physical signs of the first stage, that condition which is described under the head of moist crepitation. The character of that moist crepitation, which is considered as evidence of the softening of tubercle, can only be made manifest to our conception by actual auscultation, as there is a variety of sounds, originating from different physical conditions to which the term *crepitation* is applied—such as *dry* crepitation, and other forms to which a specific name is attached, indicating the pathological condition under which it exists—for example, *pneumonic* crepitation.

It is not my intention to enter into a description of the various kinds of crepitation, but would merely remark that the phthisical moist crepitation may in a general way be recognised by the following characters. The bullæ are of moderate size, between the fine crackle of pneumonia and the large, as it were, round bullæ of an œdematous lung, and they give the idea of air passing through a viscid fluid, as if the bubbles were unwilling to break. Further, the moist sound usually accompanies both the respiratory acts, and is not easily removed by a cough. In addition, the position of the crepitation and other circumstances may be taken into consideration in aiding the diagnosis, as in the first stage.

Whilst in many, perhaps the majority of cases, the occurrence of moist crepitation may be the first appreciable sign of the softening of tubercle, it must be remembered that in other cases where the presence of tubercle has been ascertained, there are often other indications of softening developed previous to the changes in auscultation; and I would here notice the importance of making the patient cough whilst the ear is still on the stethoscope, as crepitation is often heard in the forced inspiration preceding and following the act of coughing, when it is absent in ordinary deep inspiration. Amongst the other symptoms indicating the occurrence of softening may be mentioned the character of the *expectoration*, which microscopically may present evidence of the disintegration of pulmonary tissue, and *hæmoptysis* in certain cases—viz., in those in which there is good reason for believing that the blood proceeds directly from a ruptured pulmonary vessel, which must be the result of disintegration and ulceration, the real explanation of the condition of softening. Also, in certain cases the crepitation has entirely ceased, the patient improving generally, leaving only the physical signs of the first stage. In these, therefore, we should be apt to conclude that the disease had not reached the second stage.

As the division into second and third stages is arbitrary, so the passing of a lung from the second into the third is in many cases not to be detected by the stethoscope. For though in a large proportion of cases one or more cavities are formed in one or both of the lungs at some period in their history (the only case to the contrary that I can remember of those that I have seen to their end being one that succumbed at an early stage of the pulmonary disease from tubercular meningitis), yet as there are cases in which small cavities are found by post-mortem inspection, undiscoverable by the closest physical examination during life, from being situated deeply in the organ, it is but reason to suppose that many cases have passed into the third stage, while as yet we are unable to pronounce such to be the case from physical signs.

There are, however, certain auscultatory signs which when present may be said to be distinctive evidence of the presence of a vomica in the lung-substance. The percussion over a region containing a vomica is generally exceedingly dull, from the density and great consolidation of the surrounding pulmonary tissue. In a few cases, however, the percussion note is of rather a tympanitic character, when the vomica is large, near the surface, and contains only air.

The bruit *de pot fêlé*, or cracked-pot sound, is one on which great stress has been laid, but the value of this phenomenon has been ably inquired into by Dr. Hughes Bennett, and proved to be of limited extent. For my own part, I have always regarded this sign more as a matter of interest than of importance, inasmuch as I believe that in those cases where the sound is unmistakeably developed from the existence of a vomica, the condition of the lung can be with confidence pronounced upon from the other physical signs. It will often indicate the large size of a vomica, and is most easily elicited where there are numerous anfractuous communicating cavities. In auscultation the signs that may be considered indicative of a cavity are when the respiratory sounds, the moist rhonchus, or the voice have a cavernous or amphoric character. The moist rhonchus resembles often a loud gurgling, and in some large vomicæ metallic tinkling, resembling the sound of water dropping into an empty pitcher, may be heard.

The exceeding loud vocal resonance has been named *pectoriloquy*, but it is often difficult to draw a line of distinction between loud bronchophony and pectoriloquy. There is one character of the voice which may be considered almost invariably diagnostic of the presence of a cavity—the *whispering* pectoriloquy—where a whisper uttered by the patient is heard through the stethoscope with as great or greater distinctness than from the mouth, as if the whisper originated in the chest. I believe, however, from cases observed, that a modified form of whispering pectoriloquy may be detected in the chest occasionally without the presence of a vomica.

The importance of a physical examination of the chest is thus seen to be very great, not only in ascertaining the existence of tubercular disease, but still more in revealing also the extent of the affection. To aid in the diagnosis of its existence there are also certain general symptoms of dire indication which demand a slight notice, and of these, gradual and, it may be, slight emaciation is one which may cause some degree of anxiety before the patient has a cough, or *knows* that he has a cough. In illustration of this I will mention the case of one man, who averred that he had had the slightest possible cough for one month previous to the time he came under my observation, and *only* for one month, whilst he was aware of having lost flesh for at least a month previous to the commencement of his cough. On examination of the chest, the sounds of percussion and auscultation deviated so very slightly from a perfectly normal condition, that but for general symptoms it would probably have been pronounced healthy. The emaciation continued, and six weeks after, another examination of the chest revealed only slight dulness on the right clavicle and a little increase of vocal resonance below it, whilst in another fortnight the pulmonary affection had advanced so rapidly as to give unmistakeable evidence of the existence of a small vomica at the right apex, whilst the additional loss of weight amounted to seven and a half pounds, twenty-three pounds less than his normal standard.

Hæmoptysis, or the expectoration of florid blood, is a symptom that often assists greatly in aiding the practitioner to decide upon the presence of tubercles in the lung, and may in some cases be the first symptom that enables him to pronounce upon the state of the lungs, not to mention its being frequently the first circumstance that leads the patient to be anxious about himself. There are, of course, cases which show that it is not to be regarded as a positive sign of tubercular disease, such as the well-known vicarious hæmoptysis in the female; but to enter upon this subject would be rather beyond my object. Its great importance will be evidenced by the fact, that of 212 male and female cases, it occurred to a greater or less extent in three-fourths of the number, or 75 per cent. Of male cases alone the average of occurrence was rather greater than of the female cases taken alone, being 77·8 per cent. in the former and 68 per cent. in the latter. Very copious hæmoptysis, that is, to the amount of about a pint and upwards at one time, occurred in about 15 per cent. of all the cases, and that decidedly more frequently and more copiously in the male sex. The number of deaths consequent upon copious hæmoptysis was five, or 2·27 per cent., one female and four males, the female and one male having died in the attack from complete loss of blood, the ventricles of the heart being in each case found empty. The others died respectively one, seven, and eleven days after the commencement of the hæmoptysis. In four of the cases there was much disorganization of both lungs, with large cavities; whilst in the one that survived eleven days the pulmonary affection was limited to a little miliary tubercle in each lung, and three vomicæ, not larger than almonds, in the one from which the blood proceeded. In three it was distinctly traced that the source of hæmoptysis was in the left lung; in one it *appeared* to have proceeded from the left, and in one apparently from the right.

In none of the above cases could the actual source be traced to a ruptured vessel, though in three of them the locality was determined by the clots of blood which filled the bronchi and the vomicæ from which the hæmorrhage proceeded.

In these cases of sudden death from hæmoptysis the amount of blood expectorated cannot be regarded as evidence of the amount which the system loses, for a large quantity may be swallowed, as occurred in one of those above mentioned, where only two ounces were expectorated, and yet the patient died in the course of a few minutes, the autopsy revealing the heart's cavities quite empty and the stomach full of coagulated blood. On the other hand, it is interesting to note what a large quantity of blood may sometimes be expectorated and yet the patient not only survive but continue to live some years. Patients often state that they have had hæmoptysis to the extent of some pints or quarts, but these statements must be received with reserve, as the alarm occasioned by the occurrence will cause them unwittingly to exaggerate. The following case, however, came under my own observation, having a history of frequent and copious attacks of hæmoptysis to the aggregate amount of at least six pints in the course of six months. This patient had physical signs of softening at the right apex, and slight consolidation in the left lung. In a period of two months I was at his bedside during repeated attacks of hæmoptysis, on almost each occasion occurring to the amount of half a pint or a pint, and the aggregate quantity of blood lost was at least six and a half pints, independently of much that was swallowed and subsequently vomited. On most of these occasions he did not feel weaker after the hæmoptysis, but experienced great relief in the chest, and at the end of the two months he weighed only two pounds less than before the loss of the six and a half pints of blood. During a subsequent period of two months' observation this patient improved in health and strength without recurrence of hæmoptysis.

Whether patients sometimes have hæmoptysis before the commencement of the cough is a question that admits of doubt. Such would seem to be the case from the account of many of the most intelligent patients. There are some who affirm they had not even the slightest "hacking" cough before the hæmoptysis, but that since its occurrence the cough has persisted. Others state that having positively had cough only six months, they had an attack of hæmoptysis to the amount of perhaps half-a-pint to some pints two or three years previously, which is generally attributed to an overstraining in lifting weights, &c.

In the former class it is more than probable that some continued cough existed before the attack, too trifling to call the patient's attention to it, and in the second class, it becomes a question whether the existence of tubercles in the lungs is to date from the cough or from the hæmoptysis. Thus much, however, is to be learnt from these cases, that in many instances the occurrence of hæmoptysis is *practically* the first indication of tubercular disease of the lungs.

It may be interesting, though perhaps not of much real importance, whilst speaking of the anatomical condition of the lungs, to advert to the subject of the relative frequency and priority of the affection of the right and left pulmonary organ.

Of 157 male and female cases in which a note on this subject was made, the disease was found to have attained a more advanced stage in the right lung, comparatively with the left, in 59·2 per cent. of the cases. Thus it would seem that the right lung is more prone to the affection, but this per-centage is liable to modification, as will be seen by taking the male cases alone and the female cases alone: 109 of these patients were males, and of them the greater amount of disease in the right lung was found in 65 per cent.; whilst of the 48 female cases, the right lung was principally affected only in 45·7 per cent.; so that the per-centage would seem to be materially altered according to the relative proportion of males and females; and it may also be said that these numbers are too small to found any conclusions upon. I may mention, however, in support of the above, that of 114 other male and female cases, taken from the records of the hospital, in which, from the account, it could safely be concluded in which lung there was the greatest amount of disease, the result gave a per-centage of 55 right lung cases, corresponding somewhat nearly with my own 59·2 per cent. Also from the report of 170 cases, a proportion of 53 per cent. showed more cases of excess of disease in the right than in the left lung.

I ought, perhaps, to mention that the above ratios do not include any cases in which the disease seemed to be equal in both lungs, a fact which must be remembered in comparing these statistics with the results arrived at by others.

These three independent testimonies, however, do not accord with the investigations of others; for example, Dr. Cotton, in his work on consumption, finds among 839 cases a per-centage of only 45·7 of right lung cases; and he infers, therefore, that "the left lung is rather more liable to become tubercular than the right; but that the difference in this respect is so small as to render it a subject of curiosity only, and not of the slightest practical value, either in the diagnosis or treatment of consumption;" an opinion which my own observations would tend to confirm.

Art. IV.

On Live-Birth. By W. B. Kesteven, F.R.C.S.

The vexed question of what constitutes live-birth has been recently revived in consequence of a case tried before the Vice-Chancellor Sir J. Stuart. In this case the medical opinions were opposed; it may therefore be instructive to consider the grounds of the views entertained by the medical witnesses, and to review the opinions on this matter of the principal British and foreign medico-legal authorities, together with the laws of various countries as bearing upon this question.

The case which has given rise to the present discussion is briefly as follows:

"Upwards of twenty years ago, a medical man died without a will, leaving property, and a widow who was pregnant at the time of his death. As his widow, she succeeded to a portion of the effects; but in case of her having a child, and the child dying, she became, as the heir of her own offspring, entitled to a further portion. She was delivered by Mr. Freeman, now of Plymouth; and the question was, Did the mother give birth to a live or a dead child? Mr. Freeman distinctly stated in his affidavits that the funis pulsated after the separation of the cord, and that the child moved vigorously a minute before birth. He could not swear to the fact of respiration having taken place, but he remembered that the chest was full and arched, and he believed it must have breathed, because it was his usual practice not to divide the funis until respiration had taken place. He also placed the child in a warm bath, and he stated that he should not have done this had he not considered the child alive and capable of resuscitation. His distinct opinion was that the child was born alive, but weak; and that it died shortly after birth from the effects of a severe labour."*

This case in most points bears a close resemblance to that of Fish *v.* Palmer, in the Court of Exchequer, 1806, which has come in some sense to be looked upon as a *cause célèbre,* and being to be found in all standard works on medical jurisprudence, to these we may refer for details thereof. In that case, as in the instance before us, the most distinguished obstetric physicians were consulted, and differed in opinion. Dr. Denman, drawing the distinction between intra- and extra-uterine life, contended that the child was not born alive; Drs. Babington and Haighton held the view, more consonant with physiology, that where there was muscular movement there was life. In Brock *v.* Kellock, Dr. Robert Lee and Dr. Ramsbotham strongly urged that as respiration was not proved to have taken place, the child was still-born. Dr. Tyler Smith, in whose opinion Dr. Alfred Taylor concurred, held that pulsation of the funis, after separation of the cord, was sufficient evidence of live birth. Mr. Freeman, of Plymouth, who had delivered the mother, was also of opinion that she had given birth to a live child.

Dr. Robert Lee suggested that the pulsation in the cord alleged in this case might have been merely the pulsations of the arteries in the finger and thumb of the accoucheur! Such pulsations, even if they had occurred, were regarded by Dr. Lee as simply analogous to the contractions known to occur in the hearts of some animals for hours or even days after their separation from the body. Dr. Ramsbotham concurred in the opinions expressed by Dr. Lee.

Dr. Tyler Smith, with the laws of physiology on his side, opposed to the laws of several countries, successfully contested the affidavit of Dr. Lee. He contended that it is scarcely possible to confuse the two pulsations, that of the child's cord and that of the accoucheur's hand; and urged the wide difference that exists between the beats of a

* Brock *v.* Kellock: Lancet, May 11th, 1861.

heart within the thorax of the child, from those of the heart detached from the body of one of the lower animals, although in both cases these very contractions are indisputable proofs of vitality.

The Vice-Chancellor affirmed this view of the case, and held it proved that the child had been born alive.

It is considered, as we have seen in the preceding case, by some medical authorities that a distinction should be drawn between intra- and extra-uterine life. Casper, of Berlin, regards respiration as the only evidence of independent existence. Beek looks upon all movements previous to the establishment of respiration as merely the last remains of the muscular contractility of fœtal life in the sense held by Drs. Lee and Ramsbotham at the late trial. To draw such a distinction is, however, not less contrary to physiology than it is pernicious in practice, and calculated to favour infanticide. A case of child-murder reported by Dr. Ramsbotham bears out our view of this matter:—

"A servant, sleeping in a room alone, was taken in labour during the night. She placed herself on a slop-pail, which stood in her chamber, quite full of the house-slops of the previous day. The child was expelled head foremost into the fluid. She raised herself suddenly, and broke the funis close to the placenta, leaving the placenta itself *in utero*. Her mistress, finding her in the morning unable to rise, called the family surgeon to see her. He discovered the child, and sent for me. She stoutly denied that the child was hers; but, on examining the abdomen, I felt convinced that the placenta was still *in utero*. I found it adherent there, and removed it by the hand. Of course an inquest was held on the child's body. No air had entered the lungs; indeed, from the circumstances, that was impossible; but in the stomach we found nearly an ounce of exactly the same kind of dirty fluid in which the body was immersed. The late Mr. Baker, the coroner for the district, ruled that the child was not born alive, because it had never breathed, and a verdict of still-birth was accordingly returned; this, too, in the face of the strongest proof that it was born alive, else none of the fluid which surrounded it could have passed into its stomach."*

We may cite also another similar case:—

"*Deglutition as a Sign of Life in Infants who have not Respired.*—Dr. Houzé d'Aulnot was consulted concerning a case in which an infant was found dead in a privy, and whose lungs sank on the application of the hydrostatic test; a portion of fæcal matter being found in the stomach, he instituted various experiments in elucidation of the subject, in order to prove whether the child was living or not when it was dropped into the privy. From these it results, in his opinion, that the presence of any foreign body, whether solid or fluid, in the digestive tube of a new-born infant, is a proof that deglutition has taken place; and deglutition being a vital process, is proof also that life existed at the time of its occurrence."†

Dr. Ramsbotham justly argues that the verdict of the coroner's jury was contrary to fact in a case of infanticide, while on a civil trial he lays down a dictum the very reverse, that nothing short of free respiration should be accepted as proof of live-birth. He would then insist upon the distinction between intra- and extra-uterine life, "between that life which is preserved to the fœtus by its parent's

* Lancet, May 25th, 1861.

† Moniteur des Sciences Méd., 1860, No. 114.

intervention, and that in which the child carries on its own independent existence." The writer of the present article formerly expressed the same opinion,* but has from enlarged clinical observation become convinced of its incorrectness. Such cases as the preceding quoted from Dr. Ramsbotham negative the conclusion. The daily experience of the obstetric practitioner still more forcibly tells against it. It must be impossible to determine the precise moment at which intra-uterine life ceases, and extra-uterine life commences.

In the 'Lancet,' May 18th, 1861, Mr. Hurd gives the particulars of a case in which the pulsation of the funis ceased between eight and ten minutes *before the head was born.* There were no signs of arterialization for full twenty minutes, and respiration was not established until three-quarters of an hour after birth. It cannot be doubted that this child had an independent existence, although it did not breathe during three-quarters of an hour.

Dr. Robert Lee puts the following question:—"If a child is born, but neither moves nor breathes, and shows no other sign of life except 'a slight pulsation at the cord' (funis), ought I to consider such a child to be born alive, and write a certificate accordingly?"† Having, in common with others, met with many instances of such suspension of the vital functions, followed at various intervals by their complete establishment, we should not hesitate to answer Dr. Lee's inquiry in the affirmative. To write a certificate of still-birth in such cases would, as has been observed by the editor of the 'Lancet' (May 18th), incur the risk of its negation by the subsequent breathing and life of the infant.

The frequent coincidence of the expulsion of the placenta with the birth of the child proves that intra-uterine dependence upon the parent must have ceased at some period before birth—in fact, as soon as the uterine contractions have separated the placenta from its uterine attachments, or interfered with its physiological relations.

The infant *in utero,* or, in legal phraseology, *in ventre sa mère,* is legally supposed to be born for certain purposes; thus, it may have an estate made over to it; it may have a guardian assigned to it; it may be made an executor. The law, then, which regards the fœtus as a living being capable of legal or civil functions, cannot consistently refuse to this same being the attribute of vitality, because, though at its birth it manifests the movements of a living being, it does not exhibit all the movements required for the maintenance of its life, of which respiration is one, but not the only proof. The condition of a child after birth and before respiration has taken place, is like that of drowned persons in whom life is suspended.

It has now been decided by British judges in civil cases, that a child may live and not breathe for some time after birth—the absence of signs of breathing being held to be no proof of its being born dead, as is the case in criminal proceedings. The importance of Sir J. Stuart's decision in the case of Brock *v.* Kellock is very great, as it must go far to settle the legal definition of live-birth upon the sound

* Legal Examiner, 1853, p. 65. † Lancet, May 18th.

basis of physiological science. It is certainly more consistent with physiology to take as proof of live-birth that one vital action, the beating of the heart, than to insist upon the act of respiration, which may be only deferred for some time after the expulsion of the child's body. "This decision," observes Dr. Taylor,* "is in accordance with law and common sense. Pulsations indicate action of the heart, as motions of the chest indicate an action of the intercostal muscles."

It is not within the scope of the present remarks to consider the proofs of live-birth demanded in criminal proceedings, but we may here briefly state that the English law requires proof of entire birth and respiration in order to sustain the charge of murder, and consequently violence done to a child during birth is not infanticide. The effect of this contradictory state of the law is to frightfully augment the crime of child murder. It is familiar to every practitioner of midwifery that a child may make successful efforts at respiration after its head is in the world, and while its trunk is in the vagina; in a few seconds or minutes, the body being wholly born, free respiration and crying are established. On the other hand, it as frequently happens, especially in instrumental labours, that an infant is wholly born, moves, and presents pulsations of its cord, yet does not breathe for some time. Surely, in the one case as in the other, the child is equally alive. Yet our criminal law would not regard the former as a live-born child; while our civil law would regard the latter as live-born even if it died before it respired. The wilful destruction of a living and breathing child before its entire birth is not by our laws contemplated as a crime: clearly our laws are not guided by physiological science.

Sir C. Locock has recorded the following case. On a Saturday evening a lady was in labour with her first child. The head and one arm were born two or three minutes before a neighbouring clock struck twelve. There was a cessation of pain for several minutes, during which time the child cried and breathed freely. The rest of the body was not born until five minutes after the same clock had struck twelve. The question arose, was this child born on Saturday or on Sunday? Dr. Locock justly gave it as his opinion that the child was born on Saturday. Physiologically speaking, the child lived an independent life before twelve o'clock on the Saturday, and the judge in a civil proceeding would at once concur in the correctness of the opinion; but the same judge, had the case involved a charge of murder in this interval, would have been obliged to declare that the child was not born alive because it was not wholly born into the world.

Having thus discussed the question of what constitutes live-birth according to our own laws and in the opinion of the most eminent physiologists and medical jurists of our own country, we proceed to subjoin a brief sketch of the laws of other countries on this point.

The laws of France require only that a child shall be declared viable in order that it may attain the rights of inheritance; "lorsqu'il est constant qu'un enfant est né vivant, il y a pour cela même presumption

* Medical Jurisprudence, seventh edition.

de viabilité, si l'enfant parait né à terme; si d'ailleurs rien dans sa conformation n'annonce qu'il ne puisse pas conserver vie."*

Devergie† explains the sense in which "*né vivant*" is understood, by quoting a report made to the *Tribunal* by Chabot (d'Allier): "Lorsqu'un enfant n'est pas né viable, il est aussi réfuté n'avoir jamais vecu, au moins pour la successibilité; en ce cas, c'est la même chose que l'enfant soit mort *ou qu'il naisse pour mourir*." Beek‡ states, on the authority of Capuron, that the interpretation put upon the words "being born alive" is, according to the most distinguished lawyers and physicians of France, *complete and perfect respiration;* but this, we should observe, is also Dr. Beek's own opinion, and is not confirmed by Devergie. We find that under the government of Postumus, a native of Gaul, in the reign of the Emperor Valerianus, about A.D. 250, the law required, in order to the rights of succession, that the child should be born perfect and at such a term that it should be capable of living. "*Non nasci idem est ac non posse vivere.* Non nasci et *natum mori, paria sunt.*"§ But proof of respiration is not here insisted upon.

The Roman law, upon which the above is based, emphatically distinguished between the *partus*, a live child, and the abortivus, or still-born. Thus Ulpian, as quoted by Mr. Tapping in the 'Legal Examiner and Medical Jurist,' March 19th, 1853: "*Tamdiu autem venter in possessionem esse debet quamdiu aut pariat, aut abortum faciat, aut certum sit non esse pregnantem.*" Here a specific value is attached to the verb *parere*, restricting its application to the *nati*, or born alive. In the Roman, as in English law, the *venter*, or unborn child, *in ventre sa mere*, is, for the purposes of testamentary succession, considered as already living; at the same time, if the evidence of life failed at birth, the *venter* could not *in strictum jus* take the *bonorum successio*, but was said to *vacare*, and the *bona* of the testator went into the *fiscus*.|| The *fiscus* being the treasury of the prince or emperor, as distinguished from the *Ærarium*, or public treasury. The Roman law, however, did not demand proof of respiration. "*Si vivus perfecté natus est, et si vocem non emisit.*"¶

The law of the United States follows that of England, in not requiring proof of crying or breathing, in order to establish live-birth, and accepts as sufficient evidence, proofs of motion, stirring, &c.

In Scotland the law distinctly requires not only that the child shall be wholly born, but that it shall have breathed, and also cried; in conformity with this law, a child which lived, breathed, and died in convulsions at the end of half-an-hour, was declared to have been born dead!**

The Prussian Statute Book requires proof, not only of respiration and crying, but of distinct voice. "*Dass ein Kind lebend zur Welt gekommen sei, ist in dieser Beziehung schön für ausgemittelt anzunehmen,*

* Code Napoléon, annoté de Sirey, édition par Gilbert, 1859, chap. ii. art. 57.
† Médecine Légale, tome i. p. 222. Paris, 1852.
‡ Med. Jurisp., vol. i. p. 353.
§ Zacchias, in Devergie, loc. cit.
|| Tapping, loc. cit.
¶ Chaussier, quoted by Beek, vol. i. p. 353.
** Dyer's Reports, 25.

wenn unverdächtige, bei der Geburt gegenwartig gewesene Zeugen die Stimme desselben deutlich vernommen haben." *

The ancient German law expressed the same conditions of live-birth in the formula of "*Beschreien der vier Wände*"—(*vox audita intra quatuor parietes domus.*) †

Casper, the distinguished medical jurist of Berlin, holds that breathing is to be held as synonymous with life—"not to have breathed is not to have lived." Respiration is held by Casper to be the only evidence of an existence independent of the mother. The life without breathing which daily experience incontestably proves possible, is regarded by Casper simply as apparent life; the mere spark of which may easily be extinguished by omission of efforts to save it. Exceptional instances of such *post-partum*, non-respiratory life, Casper does not regard as facts with which the medical jurist has to deal; he can recognise only life with respiration. The authority of Galen is cited by Casper: "*Respirationem a vita et vitam a respiratione separari non posse, adeo ut vivens omnino spiret et spirans omnino vivat.*" "Brief and clear!" (*kurz und clar!*) adds Casper, while he fortifies himself with the meaning of the word "*expirare*," which, if applied to the mother, is equivalent to "*moriri*," to die, to breathe out the last breath. Casper, however, has mentioned two instances of infants surviving, at their birth, a suspension of the respiratory functions, the one for seven, and the other for twenty-three hours. We could have added many examples of a like kind from medical journals, but deem it needless to overload these pages with illustrative cases. It may suffice to have shown that our own laws are now in unison with the laws of nature, and have at length come to be settled on the sound basis of science and common sense.

* Preussischer Gesetzbuch: quoted in Casper's Gerichtliche Medicin, Band i. s. 703.

† Casper, loc. cit.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON MICROLOGY.

By JOHN W. OGLE, M.A., M.D. Oxon, F.R.C.P.
Assistant-Physician to, and Lecturer on Pathology at, St. George's Hospital.

PART I.—PHYSIOLOGICAL MICROLOGY.

NERVOUS SYSTEM.

On the Structure of the Spinal Cord of the Petromyzon Fluviatilis (River Lamprey or Lampern). By Professor Reissner, of Dorpat.*—The author prefaces his paper by allusion to the observations made by Owsjannikow, in the case of certain fishes, upon the relation between the roots of the spinal nerves and the longitudinal fibres and the nerve cells, and upon the connexion between the latter.† The following are the results of Reissner's researches :—

1. That the colourlessness and transparency of the spinal cord of the petromyzon fluvialis is owing to the fact that the longitudinal fibres surrounding the grey substance possess no such nerve-medulla as the white fibres in the other vertebrata.

2. That there is neither an inferior nor a superior longitudinal fissure in the petromyzon; and instead of the latter, connective-tissue fibres from the grey mass surrounding the central canal proceed and form a separation into two lateral halves.

3. The grey mass corresponds in some measure (as to its boundary) to the form of the entire spinal cord, and like that, differs much in character from the cord of most vertebrata. It exhibits a broad band, which in the middle, surrounding the central canal, and being much thicker in its outer portion, is thinnest at the part between these two places. From the middle of the grey mass a broad process extends to the under surface of the spinal cord, producing a division into two lateral halves.

4. The central canal in the middle parts of the cord has an oval or elongated "lumen," whose largest diameter is perpendicular to the long axis of the cord; in the upper and lower end it has a circular circumference. It is always nearer to the under than the upper surface of the cord, especially at the posterior part of the cord.

5. In the central canal a band is constantly to be found very comparable to an axis cylinder.

6. The grey mass consists of a granular or striped matrix, in which lie the following four kinds of cells:

* Reichert and Du Bois-Reymond's Archiv, 1860, p. 545.

† Disquisitiones microscopicæ de medullæ spinalis textura, imprimis in piscibus factitatæ. Dorpati, Liv., 1854.

(*a*) The smallest ones, which exist everywhere, and optically coincide with the so-called connective-tissue corpuscles.

(*b*) Nerve cells, always of considerable size, and mostly possessed of strong processes; they are of three varieties:

(1) Large interior nerve cells, situated near the middle line in the upper surface of the grey mass; they are of the form of flattened balls, and send a process forwards and backwards, which in its further course turns somewhat outwards and upwards. Occasionally there is a third process which passes directly outwards. These have no connexion with the fibres of Müller in the middle parts of the spinal cord.

(2) Large outer nerve cells. These are of various forms, are mostly elongated, lie in the outer segment of the grey mass, and send out more processes, as many as six in some cases. The processes are the axis cylinders of the fibres, which pass out of the spinal cord as lower roots from the under commissure, and most probably pass directly into the longitudinal fibres.

(3) Small nerve cells. These are like the preceding ones in form: are more transparent, are less deeply coloured by carmine, and lie partly between the large nerve cells and partly more inwards than these. Their processes (much weaker) pass to the place of entrance of the upper root, and form radiating fibres to the posterior commissure.

7. Besides the nerve cells of the grey mass, others also in many places are to be seen between the longitudinal fibres. Those are most constant which are to be met with either near the outer edge of the cord, or between that and the outer extremity of the grey mass. They are chiefly placed transversely.

8. Neither the commissure fibres, nor the root fibres as a rule, extend to the nerve cells in quite a transverse direction.

9. The longitudinal fibres show the greatest differences as to breadth. The strongest, those so-called of Müller, are disposed in two groups, on each side one consisting of from six to eight fibres, along with the process of the grey mass which reaches from the central canal to the under surface of the spinal cord. The fibres of Müller (diminishing in diameter posteriorly) evidently traverse the whole length of the spinal cord, and form no connexion either with the nerve cells or nerve fibres. The axis cylinders of the so-called fibres of Müller appear, on transverse section, very irregular, much flattened, crescentic-shaped, and only occasionally fill up the lumen of the fibres.

10. The distinction between the nerve fibres of the spinal cord and the fibres of the peripheric nerves cannot be established, because the primitive sheath of the latter is not less an accessory structure than the investment of connective tissue belonging to the former. The spinal cord of the petromyzon fluviatilis possesses but few bloodvessels, but they are very numerous in the pia mater.

EPIDERMAL SYSTEM.

On a Peculiarity of Development of the Superficial Epidermic Cells in the Fœtus. By Dr. C. Robin.*—The cells composing the most superficial layer of the epidermis in the fœtus are remarkable for their considerable and often uniform size, and their regular, generally hexagonal or pentagonal figure. They are delicate, transparent, and wanting in molecular granulations in proportion to the advanced age of the fœtus: at last they are quite free from granulations. These cells possess a large nucleus, ten or twelve thousandths of a millimetre in size; the nucleus is spherical or ovoïd, being ovoïd with one or two nucleoli in young embryos, and gradually becoming spherical in the case of

* Brown-Séquard's Journal de la Physiologie, April, 1860, p. 228.

most cells losing their nucleolus. Whilst these changes are going on, the nucleus is increasing in size.

Towards the end of the second or beginning of the third month, the nucleus, which has increased as well in length as in thickness, begins to burst out in a remarkable way at the free or cutaneous surface of the cell, and becomes at the same time rounded, retaining, however, its outline. Having become free to a considerable extent, the nucleus becomes quickly developed, so that towards the middle of the third month it attains a size of twenty or twenty-five-thousandths of a millimetre, acquiring thus, in the hands and feet, a size equal about to two-thirds of the cell itself, so that seen in front they appear almost to touch at their edges. In the other parts of the body their length is only half that of the cell. Their breadth becomes at least equal to the half of their length.

In examining fœtuses of different ages, or different parts of the same fœtus, it is seen that at the same time that the nucleus increases at its projecting part and becomes free at the surface of the body, the part adhering to the cell becomes narrowed so that it becomes really pediculated and moveable upon this fixed point. About the fifth or sixth month this pedicle of the nucleus has become so fragile that the necessary manipulations separate a large number of them, exposing at the point where they had been adherent a small, pale, rough, circular or oval spot, of from three- to five-thousandths of a millimetre in size. About the seventh month the nuclei fall off spontaneously, so that at the eighth month of intra-uterine life the most superficial epidermic cells are quite without nuclei. The epidermic cells which appear "*after birth*," and which are without nuclei, lose them by gradual atrophy, owing to the production of subjacent cells.

In the fœtus of from two and a half to five months old, when the nuclei are at their largest size, they are disposed as follows: they exist over the whole body as far as the level of the skin with the tissue of the umbilical cord, where they cease. They also cease at the edge of the lips and sphincter of the anus, and they are absent in the vagina and on the clitoris, which are covered with large nucleated pavement epithelium. Some are, however, found on the epidermis of the labia majora. In the epidermis on the soles of the foot and palms of the hands viewed frontways, the nuclei are seen hypertrophied and bursting from the cells, as is the case with other parts of the body at this age, but later on, the hypertrophied and out-bursting nuclei are to a certain degree dispersed over the epidermis. The cells of the superficial layer are very delicate, very transparent at their edges, well-defined and pale, and void of granulations; while the subjacent ones are less, thicker, fairly granular, and provided with an ovoïd nucleus.

Viewed frontways, the hypertrophied elevated nuclei are sometimes sinuous and indented, sometimes divided as it were into lobes by indentations. Viewed sideways, they may be seen flattened on the side next to the cell to which they are adherent, and are spherical, ovoïd, pyriform, &c. At the soles and palms they, like the cells, are smaller than at other parts of the body, each one covering almost the whole of the corresponding cell.

Over other parts of the body the cells are larger and more regular, though the nuclei are not larger than those at the soles and palms; being on the scalp the largest, where also the nuclei are somewhat larger than elsewhere. At that part also they are more lobulated, giving at times, when five or six in number, the general appearance of a mulberry.

Some of the nuclei, especially from the scalp, but elsewhere also, are uniformly and finely granular, and some enclose a few small fatty granulations of a yellow colour. Most have a clear regular outline, and are transparent and almost quite homogeneous; enclosing only fine granulations accumulated with ill-defined edge at the narrow part adherent to the cell.

Acetic acid renders them slightly pale, swells them up, and makes their surface a little lobulated. Sulphuric acid renders them extremely pale, and gradually dissolves them from their surface towards their point of adherence.

Potash swells up the epidermal cells (rendering them very pale and transparent). It dissolves tolerably quickly the nucleus of the cells, but it only renders very pale the hypertrophied and pediculated nuclei of the superficial layer without swelling them up; on the contrary, it makes some of them smaller, and at the beginning of its action renders their centres thicker.

CONNECTIVE TISSUE, TENDON, &C.

On Connective Tissue. By S. Martyn, M.D.*—The author in this communication briefly epitomizes the various views formerly and recently put forth, mainly in Germany, by observers respecting the morphology of the so-called "Bindegewebe" (connective tissue), or areolar tissue, as it is more commonly termed in England. In doing so he takes occasion, by way of comment, to show the points in which his own observations lead him to agree or differ. Of these, perhaps the most important is as to the existence or non-existence of certain stellate forms or appearances (sternformiger Bindegewebskörper, as they have been termed) which are seen on "transverse" section only of portions of tendon. These forms, with their radii, have been looked upon by Virchow, and subsequently by Kölliker, as true cells, with communicating or anastomosing branches, whose function is that of forming a machinery for irrigating (or the conveyance of nutritive fluid among) parts not supplied with bloodvessels, an "intermediate juice-channel system" (Saftröhrensystem). Again, Henle not being able to find fibres connected with these cells (as he supposes the forms in question to be), looks upon them as "vestiges of fœtal organs, destined at most to preserve interstices." Dr. Martyn looks upon these stellate anastomosing structures as being nothing of the kind as cells, but simply as being "interstitial places formed by the junction of two, three, or more bundles of the extra-cellular substance." He observes that Virchow is correct as to the existence of what he (Virchow) calls processes from the cells, for there are such in the longitudinal direction, but that they are not really processes, as they "*are often seen without either a cell or fibre in the centre; they retain their forms, as was shown by Henle, if the preparation be viewed from the other side; they disappear under the influence of acids, and they are not seen in a longitudinal section. There is no other possible explanation of their absence (on longitudinal section) than that they are planes, which in the longitudinal section cannot obstruct light enough to be seen, doing so, however, when cut across.*"

Concerning the development of connective tissue, the author says:

"I hold that at the earliest period what is to become connective tissue consists of embryonic cells in contact with each other, and that the next change is a division, in longitudinal directions, of the nucleus; at the same time, a deposit takes place on the outside, and probably only on *one* (the nuclear?) side of the dividing cell. This is the extra-cellular substance, the outer or secondary cell-wall, corresponding to the cellulose wall of vegetable cells. This deposit is made in layers, and is striated accordingly; extending then chiefly in a longitudinal direction, it carries with it the nucleated centres of its generating cell. Thus do these centres become separated from each other, and the intermediate cell is reduced to a delicate thread. Some of these cell networks become transformed into thicker and stronger bands, and so on ultimately into elastic tissue; others remain, elasticity not being much required, in their primitive state. If at the first appearance of extra-cellular deposit the tissue be torn up for examination, the 'matrix' will separate into longitudinal, fibrous-looking masses, more or less fusiform, each having its generating cell attached. This is the scheme

* Beale's Archives of Medicine, No. 6, p. 99.

which Schwann figured, and to which Kölliker accedes to the extent of the cellular nature of the fusiform masses. But from every point of view, even the drawings of Schwann themselves, I am led to believe that there is no including membrane, and that these must be considered as true fragments, although genetic unities, of what I have called 'extra-cellular' deposit. As the metamorphosis proceeds, the long wavy bands result in various directions, between and around which wander the elastic fibres at very acute angles; so that if any one bundle be expanded violently by acetic acid, the well-known spiral constructions are produced by what have then become nearly transverse ligatures. Much unnecessary confusion has been introduced in explaining this simple phenomenon, a very elaborate disquisition of which, by Dr. Klopsch, may be found in Müller's 'Archives' for 1857 (p. 417–35). His conclusions do not stand the test of further investigation. We have already seen Virchow's and Kölliker's view to be that the cell-processes form a system of channels for nutritive fluid (plasm cells). Now, as I have already shown the stellate forms to be interfascicular, it remains to inquire whether the elastic fibres can be tubes for the above purpose.

"Virchow believes that they are so;* and indeed some of the most important propositions of cellular pathology hinge on this idea of a permeable cell network in connective substances. But though all prolongations of cells must be conceived of as hollow at first, I have been unable to convince myself that the elastic fibre retains that character; in fact, very thin sections of tendon moistened with glycerine, and viewed with high powers, show the cut ends of the fibres with no trace of a central canal or point of a medullary kind. It seems to me rather rash to assume the persistence of a tubular character in fibres of the finest kind, when we cannot demonstrate it in coarser ones. The only approach to a proof of such a structure is that in injections of cornea and tendon with indigo, made by Willich, the blue particles were found to have permeated the fusiform elements of a longitudinal section; but it should be remembered that if there be a space between the bundles in which the cell and fibres lie, the colouring matter must accumulate in contact with them; in fact, the investigation requires confirmation with extraordinary care before we can give our adhesion to any conclusions. There is no *à priori* reason for seeking a system of nutritive canals in the tissue under consideration; certainly no more than in cartilage, where we know that nutritive matter must pass through the intervening solid substance from bloodvessel to cell, or from cell to cell."

The author concludes his communication by strictures on the prevailing plan (set forth by Reichert) of classing under the common head of *connective substances*, all tissues "which are formed from stellate or fusiform modifications of embryonic cells, as for instance, the capillaries and terminal nerve plexuses.

PART II.—PATHOLOGICAL MICROLOGY.

VASCULAR SYSTEM.

Primary Softening of the Heart. By E. Wagner†.—The case was that of a child, sixteen days old, whose mother had died after childbirth. Emphysema of the right lung and sanguineo-purulent exudation in the left pleural cavity were found. The heart was very flabby, and the wall of its left ventricle was quite soft and pappy. The microscope showed fragments of muscles from about $\frac{1}{60}$th to $\frac{1}{10}$th of a line long, void of any cross-striping, with one, two, or no nuclei. The sarcolemma was thickened, as if œdematous, but otherwise

* Cellular Pathologie, p. 94.

† Archiv der Heilkunde, i. 2, 1860: quoted from the Vierteljahrsschrift f. d. prakt. Heilk., 1861, xviii. Analekten, p. 39.

normal; and the ends of the muscle fragments were irregular, pointed, obtuse, or indented, many being very opaque. Free nuclei, albuminous molecules, and some longitudinally-striped portions of muscle were also seen. Transversely-striped muscle and fat globules did not exist. There was no indication of fatty metamorphosis or of putrefaction.

New Formation of Lymphatic Elements in the Connective-tissue of the Pleura and Lungs. By Prof. Wagner.*—This was observed in the body of a woman, aged twenty-two years, who died in childbirth in her first confinement. Bands like lymphatic vessels were found in the pleura of the lower lobe of the right lung, which was adherent to the opposed thoracic pleura. On section of the lung a greyish-yellow thick fluid escaped, showing under the microscope many round granulated cells, containing single and sometimes double nuclei. The bands had apparently no special walls, and were mostly regularly cylindrical or partly knotted and divided by transverse complete or incomplete septa into partitions of from $\frac{1}{4}$th to $\frac{1}{3}$rd of a line in length. Along with these large lymphatic spaces smaller ones existed, showing every transition into connective-tissue corpuscles, having a thin homogeneous membrane. The smallest were cylindrical or stellate, filled with minute nuclei, and having processes communicating with other cells. The arrangement of these elements in the pleura was such that superficially there was a thin layer of fibrin without vessels or corpuscles, under which appeared a layer of about $\frac{1}{15}$th of a line thick, containing but few vessels and slightly developed connective-tissue cells, as also numerous elastic fibres. Under this again, existed numerous dilated capillaries in whose meshes was a layer (about $\frac{1}{10}$th of a line thick) of connective-tissue bundles, with numerous, evidently cellular connective-tissue corpuscles, most being stellate, and filled with one or two nuclei, or cylindrical, containing a row of from four to eight nuclei.

This observation is adduced by the author as illustrating the connexion between lymphatic vessels and connective-tissue corpuscles; inasmuch as the latter form in their cavities colourless cells and fibrin, and may expand into large spaces like the lymphatic alveoli.

Fatty Degeneration of the Umbilical Vein.—Dr. Alexander Simpson† has described to the Edinburgh Obstetrical Society an interesting case, in which the umbilical vein had the appearance of being affected with atheromatous degeneration of its inner coat. Microscopical examination, however, showed that this appearance was owing to the fibres of the muscular coat having undergone fatty change. The specimen was from the body of a child born at the end of the fifth month. The placenta was quite healthy, as appeared also the other parts of the body.

DIGESTIVE ORGANS.

Peculiar Polypous Vegetation of the Mucous Membrane of the general Large Intestine. By Luschka.*—In this case, not only the rectum but also the entire colon was beset with the structures in question. The patient was a woman, aged thirty, who for many years had at times been subject to diarrhœa attended by bloody stools. This eventually produced such complete anæmia that she died of collapse. After death the polypous growths before alluded to were seen to be very vascular, easily bleeding, and beset the entire inner surface of the large bowel. Most were red, some were of a greyish colour, and some contained dark pigment: they were soft, and on section gave out a slimy substance. They varied in size from a mere slight elevation to bodies of several

* Archiv f. Phys. Heilkunde, iii. 3.
† Edinburgh Medical Journal, No. lxvi. p. 573.
‡ Virchow's Archiv, Band xx., Hefte 1, 2, p. 133.

centimetres in length; mostly being pedunculated; the free end being clavate, occasionally incurved, and lobulated. Moreover, diffuse tumour-like bodies also existed here and there. The surface of the elevated mucous membrane exhibited under a lens numerous round points as if produced by a needle, which turned out to be the openings of pouch-like glands; and even with the naked eye on the surface of most of the polypi a kind of fissuring was visible, which under the microscope proved to be owing to projections of the interstitial areolar-tissue-substance. These structures mostly possessed cylindrical epithelium, of which the cells were very long and clear, and less acted upon by water than those of ordinary intestinal epithelium. The parenchyma of these tumours was a finely-striped, partly granular basis, containing great numbers of naked rounded nuclei and smaller and larger cells, of which many contained two nuclei with obvious nucleoli. The interstitial mass was thickest in the centre of the polypi, from which strong processes passed to the periphery dividing and supporting the various elements, which consisted of numerous bloodvessels arranged in a network and in loops, with difficulty isolated; and also of glandular structures in very great abundance, and possessed of true cylindrical epithelium. This epithelium was often so excessive that the glandular cavity, containing molecular matter, nuclei, and roundish cells, was very small. No basement-membrane was to be found beneath the epithelium.

On preparing fresh portions of these polypi by boiling in dilute sulphuric acid, then drying and making thin sections, and then moistening with dilute chromic acid, the irregularities of the surface of the polypus were found to be owing, on the one hand, to depressions corresponding to the openings of glands and to indentations; and on the other hand, to elevations consisting of areolar-tissue-material beset with cells and nuclei between the glands, and passing into minute papillary excrescences. The glands, which were chiefly predominant at the periphery of the polypi, were partly simple pouches like those of Lieberkühn but larger, and partly a number of segments exhibiting out-bulgings and even true ramifications. The author supposes the growths in question to be glandular polypi, exhibiting simple circumscribed hypertrophy of the mucous membrane, partial growths of the interstitial areolar-tissue next the submucous tissue having taken place as a result of an increase of cells. He mentions another case which came under his own observation, in which pedunculated polypi of the large intestine contained cyst-like cavities filled with slimy material. He also alludes to specimens of a somewhat but not exactly similar nature, described by Rokitansky,* as observed after death by chronic dysentery.

GLANDULAR SYSTEM.

The Lobules of a Healthy Liver compared with those of a Cirrhosed Liver.—In comparing two specimens of healthy and cirrhosed liver, Beale makes the following remarks: "The section of the human liver magnified 215 diameters shows the capillaries injected blue, and the cell-containing network alternating with them in all parts of the lobule. Both the capillaries and the tubes containing the cells have distinct walls. Another section of healthy liver under a power of forty diameters shows the following points: In this specimen the portal vein was injected with carmine, and the hepatic vein with Prussian blue. The capillaries of the lobule are filled with the colouring matter—those in the centre of each lobule being *blue*, while those at the circumference are *red*. Observe how *very narrow the interlobular fissures are*, and how in many places the capillaries of one lobule are continuous with those of adjacent lobules. The interlobular spaces are clearly destitute of any areolar or fibrous tissue. They are occupied by the branches of the portal vein which you see, and

* Handbuch der spec. Path. Anat., 1842, Band ii. s. 263.

branches of the artery and duct, and lymphatics, which have not been injected in this specimen.

"Let this preparation be compared with the cirrhose liver in which the vessels have been also injected. What a wide space exists between the contiguous lobules, of which but very little, and only of the *central part of the lobule*, remains in many cases. Vessels and tubes, which may be seen more distinctly by using higher powers, are observed in the substance of the tissue usually stated to be *fibrous*.

"In another specimen of a cirrhose liver, magnified 130, which has been soaked in carmine, the shrivelled cells within the narrowed tubes, and the network, can be seen so distinctly, that you will hardly fail to wonder how it has happened that the nature of this so-called fibrous tissue had not been made out long since; but many of the most delicate and beautiful textures appear fibrous enough when placed in water and roughly examined; and thus it has been supposed that morbid changes have originated in a comparatively passive structure, the areolar or connective-tissue.

"A section from the same liver has been put up in water, and not a vestige of anything but 'fibrous tissue' is to be seen where it has been shown that numerous tubes and cells and vessels can actually be demonstrated. By immersing a delicate preparation in water, the appearance of the presence of a large quantity of fibrous or connective-tissue can often be produced.

"Cirrhosis is not the result of inflammation of Glisson's capsule, which structure does not exist around the lobules of the human liver, but it arises from a degeneration in the secreting cells, and wasting of the entire lobule may result. This wasting always begins at the *circumference of the lobule*, and spreads towards the centre; the lobules become absolutely smaller; the interlobular spaces appear to become wider, but the greater width depends merely upon the *lobular structure* having become so altered as to resemble the appearance produced by the tissues usually occupying the space or interlobular fissure. The lobules gradually shrink, many cease entirely to secrete bile, the little bile formed is often acid, the quantity of blood distributed to the lobule becomes less and less, the whole liver shrinks and becomes condensed, and gradually ceases to perform its functions."—*Lectures at the Royal College of Physicians of London.*

Disease of the Pancreas; from a Case of "Sick Headache." By C. Hooper.*—The patient, aged forty-four, fat, florid, and bloated in appearance, had suffered from so-called bilious attacks for some years, and had been subject to vomiting of yellowish and greenish fluids, chiefly at night. He was one day, after eating a heavy beef supper on the night previously, seized with severe pain below the left breast, and sickness. There were constipation and tympanitis, and the pulse was very weak. The pain and vomiting continued in spite of treatment, and collapse preceded death, which occurred on the second day after beginning of attack. On post-mortem examination much fat was met with in the omentum and folds of peritoneum; the small intestine was rather of a pink colour, and the pyloric end of stomach congested; the liver was large and fatty, and the kidneys flabby but small; the pancreas was hard but brittle, and throughout its substance were several dark red patches, apparently small extravasations of blood. These were found to be situated "in the follicles" of the gland. In many instances the epithelium was of a dark brown colour. In other follicles very small dark black masses, apparently owing to changes in small clots of blood being extravasated, were seen. Several of the follicles were quite disorganized, and their place occupied by fatty globules; and many small granular cells existed in the intervals between

* Beale's Archives, No. 8, p. 282.

adjacent follicles. Most likely all this was owing to capillary hæmorrhage, the result of long-standing congestion; just as we have blood extravasated within the follicles in the case of the gastric glands of the stomach, Lieberkühn's glands, uriniferous glands, &c. "The blood had afterwards undergone change, partly from the action of the secretion of the gland upon it, but principally from its remaining for some time in contact with living tissue."

MISCELLANEOUS.

On the Different Microscopical Characters of the Secretions from Infecting and Non-infecting Syphilitic Sores. By H. Lee, Senior Surgeon to the Lock Hospital and Asylums, &c.*—The following is an abstract of Mr. Lee's views of the differential characters of the two classes of syphilitic sores. From this it would appear that a local affection which will produce secondary symptoms may be distinguished from one which will not, as well by the nature of its secretion and by the inoculability of that secretion upon the same person, as by the induration which by some surgeons has been considered as always characteristic.

Mr. Henry Lee says, that "the object of the investigation was to ascertain whether the secretions from the surface of syphilitic sores afforded any characters by which the nature of those sores might be recognised, and especially whether those secretions would furnish a test of the liability of the sores to be propagated by contagion to another part of the same patient's body on the one hand, or of their power of infecting the patients' constitutions on the other.

"It became very soon apparent that sores which were the most contagious (*i.e.*, liable to be communicated by contact, or by artificial inoculation to another part of the same patient's body) were the least liable to be infectious (*i.e.*, were the least liable to infect a patient's constitution); and the converse of this was proved to be true—namely, that those sores which were likely to be followed by constitutional infection or secondary symptoms, were not, as a rule, contagious as far as the patients themselves were concerned. It was found that the local contagious disease could be inoculated upon the same patient almost an indefinite number of times; but that the infecting disease, when it had once developed its morbid process, could not again, as a rule, be reproduced, either by contact or by artificial inoculation, upon the same patient. Upon a patient who had not previously had syphilis it would doubtless be inoculated readily enough; but from the nature of the case any experiments of this kind were not instituted.

"In examining the secretions under the microscope of these two classes of cases, those from the contagious local sores were, as a rule, very much of the same nature. They always presented well-formed pus globules, so long as their characteristic contagious nature remained; and if a little acetic acid was added to the secretion before it was examined, the distinct nuclei of the pus globules were rendered visible. These nuclei were generally of the same shape and size; and one, two, or three of them might be seen in the space occupied by each pus globule.

"The secretions from the second class of syphilitic sores—namely, those which were not again inoculable upon the same patient, but which were, as a rule, followed by secondary or constitutional symptoms, varied very much more than those of the first class above described. They consisted generally of epithelial débris floating in a transparent fluid; or of globules of various shapes and sizes, often matted together. Upon the addition of acetic acid nuclei were sometimes seen to occupy the interior of the globules; but these were again of different sizes and shapes, and of varying degrees of opacity. When care was taken to obtain nothing but the natural secretion from these sores, the well-formed pus globules (with their characteristic nuclei upon the addition of acetic

* Beale's Archives, No. 8, 1861, p. 277.

acid) were absent. In some cases it was found difficult to obtain the secretion from the sore unmixed with that of the neighbouring inflamed mucous membrane; in others, the nature of the secretion had been altered by the application of caustics; in others again, the part had become irritated by the prolonged detention of diseased secretions upon its surface, or by mechanical means. In all these instances it was found necessary to distinguish between the natural secretion of the part and that which was produced by the accidental irritation. In order to effect this it was generally found sufficient to apply water dressing to the sores for two or three days. The accidental irritation had then usually subsided, and the natural secretion would be obtained unmixed with other fluids. When thus examined, it was found that the secretion of the sores which could not be inoculated again upon the patients who had them, but which, as a rule, produced secondary symptoms in those patients, did not contain well-formed pus globules; and that when globules were present in the secretions, they did not, upon the addition of acetic acid, yield the characteristic nuclei of pus. A clear diagnostic sign was then here established between those sores which were in their nature likely to infect a patient's constitution, and those which were not. The first were found to be suppurating local affections, requiring only local treatment; the latter were found to depend upon the adhesive form of inflammation, and to require mercurial and other constitutional treatment.

"The mode of distinguishing the different kinds of primary syphilitic sores now described, affords in practice most important and valuable information. By this means a chancre which, if left to itself, would infect the patient's constitution, may be distinguished from one which will not, even in cases where the ordinary mode of diagnosis fails. Thus, out of a hundred cases that were noted and registered as presenting, upon microscopic examination of the secretion, the characters of suppurating sores, in two only, as far as I could ascertain, did any constitutional symptoms follow the local affection; and even in these two cases it was satisfactorily made out on further inquiry, that the patients had exposed themselves to more than one source of disease. The contagious suppurating sore in these two cases may, therefore, in reality, have been superadded to, and have masked the non-suppurating infecting sore. On the other hand, ninety-five cases were recorded in which the secretion was not purulent, according to the test above mentioned, and these presented the ordinary characters of the primary indurated chancre.

"Many of these cases individually were lost sight of after they left the hospital, but during the same period seventy-three cases were noted as having presented constitutional symptoms, where there was evidence of the primary disease having been accompanied by the adhesive, and not by the suppurative inflammation."

On the Endogenous Origin of Pus and Mucus Cells.—Remak,* after touching upon the endogenous cell-formation in the embryo, passes on to a brief mention of his own observations upon the division of cells in tumours, those of Virchow on cell-formation in the brood-spaces, and those of His and Weber on the formation of pus-cells in areolar-tissue corpuscles. He then particularly instances the statements by Bühl† of Munich, upon the endogenous pus-cell-formation in enlarged epithelial cells of hepatized lung; and relates the following descriptions in confirmation of his observations. He was called to see an old woman who had suffered for some time from retention of urine and convulsive fits, owing to urea-poisoning. Subsequently the urine was freely passed, but it was very turbid, and contained a number of mucus-cells, and a large collection of cyst-like cells from one-eightieth to one-fiftieth of a line, which, by their form and size, and the chemical characters of their parietes, were

* Virchow's Archiv, Band Hefte 1–2, p. 198. † Virchow's Archiv, Band xvi. 168.

evidently epithelial cells from the base of the bladder and ureters. These cells contained, besides the embedded nucleus, a number (from six to fifteen) of small cells like mucus cells. Later on, the urine contained less and less of the mother cells. Some months previously, the urine of a young man, full of albumen and fibrinous casts, was found, when examined fresh, to contain, besides blood and pus cells, a certain number of large cells very like those above described, containing from four to eight small granulated cells without any visible nuclei. From these facts, Remak infers that pus-cells may originate just as much in epithelial cells as in areolar-tissue cells, and that also mucus-cells in blennorrhagia, even if they possess no purulent character, form inside epithelial cells. The author allows that all mucus cells do not originate in this way. For example, in the rabbit lately fed, whilst the duodenum is yet empty, cylindrical granular cells fall off when cell-membrane becomes elevated, in which the granular contents collect together, giving the cell a round form and the actual appearance of a mucus cell. He suggests that it is not unlikely that the small cells of the saliva, whose granular contents perform a molecular movement, have a similar method of origin.

The following communications, bearing on the subject of this Report, but for the analysis of which we have no space, may be mentioned as of interest.

On the Structure of Tissues. Observations on their Growth, Nutrition, and Decay. By Dr. Beale. (Beale's Archives, No. vii. p. 179.)

Examination of the Urine and Kidney of a Case of Suppurative Nephritis. By Dr. Beale. (Ditto, No. viii. p. 286.)

On the Histology of Muscular Fibre. By Steffan, Welcker, Sczelkow, Margo, Weismann, Schultze, &c. (See Schmidt's Jahrbücher, 1861, No. vii. p. 8.)

New Formation of Uterine Glands in Fibrous Polypi of the Uterus and Ovaries. By Rokitansky. (Zeitschr. d. Wien, Arzte 1860.)

On Fatty Degeneration of the Retina. By Nagel. (Gräfe's Archiv, vol. ii.)

New Observations on the Intimate Structures of Diseased Lymphatic Glands. By Dr. Billroth. (Virchow's Archiv, Bd. xxi. Heft 4, p. 425.)

On the Trichina of Muscle and Urinary Sarcinæ. By H. Welcker. (Ditto, p. 453.)

On the Anatomy of the Spleen. By Axel Key of Stockholm. (Virchow's Archiv, Bd. xxi. Hefte 5 und 6, p. 568.)

On the Intervening Tissues of the Liver. By J. Engel-Reimers. (Explorationes Microscopicæ de Telu Hepatis Conjunctiva. Dissert. Inaug., 1860. Ditto, p. 585.)

Two Cases of Acute Miliary Carcinoma. By Dr. J. Erichsen, of St. Petersburg. (Virchow's Archiv, Bd. xxi. Hefte 5 und 6, p. 465.)

On the Formation of Pus in the Anterior Chamber of the Eye. By Prof. Jemge, of St. Petersburg. (Virchow's Archiv, Bd. xxii. Hefte 1 und 2, p. 193.)

On the Ciliated Epithelium of the Intestines of Birds. By Eberth. (Siebold and Kölliker's Zeitschr. f. w. Zool., B. x. s. 373.)

On the Structure and Formation of Fat-Cells. By C. Lenk. (Dissert. Inaug. Leipzig, 1859.)

On the Molecular Structure of Animal Tissues. By W. Müller. (Henle und Pfeuffer's Zeitschr., Reihe iii. Bd. x. s. 172.)

On Medullary Cells in the Diaphyses of Cylindrical Bones. By Luschka. (Würzburg. Verhandl. Bd. x. s. 175.)

The Histology of the Cortex of the Brain. By Stephany. (Dissert. Inaug. Dorpat, 1860.)

On the Minute Anatomy of the Spinal Cord. By F. Goll. (Med.-Chir. Gesellsch. d. Kantons Zürich, 1860.)

On the Nervous Ganglion of the Excretory Ducts of Glands. By Dr. W. Manz. (Verhandl. d. u. Gesch. z. Freiburg, Bd. ii. p. 163.)

On the Regeneration of Transplanted Nerves. By Philipeaux and Vulpian. (Académie des Sciences, April 29.)

On the Olfactory Bulb and the Olfactory Mucous Membrane. By J. Lockhart Clarke. (Reprint from the Zeitschr. f. wiss. Zool., Bd. xi. Heft 1.)

[We hope to give an analysis of this paper in our next Micrological Rep.]

On the Structure of the Hair Follicles of the Scalp. By P. Chapuis and J. Moleschott. (Moleschott's Untersuch., Bd. vii. s. 325.)

On the Molecular Theory of Organization. By Prof. Bennett. (Proceedings of Royal Society of Edinburgh.)

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HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

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I. Toxicology.

Poisoning by Croton Oil.—A. J——, a chemist, forty-three years of age, took by mistake, on the 28th of January last, about half an ounce of impure croton oil, intending to have taken cod-liver oil. He immediately felt a burning sensation in his throat and stomach, followed soon afterwards by vomiting, and then by alvine evacuations, so abundant that they numbered nearly one hundred. The vomitings were kept up by the administration of cod-liver oil and lukewarm milk. He was taken to the General Hospital of Prague, when he presented the following symptoms: The skin was cold; the face, hands, and toes were slightly cyanosed; the pupils were equally dilated; the external senses were intact; the cutaneous sensibility was normal; the patient expressed a sense of piercing cold, and faintness and debility were very decided. The fauces were red; the tongue was slightly loaded; the pulse was small and weak; the breathing was painful and slow, being reduced to twelve respirations per minute; the heart's action was weak, but of natural rhythm; the stomach was distended, and sensitive to pressure, and the vomiting and diarrhœa continued. An emulsion with cherry laurel water, and with cow's milk and emulsion of almonds for a drink, were prescribed; cold fomentations were applied to the head, and lavements of ice water containing opium were administered. Under this treatment the patient became convalescent without any other peculiar sign. The recovery was perfect on the 12th of February.—*Revue de Thérapeutique Médico-Chirurgicale*, Mai, 1861.

Poisoning by Croton Oil: recovery.—A second case of poisoning by croton oil is given by Dr. James Brydon of Hawick. Janet W——, a young woman aged nineteen, suffering from phthisis, was using an embrocation composed of equal parts of croton and olive oils. On the 6th of May, at 9 P.M., Dr. Brydon was called to this patient in consequence of her having swallowed by mistake a teaspoonful of the liniment instead of a dose of mixture. He saw her about half an hour after the accident had occurred. She felt no pain in the stomach, but complained of an intense burning sensation in the throat and all down the course of the œsophagus. The pulse was 84, and feeble, or nearly the same as it had been in the morning, and the temperature of the surface was not altered. A free quantity of warm water was at once administered, and a full emetic dose of sulphate of zinc was sent for. Before the zinc was procured she had commenced to vomit; but to make sure the zinc was administered. The vomiting was now continued and severe, and was further encouraged by additional large quantities of tepid water. The ejecta contained, in addition to the water swallowed, a considerable amount of bile, and also a little of the remains of a scanty meal she had taken three hours before; they tasted strongly of croton oil. After the vomiting had continued for a quarter of an hour, she began to complain of severe pain in the stomach, not aggravated, however, by pressure. She was ordered an enema, consisting of thirty drops of tincture of opium in a tablespoonful of starch, with directions to have it repeated in an hour. At half-past eleven—the bowels having acted freely about ten—Dr. Brydon found that the vomiting had ceased and the pain had considerably abated in the stomach, but was now very severe at the lower part of the bowels; at neither place was it increased by pressure. On the 7th of May the bowels had not been moved again; the pain had entirely ceased, and she had slept a little, although not soundly; the tongue was furred and dry; there was considerable thirst, and a bad taste in the mouth. In a day or two she was quite as well as before the accident, and the bowels, which had formerly been costive, again required purgative medicine. Dr. Brydon adds that he knows of only two other recorded cases of a similar kind. One was by Orfila in the 'Gazette des Hôpitaux,' 1839; the other in the 'Medical Gazette,' vol. xliii. p. 41. Both cases ended fatally.—*Edinburgh Medical Journal*, August, 1861.

Case of Poisoning by Belladonna.—Dr. W. H. Butler, of Saginaw City, Massachusetts, records the following example of narcotic poisoning by belladonna. The case occurred in the practice of Dr. J. B. White. On October 31st, Dr. White was called to see J. P——, aged thirty-five, who had presented the following symptoms:—Just before the doctor was called, the patient had aroused his sleeping companion by rapidly rolling over and over in the bed, throwing his arms about, &c. This person, thinking it a case of nightmare, shook him severely for the purpose of rousing him. This only created a disposition for resistance for a few minutes; and becoming alarmed, help was obtained, when the doctor was sent for, who learned the following history:—The patient had purchased a bottle of fluid extract of taraxacum of a druggist, and on retiring about eleven o'clock, had taken several tastes from the bottle, altogether probably over a drachm. The doctor arrived at two o'clock, when he found the patient presenting the following symptoms: Pupil unusually dilated; eye nearly or quite insensible to light; conjunctiva congested; face slightly flushed, with some increase of heat; extremities cool; no perspiration; stertorous breathing, with occasional yawnings; convulsive twitchings all over the body; mouth dry; difficulty of deglutition; some mutterings, but no articular words; voice husky; coma complete; and the pulse irregular, varying both in fulness and frequency from 75 to 130 per minute, but most of the time feeble. Dr. W. remained with him from two to four A.M. Treatment: cold to head, sinapisms to the feet, and antispasmodics. When the doctor left, the coma and much of the spasmodic action had subsided. At eight he saw him again; delirium more active; pulse 80, and feebler; would occasionally spit a little thick white saliva; picked at bed-clothes; made efforts to get out of bed, and when up being unable to stand, would stagger like a drunken man; had spells of laughing; wanted to whisper to the attendants, as if he had some confidential matter, but said nothing coherently; occasionally placed his hand to his throat, as if there was oppression there; pupils less dilated; conjunctiva less congested. All the symptoms gradually improved by night, when he first seemed to be cognizant of faces and names. At six P.M. Dr. W. gave a cathartic (patient was unable to swallow much before). From this time recovery was gradual and marked. The second day he was in active delirium, precisely like delirium tremens. It was four or five days before the pupils of the eyes recovered their normal size.

The patient was a temperate man. On inquiry at the drug-store, the proprietor stated that the bottle was filled from one that was nearly empty, which had been made from Tilden's solid extract of taraxacum, and that this was the last of, a pint prepared some time ago, and they had heard of no bad effects before.

Another person took a little of the preparation on the tongue at the request of Mr. P—— at the same time, and she was made sick, and states was unable to sleep during the night. Two persons applied the fluid to the eye on following days, and it immediately dilated their pupils: in one case only a drop was used. There seems to be no question, from the effects produced, that the above was a case of accidental poisoning by belladonna. In connexion with this case Dr. Butler refers to Dr. Garrod's view, that animal charcoal is an antidote to the poisons of belladonna, henbane, and stramonium.—*American Medical Times*, April 20th, 1861.

Toxical Effects of Bichromate of Potassa.—M. Taillard, of the Military School of Health of Strasbourg, from a series of experiments with the bichromate of potassa, in which the salt was given to dogs, either by the stomach, or by injection into the cellular tissue, or by injection into a vein, in doses of 0·25 gramme, obtained the following results:—Death took place in periods varying from two to six days. The symptoms produced were vomitings, sometimes diarrhœa, loss of appetite, difficulty of respiration, and decreasing power of the circulation. If the dose was very great, all the phenomena of an acute gastritis were presented; the thirst was intense, and the vomiting mucous, bilious, and sometimes sanguineous. The extremities became cold; there were dyspnœa, great anxiety, loss of appetite, and at last stertorous breathing and death from complete prostration.

Some observations upon the effects of the bichromate on human beings have confirmed the results of these experiments. The treatment in cases of poisoning by this salt would be, according to M. Taillard, to endeavour to neutralize by alkalies the slightly caustic property due to the excess of acid present in the salt, so as to transform the salt into a neutral chromate. The patient should then be made to vomit.

As the chrome salts are now much used in various manufactures, by stainers and makers of coloured papers, these researches of M. Taillard are of value to the industrial community.—*Gaz. Med. de Strasbourg*, Avril, 1861.

Poisoning by Oxalic Acid.—In December, 1858, Dr. Littlejohn, the author of the case about to be noticed, was desired, by the authorities of the Maternity Hospital, Edinburgh, to examine the body of an infant, the suspicion being prevalent that the child had been poisoned. The mother, an unmarried woman, had been delivered twelve days previously. The child was alive and well on the 4th of December about nine o'clock in the evening, but about ten o'clock was found dead in bed. The appearance of the child at the post-mortem examination was as follows. It was full-grown, and in every respect well-developed. It was fresh, and presented the usual post-mortem rigidity. The lips were of a blackish colour, and exhibited a puckered, corroded appearance. Towards the left angle of the mouth there was a slight scratch. On opening the mouth, the tongue was observed to be of a grey colour, and coated with a slimy fluid. The navel was newly healed. The lungs were expanded, and crepitated on pressure; the right side of the heart was filled with partly fluid blood; the left side was empty and well contracted. The upper surface of the tongue had a grey, sodden appearance, and was here and there covered with a pasty-looking matter; the whole of the pharynx and the opening of the windpipe were much congested. The gullet, from the pharynx to the stomach, was raised in longitudinal folds, had a macerated appearance, and near the cardia was of a deep ashen colour. The large curvature of the stomach exhibited a perforation which implicated a considerable part of the posterior wall. The mucous membrane everywhere was of a dark colour, very soft, and could easily be raised from the muscular coat. The pyloric extremity and the duodenum were deeply congested and softened in texture. The small intestine otherwise appeared healthy, as also did the large intestine as far as the descending portion of the colon, which, with the rectum, was congested on the mucous surface. The inner surface of the stomach, and also the parts in the neighbourhood of the perforation, when touched with litmus-paper, gave marked evidence of acidity. The washings of the stomach had also a decided acid reaction. The chemical analysis instituted gave perfect evidence of the presence of oxalic acid. It was estimated that nearly four grains of oxalic acid were present.

The mother of the child was tried and sentenced to fifteen years' penal servitude. Dr. Littlejohn thinks that in addition to this case there are only four recorded deaths from oxalic acid in which the stomach has been found perforated.—*Edinburgh Medical Journal,* July, 1861.

Action of Cannabis Indica.—Dr. Charles A. Lee gives an account of the physiological action of the Cannabis Indica. It accords fully with the history supplied in our last Report from the work of Dr. Polli, on 'Haschisch.' It also accords with the observations of Christison, O'Shaughnessy, Hooke, and Donovan. Dr. Lee's description runs thus:

"The action of hemp on man is so various, that when we read the several descriptions given, differing so widely, we would scarcely suppose we were considering the same agent; but it is perhaps no less remarkable than the every-day exhibitions we witness of alcohol, to which, being more familiar, we give less attention. The great variety of phenomena presented in the use of the latter, according to the natural disposition or temperament of the person, and the condition of mind, as well as to the quantity, alternately elevating or depressing in its effect, or producing the extreme of kind or brutal emotions, can, to a certain extent, be observed in the other.

"The mental phenomenon upon a subject possessing an imaginative faculty in a high degree, when fully developed, is exceedingly interesting and curious. One writer describes it as follows: 'When it first begins to act, the effects of the *haschisch* may be considerably diminished, or altogether checked, by a firm exertion of the will. By degrees, however, the power of controlling at will and directing the thoughts diminishes, till finally all power of fixing the attention is lost, and the mind becomes the sport of every idea which arises within itself or is forced upon it from without. We become the sport of impressions of every kind. The course of our ideas may be broken by the slightest cause. We are turned, so to speak, by every wind. By a word or a gesture our thoughts may be successively directed to a multitude of different subjects with a rapidity and a lucidity which are truly marvellous. The mind becomes possessed with a feeling of pride corresponding to the exaltation of its faculties, which it is conscious had increased in energy and power. The slightest impulse carries it along.

"'The errors of perception, in regard to time and place, to which the person is liable during the period of fantasia, are remarkable. Minutes seem hours, and hours are prolonged into years, till at last all idea of time seems obliterated, and the past and the present are confounded together.'

"M. Aubert describes the influence upon him in the following language;

'I was engaged in conversation when I felt a pricking sensation in my feet, and in my head a stricture which gave way suddenly, and my skull seemed empty. Every object wore a new aspect; my companion's face assumed a grotesque expression; I burst out a laughing, and continued to laugh for almost an hour. The merest trifle renewed my mirth. Meanwhile the most varied and whimsical ideas coursed swiftly through my mind. I experienced the most perfect sense of comfort. For me there was no longer past, present, or future; the fleeting moment limited my whole existence. Then followed a calm, and sleep stole over me. The whole night was but one long delightful dream. On awaking, I remembered perfectly all that had taken place, and my head was not heavy nor my mouth dry, as it would have been after a debauch in opium or wine.'"—*Journal of Materia Medica; and, Philadelphia Medical and Surgical Reporter*, May 18th, 1861.

Poisoning by the Arum Maculatum.—A child, three years of age, having chewed, on the 20th of April, 1860, at 2 P.M., some roots and flowers of the common arum maculatum intended for a pig, immediately complained of smarting and burning in the mouth, and on the lips. Three hours afterwards he was plunged into a profound torpor, to which succeeded intense feverish reaction. At eight o'clock, M. Cancella, the author of this history, found the little invalid prostrate, speechless, frequently raising his hands to his mouth and throat, uttering at intervals a piercing cry, and getting up as if suffocated. The caustic action of the plant extended to the lips, the palate, the tongue, the tonsils, and the pharynx, and as far as the sight could extend. On pressing the stomach, the pain evinced showed that the child had swallowed some of the juice of the plant, and that the caustic action extended even there. An attempt was made to administer a solution of marine salt, but deglutition was found to be impossible. The swelling was so great that an œsophagus tube could not be introduced. Revulsive agents were used, but ineffectually; and death took place at eleven o'clock, in delirium and by asphyxia. [No results of post-mortem examination are supplied.]—*Gazetta Medica d'Oporto*, No. 6; and *Répertoire de Pharmacie*, No. 9, Mars, 1861.

Poisoning by Iodide of Potassium.—A man, thirty-eight years old, who had been suffering from sciatic rheumatism for one year and a half, but who was in other respects healthy, took from an unprofessional person a mixture consisting of two drachms of iodide of potassium, four ounces of water, and one ounce of syrup, in doses of one tablespoonful every half-hour. In the whole, thirty-six grains of the iodide were taken. After the second dose symptoms of poisoning commenced and continued to increase during three hours, when the patient presented the following conditions. His head was hot, his face puffed, his eyes swollen, sunken, and very sensitive to light. The submaxillary region was swollen, there was great restlessness, singing in the head, sharp pains in the head, and great exudation of mucus and saliva from the mouth; there was no particular smell perceptible from the mouth; the inclination was for sickness, but there was no pain in the lower portion of the body. In the circulatory system there was nothing abnormal. As a drink, starch flour was ordered in water, vomiting was induced by the process of tickling the fauces, leeches were applied to the temples and to the back of ears, and cold cloths were put to the head. In twelve hours the pains had ceased, and in forty-eight hours the flow of saliva, the symptoms of swelling, and the other signs of poisoning had vanished.

Clarus, who narrates this case from the 'Nass. Med. Jahrb.,' p. 747, 1861, where it is recorded by Dr. Orth, remarks upon it that it is one of interest, because the symptoms called forth by the iodide were pure, and were not interfered with by the symptoms arising from any other disease; because no general symptom of disease was exhibited except nausea; and lastly, because the symptoms induced followed so small a dose as thirty-six grains of the salt, in a strong, young, and not over-sensitive man.—*Schmidt's Jahrbücher*, Band iii., Jahrgang 1861.

Poisoning by Lead.—Dr. Wm. Moore, of Dublin, contributes some really valuable, because most carefully kept, clinical records on the paralysis and atrophy consequent on poisoning by lead. We select for quotation the following case, inasmuch as it indicates that the idea universally accepted, of recognising the effects of lead, as specially presented in the muscles of the hands and forearms, is not to be taken as a rule absolute.

"CASE 1.—Higgins, aged forty, by trade a painter, or more correctly a mixer of paints, as his occupation of late has been to prepare the colours for painting the boats of one of the large steamship companies trading from this city, was admitted into Sir P. Dun's Hospital in the month of April, 1860. His appearance on admission was anæmic and cachectic; his expression sad and melancholic; the hue of his skin generally was of a dusky cerulean character; he could not extend (laterally) or raise his upper arms, but the antero-posterior movements of both arms were comparatively unimpaired; when he attempted to walk he faltered in both legs, which seemed to converge at the knees. He stated that for the nine months previous to his admission into hospital he had been gradually losing the power of himself, but that latterly it has increased so much as to prevent him following his ordinary avocations. On a closer examination I found the well-marked blue line encircling the gums, whilst the teeth were covered with the dark incrustation first described by Tanquerel. On stripping the patient, the clavicular and scapular prominences were very remarkable, the deltoïd muscles had well nigh disappeared, whilst the supra and infra-spinati were atrophied to such a degree as to present cup-shaped depressions above and below the scapular ridges. The latissimus dorsi and pectoral muscles were soft and slightly atrophied. The biceps and triceps of both arms were wasted, those of the right arm more than those of the left. The extensors and flexors of both forearms were soft and atonic from disuse, but as regards their muscular volume they seemed intact, as also were the muscles of the thumbs and hands generally. The spinal, lumbar, and muscles of the lower extremities were sensibly atrophied, the left extremity generally more than the right, more particularly the left gluteals. The sensibility of the two extremities least atrophied—viz., the right leg and left arm, was increased so that the patient was unable to bear the weakest current of electricity, whereas he could bear electricity strongly applied to the alternate leg and arm; his pulse was small; heart's action very feeble, and he has been suffering from occasional sensations of faintness on ascending stairs or taking any undue exertion.

"The treatment consisted in a combination of strychnine and iodide of potassium, one-sixteenth of a grain of the former with four grains of the latter three times a-day; electricity was employed daily.

"On the 19th June this patient's general health gave unequivocal evidences of restoration; his appearance was much improved; the blue lines had well nigh disappeared from the gums; the teeth had lost their dark incrustation; he could shave himself and walk much more steadily; the hyperæsthesia of the right leg and left arm had sensibly diminished. The same treatment was continued till the 3rd July, when Dr. Smith, under whose care he had now been for some weeks, considered him so far recovered as to allow him to leave the hospital, previous to which I made a careful inspection, and found the muscles of both scapulæ and humeri increased in volume and tone, as were those of the lower extremities, so that the patient could raise both arms and walk steadily. The blue line was scarcely discernible on the gums. (The urine in this case was tested for iodide of lead by Professor Cameron, but he could not detect a trace of it.)

"This case may be said to possess some features of peculiar interest, writers on lead-poisoning generally confining the atrophy to the muscles of the fore and upper arms, whereas in this instance the muscles of the hands and forearms were apparently intact, the patient having perfect use of them. The atrophy and paralysis seemed to begin from the bends of the elbows upwards, and with a few exceptions, to implicate all the superior and inferior muscles of the trunk and lower extremities. The established rule in cases of *paralysis saturnina* is that the muscles of the upper extremities, particularly the extensors, are the first to suffer, the lower being secondarily and rarely involved, and when they are, it is almost exclusively in connexion with paralysis of the former. In 102 instances Tanquerel found general paralysis of the lower extremities in only one case, whereas Romberg places the proportion as 1 to 6."

After relating other examples each presenting some interesting peculiarity, Dr. Moore adds,

"The diagnosis between the paralysis from lead and the muscular atrophy and paralysis of Cruveilhier has been defined by Roberts, Banks, and others. With the muscular atrophy lead palsy has many symptoms in common. Lead palsy, however, is usually a more sudden affection, invading the muscular structures in a certain order; the extensors being specially affected, and unless the paralysis and atrophy become general, the loss of power may be confined to them. There seems usually to exist no direct ratio between the loss of power and atrophy in what may be designated the acute form of this disease. Some assert that the loss of power is primary, the wasting a secondary accession; again, that complete paralysis of an entire extremity ensues whilst there is no perceptible diminution in the muscular mass. To neither opinions can I absolutely assent, never recollecting to have seen an instance of paralysis of an extremity from lead poisoning without an atrophic condition of a muscle or muscles. The application of electricity will also guide us to some extent in doubtful cases, muscles paralysed from lead betraying at first little, if any, contractility; whereas in Cruveilhier's palsy such an absence of contractility has not been observed. In cases of general atrophy and paralysis the result of lead poisoning, the disease might be confounded with 'the general paralysis of the insane;' but just as Dr. Banks has mentioned 'the unimpaired intelligence and the amount of atrophy present in wasting palsy, as sufficient to diagnose it from the general paralysis of the insane,' so the same points hold, to a great extent, in the diagnosis of the 'paralysis saturnina' of Romberg.

"About the pathology of this disease I fear we have still much to learn. Andral, Gendrin, and Tanquerel agree as to the absence of perceptible changes in the central and peripheral nervous apparatus. Some of the older authorities have mentioned the presence of cerebral congestion and effusion of serum, whilst more recent writers have found the morbid changes to consist in uniform slight softening and yellowish discoloration of the brain. This discoloration of the cerebral substance has also been seen by Grisolle, Solon, and others. Andral has recorded a case where its consistence was less than natural, and hypertrophy of the brain has been the result as described by others. According to the first-mentioned distinguished authorities, in only two cases there was an appreciable amount of serum within the cavity of the cranium and vertebral column; in one case only the ulnar and radial nerves were found atrophied.

"The late Dr. Todd has described the morbid appearances in the brain and spinal cord in persons exposed to the influence of lead as denoting imperfect or depraved nutrition of those centres, and frequently associated with marks of chronic irritation of the membranes, such as accompany intemperance, the brain especially presenting the appearance of an ill-nourished organ, pale, soft, its convolutions wasted, the sulci between them wide, and sometimes patches of white softening are found in the hemispheres. These conditions he has observed in patients who have experienced several paroxysms of epilepsy before death, or who have died in one, and in whose brains lead has been detected.

"The paralyzed muscles present the following appearances: The muscular fibres lose their normal form and become atrophied, presenting a pale yellow or azure colour, become friable, sometimes shrivel, and contrast strongly with the fleshy and healthy appearance of the other muscles. Molecules of lead are stated to have been discovered by chemical analysis in the paralyzed extremities, and the researches of Orfila, Tiedmann, Devergie, and others have shown that in subjects who have died from lead poisoning an unusual amount of the metal has been found in the brain, muscles, viscera, and especially in the blood. Fodière has described the appearance of the heart from lead poisoning as 'withered.'

"*Treatment.*—As regards the treatment of the paralysis and atrophy from lead poisoning, it would be needless to pass in review all the remedies suggested; accordingly, I shall confine myself to those I have found most efficacious. To relieve the acute colica pictonum, I think the immediate application of electricity, applied as I have described through the medium of damp sponges, and directly over the abdominal muscles, is well deserving of a trial; Dr. Briquet, who was the first to recommend it, having seldom or never found it fail to give relief. My own experience of it is, to say the least, encouraging. After the application of the galvanism I should recommend the patient to be placed in a tepid bath not higher than 94° Fahrenheit, in which he might remain for twenty minutes. The constipation to be relieved by means of a brisk purgative combined with opium, and, if necessary, its action accelerated with tepid enemata. When the bowels have been well freed, the earliest opportunity should be taken advantage of to exhibit a general eliminator of the poison from the system, and for this purpose I should give iodide of potassium the first place, either *per se* or combined with strychnine or iron. M. Melsens, on the treatment of metallic poisoning, has caused this agent to be much employed, and his observations have been corroborated by Dr. Sieveking and others. In Dr. Parkes' paper he gives proof of the lead actually passing off by the kidney during the exhibition of the iodide of potassium. In three of the cases above detailed the urine was carefully tested for iodide of lead, whilst the patients were for some time under the influence of the iodide of potassium, but in none of the cases could lead be found. M. Melsens gave the iodide of potassium in larger doses than I should be inclined to exhibit it, having found from five to eight grains three times a day, steadily continued, prove very successful.

"The combination of the iodide with iron is indicated in the majority of these anæmic and atonic cases, and for this purpose we have at hand the very palatable preparation of the syrup of the iodide of iron. Strychnine I hold to be a most important adjunct to the iodide, its tonic and antiparalytic properties being specially serviceable where much loss of motility is present. The dose may vary from *one-twentieth* to *one-twelfth* of a grain three times a day, which may be gradually increased. Tanquerel gave it up to two grains in the twenty-four hours with satisfactory results.

"Of the topical appliances, I should give baths the preference, tepid seawater baths, or what I have found more satisfactory still, the sulphuret of potassium bath, which is made by adding from three to five ounces of the sulphuret to the bath, in which the patient should remain for at least twenty minutes. This may be repeated every night, or every second night, according to circumstances. Of the more immediate local agents I should recommend electricity, at the same time enjoining friction over the paralyzed muscles, with the occasional addition of a stimulating embrocation, as the camphor or soap liniment, with chloroform or opium. Should severe neuralgic pains be present, the addition of tincture of aconite or belladonna may be indicated. In applying electricity, I prefer a moderate current, steadily continued, and for a longer time, to a more violent and shorter application of this agent, which, when thus employed, frequently proves exhausting. To the application of splints, with the view of keeping up extension, I attach little, if any importance; in fact, I rather incline to the belief that they act more injuriously than otherwise, by tending even further to promote atrophy of the extensors from disuse; but an application has been suggested, I believe by Dr. Inman, which seems feasible and easily applied. It is made by joining an elastic strap to the back of the fingers of a glove, the other end of the strap being fastened above the elbow. This simple contrivance, whilst it tends to give support to the extensors, does not interfere with their motion or that of the extremity generally. To anticipate the occurrence of lead or other metallic poisoning in those engaged in the manipulation of these agents, frequent ablutions of the hands and arms, and perfect cleanliness of the person generally, are indispensable."—*Dublin Medical Press*, June 19th, 1861, and August 7th, 1861.

II. Hygiene.

On Climate and Disease in Japan.—Dr. Albrecht, attached to the Russian Mission in Hakodade in Japan, gives the following account of his observations respecting the weather and diseases of Japan during November, 1859, and eight following months. From the month of May, rain and fog prevail, brought by the east wind from the Pacific Ocean. In June there were fifteen rainy days. During the winter, snow fell abundantly in Hakodade, a great deal of which was blown off by the wind, so that a bad sledge-road existed, scarcely used by the Japanese.

Hakodade, situated on the declivity of a mountain, is sheltered on the north and south by a chain of high hills, consequently there are but two prevailing winds; in the autumn, winter, and spring they are from the west, north-west, and south-west, but with no fog; from May, only east winds are felt.

The country around Hakodade is not particularly beautiful; everywhere there are high hills sloping towards the sea, but only for a few versts, and but here and there accessible from the sea. There is little shelter in summer; the Flora everywhere is scarce; no beautiful meadow-land, as in our country. Husbandry is very little attended to upon the island, but in one village twenty-five versts from Hakodade there is a small rice plantation. The radish is more in use than with the Russians. The Japanese eat all maritime produce, with the exception of the poisonous kinds, all shell-fish, and innumerable roots and herbs; sea-weed is dried and cooked in soups.

The diseases most constantly occurring are ophthalmic and syphilitic diseases, both much spread, from the immense extension of prostitution, not spoken of by the medical police. If a Japanese has a disease in his eyes, and the gods do not aid him, he assuredly loses an eye, if not both. Their manner of living causes the inflammatory diseases in the eyes which are of such constant occurrence. When at home, a Japanese sits constantly before a fire-pan; he knows nothing of stoves, although the cold is very sensibly felt during winter, on account of the sharp west and north-west winds. Warmed by the fire, he goes into the open air, exposing himself to every temperature with uncovered head, the half of which is shaven. Congestion and inflammation are the results. The women shave even their eyebrows.

One domestic disease is the itch. This is met with even among the better classes. Few epidemics occur. Hakodade seems by its position to be protected from all stagnation of the air. Intermitting fevers are rarely known.

In Jeddo there is a medical school, which was established by the Dutch.

The younger pupils in this establishment obtain their information by acting as assistants to those who are more advanced. Anatomy is said to be taught from the dissections of the bodies of dogs. All surgical manipulations in cases of midwifery are disallowed.—Dr. Franz Seitz. *Canstatt's Jahresbericht. Im Jahre* 1860, Band ii.

Disease from the Consumption of Tainted Animal Food.—On the 3rd of June, 1846, a man, aged thirty-six, J——, of Dorndorf, and two of his children, one three years old, the other one year and a half, ate a small portion of beef for their breakfast. This beef had been cooked on the 31st of May, and had been taken by the whole family, consisting of eight persons, on that and the following day, without injury to them. From that time the meat had been kept on a china plate in a pantry. Soon after eating it a second time, the three first-mentioned persons were attacked with surprising weakness, faintness, and drowsiness with dizziness, the face being strikingly white, the lips bluish, with cold sweat on the forehead, quickly followed by pains in the body—viz., sharp pain in the cardiac region: nausea and sickness succeeded, whereby the meat which had been eaten was vomited.

Dr. Dehne found the patients extremely pale, with blue lips and cold perspiration on their foreheads; very weak, and in a drowsy state, which was only disturbed by the paroxysms of pain coming on in the stomach; the extremities were cold; the pulse scarcely perceptible. Emetics, enemata, mustard-plasters, strong coffee, and a solution of sulphate of potash, so far effected a cure, that in about an hour and a half the invalids fell into a refreshing sleep, from which they awoke with a warmer, moister skin, and a more natural colour. They still complained of exhaustion, but the pains were gone; the pulse was natural; the pupils, especially in the children, were widely dilated. On the following morning they were all perfectly well.

In addition to the above case, Dr. Dehne relates the following examples, in which marked symptoms of disease occurred after the consumption of a broth made from tainted beef and salad:

On the 3rd of June, 1850, during his stay at Geisenheim, Dr. Dehne was called in about 6 o'clock to a widow in the poorest circumstances, who, with her four sons, had been ill one or two hours after eating some broth made from beef, together with a salad of potatoes and lettuce. These persons all exhibited the same symptoms (but in different degrees, according to the larger or smaller quantity of salad taken), which were shown mainly in the following manner. Soon after eating, nausea, choking, and vomiting came on, with sharp pains in the region of the stomach. The substance first brought up was the salad, afterwards large quantities of watery fluid. Soon after, cutting pains in the lower region of the belly, with constant diarrhœa, at first tolerably solid, then watery. Intense thirst occurred, but upon quenching it the sickness returned, pain in the head, dizziness, confusion of the intellect, heaviness and disturbance in the head. The head was cool, the face pale, the features distorted, the pupils much dilated, lips slightly coloured blue, and the tongue moist; the abdomen was not swollen, but rather contracted and not sensitive on pressure. The skin was cool, pulse weak and wiry, the heart and respiration not essentially altered; the speech feeble; great depression, uneasiness, and weakness were observed, and when the sickness abated, a complaint of cold and a tremulous shivering appeared. A few hours afterwards, the sickness ceased, the skin became warmer, diarrhœa continued partially, perspiration and quiet returned, and a perfect cure resulted. One of the sons, who had eaten but a little, suffered only from sickness, diarrhœa, some pains in the stomach, and loss of appetite; another son, on the contrary, who was thirteen years old, besides the symptoms already described, suffered from shivering in the limbs, convulsive movements of the upper extremities, occasional wanderings, and inability to hold the head up; and in one nine years old a large quantity of ascarides was evacuated. The treatment consisted of copious administrations of milk-and-water to keep up the sickness, alternated with barley-water and an oil mixture. All the patients were wrapped in warm cloths and rubbed.

Remarks.—The poisoning could only have arisen either from nightshade germinated in the potatoes, or from the broth made of tainted meat in which the potatoes had been cooked. The first is not probable, as the family had hitherto eaten of the same potatoes without any harm arising; on the other hand, the formation of a fat acid *is* probable, as the potatoes which had been boiled with the broth stood for twenty-two hours in a great heat in a damp pantry.

To the above another example is appended, in which the symptoms were presented after the consumption of some stale fat. The author (Dehne) was sent for on the 4th of June, 1858, at 7 o'clock in the evening, to attend a whole family of five persons (a mother and four grown-up daughters), who had been suddenly taken ill after eating their supper. The supper consisted of lettuce salad and flour dumplings, with the remains of some fat which had been kept in a stone pot, all of which were stewed together. They all suffered from sharp pains in the region of the stomach, choking sensation, and copious vomiting, extraordinary debility, fainting weakness, shivering in the limbs, with sensation of cold in them, duskiness before the eyes, so that the patients fell down, then moaned aloud, and seemed oppressed with anguish. The substance vomited consisted of the food they had eaten; a more rigid investigation could not be made, as it was all quickly thrown away. After rest and warmth in bed, the symptoms subsided of themselves in a few hours, and sleep and cure ensued. A sixth person, who had partaken of some of the same meal remained perfectly well. To the acid developed in the stale fat, the symptoms observed are also attributed.—*Nass. Med. Jahrb. a. a. O. p.* 736; *und Schmidt's Jahrbücher*, Bd. iii. No. 7, Jahrgang 1861.

[We may remark, in reference to these cases, that no subject demands more urgent inquiry than that which relates to the effects of animal substances used as food, while in a state of decomposition. In 1853, at Cowbridge, in this country, in a race-week, during which two balls were given, nearly every person who attended these balls was seized with peculiar and serious symptoms, of which some died. The symptoms were all compatible with effects arising from eating tainted animal food, but in this direction no inquiry was instituted.—Rep.]

Disease from eating bread containing blighted corn.—Dr. Ricker of Hattenheim, relates being called to a family of six persons, all of whom had been seized with similar symptoms of disease after having partaken of some bread containing blighted corn. The symptoms presented were those of dryness in the throat, oppression at the stomach, disagreeable taste; vomiting of food of the consistency of pap, mixed with mucus and bile, giddiness, faintness, and in two of the affected persons, both children, diarrhœa. The symptoms all passed away within two days, after the administration of purgative medicines.—*Ibid.*

The subjoined titles refer to papers which are of value, but which, from deficiency of space, cannot be noticed at length.

Report of Proceedings in Medical Geography. By Professor Franz Seitz, of Munich. (Canstatt, Jahresbericht, 1860, Bd. ii.)

Papers on Meteorology. Collected by Professor H. E. Richter, M.D. (Schmidt's Jahrbücher, 1861, Bd. iii.)

Report on Meteorology and Epidemics. By Dr. W. Jewell. (Ibid.)

Report of Judicial Inquiries made from July 1st, 1859, to the end of June, 1860. By J. Maschka, M.D. (Vierteljahrschrift für die praktische Heilkunde, xviii. Jahrgang, 1861, Bd. ii.)

On Saturnine Poisoning by Powder of Crystal amongst Workmen engaged in the "Contre-oxydation" of Iron. By Dr. Archambault. (Archives Gén. de Méd., Août, 1861.)

Experiences on the Action of Santonine. By Dr. E. Rose. (Archiv f. path. Anat., t. xvi. p. 233.)

Case of Poisoning by Corrosive Sublimate. By Dr. Jas. L. Ord. (The American Journal of the Medical Sciences, July, 1861.)

A Case of Hydrophobia, with a Novel Mode of Treatment. By Dr. W. J. Moore. (Transactions of the Medical and Physical Society of Bombay, 1860.) [Dr. Moore's new treatment consisted in subjecting the patient to affusion of cold water, and in blistering the throat externally with lunar caustic. The patient recovered.]

On the Colour Tests for Strychnia and the Diagnosis of the Alkaloïds. By William A. Guy, M.B. (Pharmaceutical Journal for May, June, July, and August, 1861.)

Case of Perforation of the Stomach, occurring under unusual Circumstances. By Benjamin Bell. (Edinburgh Medical Journal, March, 1861.)

On Poisoning by Aconitum Napellus. (Symptoms from a minimum dose of Flemming's Tincture.) By Dr. Thos. Skinner. (British Medical Journal, April 6th, 1861.)

Case of Poisoning by Aconite. By Dr. Tombe Atthill. (Dublin Quarterly Journal of Medical Science, August, 1861.)

Remarks on a Case of Poisoning by Strychnia, with Recovery. By Dr. James Part. (Lancet, April 6th, 1861.)

On Poisoning by Infusion of Tansy. By Dr. John E. Pendleton. (American Medical Times, March 16th, 1861.)

On some of the Medico-legal Relations of the Habit of Intemperance. A Lecture by Dr. Christison. (Edinburgh: Black.)

Statistics of Prisoners; their Mental Condition and Diseases. By Dr. J. B. Thomson. (Edinburgh Medical Journal, August, 1861.)

A Question of Primogeniture. Case of Twins. By Dr. J. G. Wilson. (Dublin Hospital Gazette, Sept. 2nd, 1861.) [The twins, both males, were born while the mother was quite alone, her husband having gone to seek a nurse. The mother did not know which was born first. Dr. Wilson inquires, What would be the law of primogeniture in the case of these children?]

On the Toxical Effects and Therapeutical Employment of Benzine. By Professor M. A. Ray. (Gazette Méd. de Lyon, 1 Juillet, 1861.)

On Poisonous Mushrooms. By M. Moquin Tandon. (With Drawings.) (Revue de Thérapeutique, 1 Juin, 1861.)

On Accidental Poisoning by the Employment of Leaves of Henbane. By Dr. Martin Saint Ange. (Gazette Méd. de Paris, 16 Mars, 1861.)

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

I. *On the Treatment of Strangulated Hernia.* By M. Gosselin. (Archives Générales, February, 1861, p. 130.)

In a former paper,* M. Gosselin stated that he was enabled to communicate the results of 85 cases of strangulated hernia, and he then gave an account of

* See Medical Times, vol. xl. p. 489.

29 of these cases, which he had treated by forcible and prolonged taxis; and he came to the conclusion that this was a useful means during the first twenty-four or forty-eight hours of strangulation. In the present paper he furnishes an account of his operations, and of those cases of omental hernia in which he has resorted to temporization.

Results of the Operations.—These have been 51 in number—viz., 19 (17 males and 2 females) for inguinal hernia, with 9 recoveries and 10 deaths; 31 (25 females and 6 males) for femoral hernia, with 23 recoveries and 8 deaths; and 1 male with umbilical hernia, who died. Two of the women who recovered after the operation for femoral hernia did so with artificial anus, this having spontaneously healed in one of them. The greater mortality after operation for inguinal hernia was balanced in the sum total of the cases by the success of the taxis in inguinal hernia, the number of recoveries in a given number of cases of each description of hernia being almost alike in the two varieties. The conclusion to be drawn from these cases is, that the result of the operation is to be more apprehended, while the taxis is more advantageous in inguinal than in femoral hernia. The influence of the *duration of the strangulation* is seen in the following figures. In inguinal hernia, before the fiftieth hour of strangulation there were 6 recoveries to 3 deaths, and after that period 3 recoveries to 7 deaths; and in femoral hernia, 11 recoveries to 2 deaths before the fiftieth hour, and 12 recoveries to 6 deaths after it; giving for the two 17 recoveries to 5 deaths prior to the fiftieth hour, and 15 recoveries to 13 deaths subsequent to it. With respect to the influence of the *age* of the patient on the result, M. Gosselin goes also into details, from which it seems, that in his cases this did not exert a material effect prior to the age of seventy. At all ages it was the duration of the strangulation which determined the relative mortality. Among the 51 cases operated upon, in 7 the intestine was found more or less *gangrenous* or perforated at the point of strangulation. One of these was an umbilical hernia, and the others were femoral herniæ, four of the patients dying. In 3 cases the sac contained only a partial noose of intestine. Thus, the conclusion may be drawn that the taxis may be justifiably employed at a later period in inguinal than in femoral hernia, and especially when this last from its small size may be supposed to contain only part of a noose of intestine.

M. Gosselin adverts to a point which he once thought a matter of indifference, but which he now regards as of importance. Does the presence of omentum increase or diminish the danger of a strangulated hernia? It results from these cases,—(1) that in all the cases in which gangrene of the intestine was found, no omentum whatever existed in the sac; (2) that of 43 cases in which the facts concerning the omentum were noted, it was found that in 26 cases in which it was not present, there were 15 instances of recovery and 11 deaths, while in 17 entero-epiploceles there were 11 recoveries and 6 deaths. Thus, the issue of the case seems to be rendered more favourable by the presence of the omentum; and supposing the experience of other surgeons corroborates this view, it becomes of importance to determine in a given case which of these two circumstances we have to do with. M. Gosselin suggests that the clinical investigation of hernia requires some alteration. The questions of the seat of strangulation, and the existence, obstruction, and inflammation have been sufficiently elucidated, and it is now desirable to determine with more precision between the choice of taxis, the operation, and temporization. As material for examination in this point of view, he offers the above conclusion, that enterocele, and especially when containing only part of a noose of intestine, calls for more prompt proceeding than does entero-epiplocele. His own rule is never to allow an *intestinal* strangulation to remain under the plea that it is but slight and of but little urgency. Once recognised, he does not leave the patient until the hernia has been reduced either by the taxis or the operation.

Temporization has been observed by M. Gosselin in 5 of his cases, 2 of these being examples of umbilical, 2 of inguinal, and 1 of femoral hernia. For the umbilical herniæ this plan was pursued because they were old, large, partly irreducible, and seemingly chiefly constituted of omentum. Calomel and jalap were administered, and produced evacuations, and the condition of the hernia became much what it was prior to the occurrence of symptoms of strangulation. In the case of large irreducible inguinal hernia manifesting signs of strangulation, the author would act in a similar manner. The other three cases, two of inguinal and one of femoral (hitherto reducible) hernia were also treated successfully by the same purgative, M. Gosselin believing from its physical characters the tumour in each to consist of omentum. Stools followed before the tumour had undergone any diminution, and the symptoms gradually abated. In two an indolent projection, of less size than at first, persisted; while in the third case all swelling had entirely disappeared by the end of thirty-five days; these different results doubtless depending upon whether the omentum did or did not adhere to the sac. To the objection that the hernia in these cases was inflamed and not strangulated, M. Gosselin replies, that he has long been persuaded that when a hernia becomes suddenly irreducible, cotemporaneously with symptoms of strangulation, such hernia has become too large to repass by the aperture, and is consequently strangulated by this. Without doubt there may be also inflammation, which indeed may have been the primary phenomenon. But whether inflamed or strangulated, M. Gosselin is of opinion that omental hernia should be left to itself, and treated only by rest and emollients, and that the sagacity of the surgeon should be employed, not in examining whether a hernia is strangulated by the neck of the sac or a fibrous ring, whether it is inflamed or strangulated, but in discovering whether it contains omentum alone, or intestine with or without omentum.

II. *On the Treatment of Abscesses by Drainage.* By Professor Roser. (Archiv der Heilkunde, vol. ii., pp. 1-15.)

In this paper Professor Roser, of Marburg, protests against the indiscriminate resort to Chassaignac's "drainage" treatment of abscess, believing that it is often employed when unnecessary or even mischievous; and taxing its author with greatly exaggerating the easy execution and advantages of the practice. The following are his chief conclusions:—1. There are various circumstances which impede the healing of abscesses, besides obstacles to the free exit of their discharge. Among these is rigidity of the walls of the abscess, which not only impedes the issue of the discharge, but also the subsequent contraction of the new tissue. The cutting through the walls expedites matters, and this is one of the reasons of the success which attends the slitting up of fistulous passages. Local atrophy with anæmia, too, offers a remarkable obstacle to healing in undermined portions of the skin, as is seen in glandular abscesses of the neck. On the other hand, hyperæmia has always to be moderated in order to produce sound healing. 2. Drainage is not suitable in acute abscess, as coagula of blood or fibrin, or portions of dead tissue, have to be removed. 3. The same objection applies to cold or congestive abscesses, which naturally contain albuminous precipitates which will often obstruct the tubes. In small abscesses it is not worth while to introduce the tubes, while in sinuous and subcutaneous fistulous passages incision or excision of the skin is the proper treatment. There is, in fact, only a small number of deep-lying abscesses of the breast, abdomen, pelvis, &c., to which the tubes are applicable. And here it is very questionable whether the better practice is not to enlarge the aperture by means of a probe-pointed bistoury, or the daily introduction of a dilator. 4. In certain abscesses of the neck, thorax, mamma, axilla, anal region, &c., a valvular mechanism impedes the discharge, and this may be remedied by the introduction of the tubes. But even here this need be only exceptional, as the end will be better brought about by incision or dilatation. The drainage tubes are, indeed, not without their inconveniences, requiring preliminary enlargement of narrow fistulous openings for their application, being difficult to fix in some places, as near the anus, and sometimes stimulating the walls of the abscess too much. The time for their removal is not always easily decided. If they remain too long, they retard the healing, and if they are removed too soon, the external aperture prematurely closes. 5. The drainage tubes serve the purpose of conveying the discharge away better than do setons; but their application leads to the unnecessary multiplication of counter-openings. These are, in fact, only indicated when they may prevent the further penetration of the discharge, and facilitate its escape. 6. The drainage tubes may be used in preference to other means when a large opening is not practicable, or is not on anatomical grounds advisable.

III. *On Diseases of the Membrana Tympani.* By Professor VON TROELTSCH. (Wiener Medizinische Wochenschrift, Nos. 9 and 10.)

Diseases of this membrane are very frequent, as indeed might be expected from its position between the meatus and the cavity of the tympanum, which causes its participation in the structure and diseased conditions of both. When it is considered that the membrane thus comprises three of the most important tissues of the economy (external skin, mucous membrane, and fibrous membrane, the lamina propria), a frequency of its diseases is only to be expected. Frequent as these diseases are, they are rarely met with as primary and uncomplicated affections; and although this assertion is contradictory to the opinion maintained by most authors, Dr. Troeltsch is convinced of its accuracy, not only by extensive observation of the disease, but by a critical examination of the cases which have been recorded in the books as examples of inflammation of the membrana tympani.

In the rare cases in which it does exhibit itself as a *primary* affection, *inflammation* of the membrane may be either acute or chronic. The acute form the author has always observed manifesting itself suddenly and by night, for the most part under the influence of cold, and frequently after a cold bath. The sudden, deep-seated, tearing pain is accompanied by a sense of fulness, deafness, and very violent noises in the ear. These symptoms may last from twelve hours to three days, ceasing only as the meatus becomes moist and a discharge from the ear is gradually formed. From the commencement, the membrane is found to be in a highly hyperæmic condition, looking as if artificially injected, while its epidermis has lost its shining appearance. The handle of the malleus is less easily seen, or is even invisible, while the concavity of the membrane has more or less disappeared. At a later stage, the epithelium is loosened, and the corium is red and swollen and covered with secretion. The meatus, at first normal, soon participates in the swelling. In several cases ulceration and perforation of the membrane have occurred. In more favourable cases the appearances gradually amend, but a long time elapses before the membrane recovers its shining appearance and the manubrium acquires its former freedom and projection, so that the fact of inflammation of the membrane having been present can be ascertained long after its disappearance. All the cases seen by the author have been unilateral. The prognosis is very favourable if suitable treatment be pursued, and under favourable circumstances scarcely a vestige of the occurrence may remain. If neglected or improperly treated, the inflammation may become chronic, and may spread to the surrounding structures, all the consequences of chronic otitis following. At the commencement, some leeches should be applied in front of the opening of the ear, where they are more serviceable than when put behind the ear; and the pain is best assuaged by repeatedly filling the meatus with warm water. Cataplasms are objectionable, as they tend to render the otorrhœa profuse and chronic. As soon as secretion is set up, it must be carefully removed several times a day, by syringing with tepid water, a weak astringent being afterwards dropped into the meatus and retained there for several minutes. Even if perforation has taken place, this treatment will heal it, if the suppuration has ceased. For the removal of the remaining thickening of the cutis layer of the membrane, iodine, either in tincture or ointment, may be applied around and behind the ear; but blisters are of no use, and may do mischief.

Chronic inflammation of the membrane is of more frequent occurrence than is the acute form; but still as a substantive affection it is not met with in practice in an intense form or with abundant secretion. Under such circumstances it would soon become complicated with inflammation of the meatus or the tympanum. In its simple and uncomplicated form the attention of the patient is usually first called to it by the occurrence of deafness and some discharge from the ear. Pain in general is only of occasional occurrence, and years may pass before medical assistance is sought for. On examination, nothing is found amiss with the meatus except a slight moistening of the epithelium of its lower wall, in consequence of its penetration by the diseased secretion. This, small in quantity and pretty consistent, for the most part covers the membrane, which itself is flat, dull, and unshining, the manubrium making but slight projection. In certain places, and especially above and behind, it is stripped of its epidermis, red and swollen, the other portions being of a yellowish-grey colour, and penetrated with numerous varicose vessels. The partial tumefactions may give rise to polypoïd growths of different sizes, which, even when very small, keep up the discharge. The prognosis of this chronic form is unfavourable, inasmuch that perseverance in treatment may be required for years. Relapse, too, easily takes place, and the considerable changes which the membrane has undergone render the recovery of hearing difficult. As in all forms of otorrhœa, the treatment consists before all things in cleanliness, in the industrious removal of the discharge (which is best done by means of a pencil), and in arresting its secretion. After cleansing, the meatus should be filled with an ear-water, a strong solution of lead being the best. If the treatment is prolonged, the washes have to be changed, but those formed of mineral substances are more efficacious than are the vegetable.

Linear *laceration of the membrane* is not an infrequent consequence of sudden atmospheric compression, boxes on the ear, and the firing of artillery. According to the author's experience, these ruptures always take place in the posterior half of the membrane, in a direction parallel to the manubrium. The abnormal conditions of the *inner surface* of the membrane are due to propagation of disease from the cavity of the tympanum, and are of great practical value, inasmuch as they enable us to recognise the existence of such disease, especially when this exists in the form of catarrh. The mucous layer of the membrane is most developed at the periphery, and in catarrh of the middle ear, especially when this is chronic, we find this part to a greater or less breadth losing its transparency, becoming of a dark grey colour, swollen, and separated by a distinct circular line from the rest of the membrane. From this thickened periphery partial linear thickenings of the mucous membrane also often radiate —changes which to be seen may require a strong light to be thrown into the passage, or that the patient should inflate the membrane. These partial thickenings not infrequently cause the surface of the membrane to seem too flat or oo concave. In old and intense catarrh, the entire surface of the membrane becomes hypertrophied, and that to such an extent that the mucous surface alone may become many times thicker than the entire membrane in its normal state. For an account of these pathological conditions, as well as of the frequently occurring adhesions which take place between the inner surface of the membrane and the walls of the tympanum and the ossicula, the author refers to his recently-published treatise on the anatomy of the ear. Partial *calcifications* are not infrequently the result of severe catarrh of the tympanum, even in quite young persons. They correspond in their form and disposition to the circular internal layers of the fibrous plate. They are concentrically deposited towards the edge, but separated from this by an uncalcified zone, being generally in the form of semilunar yellowish-white spots, situated in the posterior half of the membrane, the spots being less numerous in front. Other changed conditions of the membrane, which from their appearance may be termed tendinous, observe the same disposition, and are probably only the earlier stages of calcification. During these various changes, the colour, thickness, and elasticity of the membrane as a whole may have suffered but little, its brilliancy and the view of the manubrium through it being preserved. The outer layers of the membrane only suffer in affections of the tympanum when these have proceeded to the extent of totally arresting its nutrition. Whether the *fibrous layer* of the membrane undergoes any independent changes it is impossible to say, but from its intimate connexions with the other component parts of the membrane, its pathological changes are probably simultaneous with or dependent upon theirs. In cases of long-standing perforations of the membrane, the edges are sometimes found calcified, the whole thickness of the membrane being converted into a mass of chalk. Ulcerative perforation is far seldomer brought about as a consequence of primary disease of the membrane, than as a consequence of inflammatory and suppurative disease of the meatus or tympanum. In its histological relations, perforation of the fibrous layer resembles analogous changes in the cornea, a tissue with which it has many resemblances.

IV. *Case of Spina Bifida treated by Iodine Injection.* By Professor BRAINARD. (Amer. Journ. of Med. Science, July, p. 65.)

In this paper Dr. Brainard describes another case of spina bifida which he has treated with iodine. It occurred in the person of a healthy girl, three years old. The tumour over the sacrum was eight inches in circumference at its base, and about two and a half inches in height, conical, translucent, elastic, and covered with healthy skin, except at its lower part, where it was discoloured like the vestige of a nævus. Six ounces of fluid were drawn off, and the opening in the spine being closed by the thumb, half an ounce of solution of iodine (iod. 5 grs., iod. pot. 15 grs., water 1 oz.), at the temperature of the body, was thrown in, and after a few seconds allowed to flow out again. The iodine was washed out by means of tepid water, and then two ounces of the fluid first drawn out, and kept at its original temperature, was thrown in. This was on November 10, and on the 17th the tumour having become tense, was tapped, and six ounces of slightly turbid fluid were removed. Compression was applied by means of a truss having compresses of fine linen in its centre, so as to press the skin into the opening of the spine. The tumour continued to diminish in size, and its contents became semi-solid. By the 31st December, the skin at the centre had become adherent to the opening, which was closed, a little fulness around the base at the upper part alone marking the vestige of the tumour. The report comes down to February 10, when no tendency to return had exhibited itself, and the truss had been left off for some weeks.

This makes the author's seventh case treated by iodine, without any dangerous symptoms. It is the third unaccompanied by hydrocephalus, and all the three have been cured; one with thirteen injections, one with two, and this case with one. The operation is so delicate that it is not easy in any case to fulfil all the requisite conditions. The walls in the above case were so thin at the point of puncture that it did not close for ten days, thus constituting a source of danger. In reference to the application of collodion recently recommended for the treatment of spina bifida, Dr. Brainard considers that when the walls are thick and firm, this may be safe and as effectual as other forms of compression. But when the covering is thin, the practice is dangerous by inducing ulceration. Instances of this are referred to; and Dr. Brainard regards the application of collodion in two cases treated by Dr. Gross as having given rise to the ulceration which followed, the iodine treatment, which was also adopted, having proved so far successful and safe that plastic lymph was deposited, while no dangerous symptom could be attributed to the operation itself.

V. *On Diabetic Cataract.* By M. LECORCHÉ. (Archives Générales de Médecine, May, June, and July, 1861.)

The following are the conclusions of M. Lecorché's memoir, in which he gives a minute history of the affection, and relates several examples.

"1. No doubt can prevail as to the existence of diabetic cataract. Its frequency, course, and development completely characterize it and constitute it as a well-defined morbid individuality. 2. It is soft or partially soft, its hardness being quite exceptional, and dependent upon circumstances as yet unknown. 3. It may be preceded by amblyopia, or by various forms of nervous affection; but it may also manifest itself without any precursory sign. 4. It is not met with in slight cases of diabetes, but forms a portion of the symptomatology of the grave form of the disease. 5. It only appears at an advanced stage of this, and justifies the prognosis of certain and more or less approaching death. 6. Unconnected with the changes undergone by the fluids of the eye (as saturation of the aqueous humour with sugar, its acidity, &c.), it may be regarded as one of the manifestations of a deep-seated deterioration of the economy, dependent doubtless upon the impoverishment of the nutrient fluids. 7. It requires for its removal a special mode of operating."

VI. *On Waterproof Bandages.* By Dr. A. MITSCHERLICH. (Archiv für Klin. Chir., vol. i. p. 457.)

The author points out how desirable it is to be able to expose with impunity plaster of Paris and other firm bandages, which are so highly useful in conservative surgery, to the prolonged action of warm water. In this way, limbs might be kept for days or weeks, the bandages having been fenestrated, in local tepid baths, a practice in high favour at some of the German Hospitals as a means of preventing pyæmia. Such bandages, too, would be of great utility in treating fractures in children, in whom the ordinary ones soon become softened and useless, owing to the action of urine. The prolonged tepid bath soon tells upon the ordinary plaster of Paris bandage, destroying its power of resistance, especially when largely fenestrated. The various additions which have been made to obviate the inconvenience, such as oil, dextrine, alum, or waterglass, have not met with much success. The necessary resistance may, however, be obtained in cases in which a very rigid bandage is not required, in the following manner. The plaster of Paris bandage is to be applied in the ordinary way, and then thoroughly dried, for which twenty-four hours or more may be sometimes required. A solution of from one to two ounces of shellac in one pound of alcohol is then applied by means of a pencil of charpie over the entire surface, as long as this continues to imbibe any of it. After some hours, when the alcohol has evaporated, more must be soaked in, if possible, and finally the whole is covered with a concentrated solution of the shellac in pure alcohol. The bandage on drying exhibits a shining, golden-yellow surface, and may be exposed to warm water for any time without this softening or penetrating it. One disadvantage is that the bandage so prepared requires forty-eight hours to dry before it can be put into the water; but then wounds do not usually commence suppurating before this. When greater solidity is required, as well as the possibility of at once placing the bandaged limb in warm water, the best Portland cement should be substituted for plaster of Paris. In order to secure the setting of this, however, as rapidly as plaster of Paris, it should be mixed, not with water, but with water-glass (silicate of soda dissolved in water), which is a clear fluid, having a specific gravity of 1·370. This may be diluted with an equal quantity of water, and used to wet the unbleached calico bandage, into which the powdered cement has been thoroughly rubbed. Over this is spread a layer of thin paste made by mixing the cement with some undiluted water-glass. If not made thin, this will harden too soon. The bandage thus formed is impenetrable to fluids, even at an elevated temperature.

Subjoined is a list of the principal articles on surgery which have recently appeared in the foreign periodicals, and to which, owing to limited space, we can only refer.

Abdomen, Zipp on injuries to the. (Deutsche Klinik, Nos. 7-18.)

Amputation.—Szymowski on partial amputation of the foot. (Langenbeck's Archiv, No. 2.) Discussion on Chopart's. (Bull. de la Soc. de Chirurgie, N. S.) Martrès on amputation at the shoulder-joint. (Mém. de Méd. Militaire, vol. iv. p. 498.)

Ankylosis of the Jaw, Esterle's case of operation for. (Omode's Annali, vol. clxxvi. p. 570.)

Aneurysm, Cases of employment of pressure in. (Bull. de la Soc. de Chir., N. S., vol i. pp. 187, 230, 476, 653.)

Artery, Ligature of the primary carotid. (Amer. Med. Times, April 20.) Ligature of the sub-clavian. (Bull. de la Soc. de Chir., N. S., vol. i. p. 130.)

Astragalus, Dislocation of the. (Bull. de la Soc. de Chir., N. S., vol. i. pp. 229, 280, 295, 307, 322, 344.)

Cold.—Esmarch on employment of cold in Surgery. (Langenbeck's Archiv, No. 2.)

Crystalline Lens, Fischer on dislocation of. (Archives Gén. de Méd., Jan.)

Dislocation of the Astragalus. (Bull. de la Soc. de Chir., N. S., vol. i. pp. 229, &c.) Dislocation of the femur; Gille on laceration of the capsule in. (Arch. Gén., April and May.) Cooper on old dislocation of the clavicle. (Amer. Journal of Med. Science, April.)

Ear.—Troeltsch on diseases of the outer ear. (Wurzburg Med. Zeitsc. vol. ii. No. 1.) Troeltsch on trephining the mastoïd process. (Virchow's Archiv, vol. xxi. No. 3.) Troeltsch on catheterism of the Eustachian tube. (Deutsche Klinik, Nos. 23 and 24.) Scholz on polypi of the ear. (Wien Wochenschrift, No. 27.)

Enterotomy, Tungel on two cases of. (Langenbeck's Archiv, No. 2.)

Elbow, Streubel on injuries to the, in children. (Prag. Vierteljahrs., No. 2.)

Fractures.—Smith on treatment of fractures by suspension. (Amer. Jour. Med. Science, April.) Bruns' cases of difficult union in fractures. (Deutsche Klinik, Nos. 14, 15, 17, 18, 19.) Swinburne on treatment of fractures by extension. (Amer. M. Times, March, 2nd.) Waeckerling's cases of remarkable fractures. (Langenbeck's Archiv, No. 2.) Duval on fractures after gun-shot wounds. (Gaz. des Hôpit., Nos. 7 and 10.) Lecomte on indirect fractures of the radius. (Archives Gén., Jan. and Feb.)

Hernia, Strangulated; Pitha on a remarkable case of. (Wien Wochenscrift, Nos. 24, 25, 26.) Gosselin, clinical observation on. (Gazette Méd., Nos. 13, 16, 17.) Streubel on taxis in. (Prag. Vierteljahr., No. 1.) Klob on hernia retro-peritonealis. (Wien Wochenblatt, No. 24.)

Intestinal Obstruction, Cases of. (Mém. de Méd. Militaire, vol. iv. p. 461.)

Joints, Bauchet on foreign bodies in the. (Moniteur des Sciences Méd., No. 11.) Volkmann on suppuration of the. (Langenbeck's Arch., No. 2.)

Hip-joint Disease, Discussion on treatment of. (Amer. Med. Times, vol. ii. Nos. 17–26.)

Patella, Rupture of ligament of the. (Bull. de la Soc. de Chir., N. S., vol i. p. 48.)

Penis, Demarquay on lesions of, during copulation. (Moniteur des Sciences Méd., Nos. 41, 43.)

Polypi, Naso-Pharyngeal, Discussion concerning. (Bull. de la Soc. de Chir., N. S., vol. i. pp. 7, 13, 140, 212, 258, 419.)

Radius, Separation of epiphysis of the. (Bull. de la Soc. de Chir., N. S., vol. i. p. 535.) Lecomte on indirect fracture of the. (Archives Gén. de Méd., Jan. and Feb.)

Spina Bifida.—Cases and discussion. (Bull. de la Soc. de Chir., N. S., vol. i. pp. 387, 396, 402, 437, 483, 612, 664.)

Syphilis, Vaccination in. (Bull. de la Soc. de Chir., N. S., vol. i. pp. 404–414.) Nélaton on muscular tumours in. (Gaz. des Hôp., No. 59.)

Syphilization, Sigmund on. (Wien Med. Wochenscrift, Nos. 7 and 8.)

Thyroïd Gland, Hypertrophy of the. (Actes de la Soc. des Hôpitaux, fascicule 5.)

Urinary Organs.—Schuh on cases of fatal puncture of the bladder. (Wien Spital's Zeitung, Nos. 3- 6.) Warren on perineal section. (Amer. Jour. of Med. Sciences, April.) Dolbeau on internal urethrotomy. (Bull. de Thérap., March.) Sédillot on internal urethrotomy. (Mém. de Méd. Militaire, vol. iv. pp. 28–420.) Bourquet on external urethrotomy. (Bull. de l'Acad. de Med., vol. xxvi. No. 15.) Slade on a morbid condition of the neck of the bladder. (Boston Journal, vol. lxiv. p. 33.) Maisonneuve on internal urethrotomy. (Moniteur des Hôp., No. 70.)

Varices, Verneuil on deep-seated, of the leg. (Gaz. Hebdomad., Nos. 27, 28.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D., F.R.C.P.
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I. The Unimpregnated State.

1. *On the Treatment of recent Inflexions of the Uterus by means of the Uterine Sound.* By Dr. Massmann, of St. Petersburg. (Monatsschr. für Geburtsk., June, 1861.)
2. *A Case of Peri-uterine Hæmatocele; with Contributions to the History and Diagnosis of this Affection.* By Dr. Alfred Hegar. (Monatsschr. für Geburtsk., June, 1861.)
3. *Retro-uterine Hæmatocele: formation of a Recto-Vaginal Fistula: Recovery.* By Dr. Gauchier. (Gazette des Hôpitaux, 1860.)
4. *The Origin of Ovariotomy.* By Professor Gross. (Amer. Med. Monthly, January, 1861.)
5. *Case of Malignant Ulceration (?) of the Cavity of the Uterus.* By Professor Virchow. (Monatsschr. für Geburtsk., May, 1861.)

1. Dr. Massmann, after adverting to the difficulty in restoring the uterus to its normal state after long-standing inflexions, dwells upon the practicability of treating this condition more successfully if seen at an early stage. The greater number of cases, he says, arise after labour or abortions, when their origin is often overlooked. After labour, the uterine tissue being relaxed and enlarged, is prone to bend in one direction, and this bending becomes an obstacle to perfect involution, thus increasing the evil. But if means be taken to restore the uterus to its straight condition before the tissue at the seat of the inflexion has undergone abnormal change, the cure is comparatively easy. Dr. Massmann relates four cases in which the inflexion was so marked that the fundus uteri was thrown backwards and downwards, so as to be on a level with the os, and to protrude the hinder wall of the vagina, and which were cured by replacing the fundus, by passing the uterine sound from time to time, and keeping the patients in the dorsal decubitus.

2. Dr. Hegar contributes an instructive case of peri-uterine hæmatocele. A girl, aged eighteen, had menstruated from the age of sixteen, always with acute pains in the abdomen and sacral region. In 1859 she had a severe chill after walking in a brook with naked feet. Violent pains in the abdomen set in. From this time the menses returned regularly, but were attended with considerable pains; and the patient in sitting was inconvenienced by a swelling. On the 17th May, 1860, menstruation returned; the pains were intense; and when, after a few days, the discharge of blood ceased, the patient could not pass water. Symptoms of prostration followed. A hard tumour was felt, rising four or five inches above the symphysis, filling the right side, and passing a little over to the left of the linea alba. It was slightly painful to pressure. In the vagina was a conical projection appearing a little way beyond the vulva. This projection was the lower end of a tumour, which stretched along the entire right side of the pelvis backwards and upwards. The left pelvic space was free. The os uteri could not be reached. Fluctuation was communicated to the conical projection in the vagina by pressure on the abdominal swelling. The catheter easily passed into the bladder, but could not be felt through the vagina, because the swelling extended between the anterior wall of the vagina and the bladder, as well as behind. On the 17th September, the swelling was considerably larger, reaching almost to the navel and more over to the left side. The patient's condition getting gradually worse, an exploratory trocar was pierced through the conical projection in the vagina, and the opening was enlarged. A quantity of thick sanguineous fluid escaped, the abdominal swelling sinking. The discharge and simultaneous diminution of the swelling continued during the night, and the vaginal portion of the uterus could be felt. The patient recovered.

Dr. Hegar appends an excellent historical *résumé* of the subject, and draws the following conclusions:

(1.) Under the name peri-uterine hæmatocele, effusions of blood into the female pelvic spaces are described, which are very various, both as to their seat and their origin.

(2.) The seat of the effusions may be the anterior or posterior pelvic spaces, inside or outside the peritoneum, between the peritoneum and the pelvic fascia.

(3.) Extravasations of this kind may happen during pregnancy, labour, the puerperal period, as well as during menstruation.

(4.) The differential diagnosis between intra-peritoneal and extra-peritoneal hæmatocele is not always to be made with certainty. Deep downward projection of the swelling marks an extra-peritoneal effusion when it is observed from the commencement, or where the tumour is perceived in front of the uterus. As to the dislocation of the uterus, it must first be ascertained whether alterations in the position of the vaginal portion have not been caused by intra-peritoneal effusions. The livid colour is not always present in hæmorrhage outside the peritoneum. A very irregular boundary of the tumour, which can be felt at the same time around and behind the vaginal portion, indicates that its seat is outside the peritoneum.

3. The case of retro-uterine hæmatocele related by Dr. Gauchier is interesting from the occurrence of a perforation of the rectum, and the establishment of a fistula between rectum and vagina. Seven days afterwards, fæces passed by vagina, the discharge of blood ceased, not to return. The fistula closed spontaneously; the menses returned regularly, and the patient made a good recovery.

4. Professor Gross claims for Dr. Ephraim McDowell, of Danville, Kentucky, the merit of being the first to practise ovariotomy. The first operation was performed upon Mrs. Crawford, of Kentucky, in December, 1809. Dr. McDowell is said to have operated in thirteen cases, with success in at least eight.

5. Virchow related to the Berlin Obstetrical Society the history of a patient, communicated to him by Dr. Kugelman, of Hanover. The subject was a woman, aged fifty-eight, who had borne several children, when she began to lose small quantities of blood from the uterus; she suffered pains, at first dull, afterwards most acute, in the right side, and during the last six months before death had several severe shivering fits with delirium. The autopsy revealed in the right ovary a small cyst. The uterus was healthy in the cervix, enlarged in the fundus, and contained in the hinder wall an apparent abscess; the mucous membrane of the fundus was eroded. Virchow was of opinion that it was not a primary abscess, but that there had been a large ulcerated spot which had spread into the substance of the uterus. The condition of the inner surface of the uterus resembled villous cancer.

II. Labour.

1. *The Obliquity of the Fœtal Head in the Mechanism of Parturition.* By Dr. J. Matthews Duncan. (Edinburgh Med. Journ., August, 1861.)
2. *On the Influence of the Anterior Wall of the Pelvis upon the Mechanism of Labour, especially in Pelvic Contractions in minor degree.* By Dr. Crüger. (Monatsschr. f. Geb., January, 1861.)
3. *On a rare Mechanism in Face-Presentations.* By Prof. Braun. (Monatsschr. f. Geb., February, 1861.)
4. *Quinine as a Parturifacient.* By Dr. John Lewis. (Amer. Med. Monthly, February, 1861.)
5. *An effort to shorten the Duration and diminish the Pain of the first Stage of Labour, with a record of* 147 *Cases.* By Dr. Fordyce Barker. (American Med. Monthly, June, 1861.)
6. *Post-partum Hæmorrhage.* By Prof. George Mendenhall. (Amer. Med. Monthly, January, 1861.)
7. *Contributions from the Obstetric Clinique at Würzburg (Puerperal Fever).* By Otto V. Franque. (Monatsschr. f. Geb., May, 1861.)
8. *A Case of Expulsion of the Fœtus after Death of the Mother.* By Dr. Richter. (Casper's Vierteljahrschr., 1861, and Monatsschr. f. Geb., June, 1861.)
9. *Uncompleted Perforation and Cephalotripsy; Birth of Child after Mother's Death.* By Dr. Crüger. (Monatsschr. f. Geb., January, 1861.)

1. Dr. Duncan contends that Naegele's description of the oblique descent of the head into the pelvis is not correct. Dr. Duncan says that the axis of the

child's body, of the uterus, and of the brim of the pelvis, are represented by the same line; therefore, Naegele's obliquity of the child's head implies lateral flexion, or the approximation of its left ear to its left shoulder, a condition which Dr. Duncan denies. The head enters the brim, he says, *directly*, not obliquely.

2. Dr. Crüger, in an interesting memoir "On the Influence of the Anterior Wall of the Pelvis upon the Mechanism of Labour," remarks that the consideration of the shortest pelvic diameter is by no means the most important, and that the relation of the anterior wall has been entirely overlooked. He relates three cases, in one of which, with a conjugate diameter not exceeding two inches and three quarters, a living, nearly full-grown, child was born by the natural efforts; a second, with a conjugate diameter of three inches and a half, in which a premature child was born with its cranium burst; and a third case in which, with a conjugate of three inches, delivery was impossible. In examining a number of skeleton-pelves, it will be seen that the anterior wall is very variously formed. Sections through the symphysis are either nearly parallel as to their anterior and posterior surfaces, or concave within, or, as in the majority of instances, the anterior wall in its upper third, sometimes in its upper half, is bent forwards. These forms must exert a considerable influence upon the mechanism of labour, especially when the pelvis is slightly contracted. In normal labour, Dr. Crüger describes the direction of the force of the uterine contractions to take a line forming an angle with the level of the brim, which tends to throw the head upon the anterior wall of the pelvis. This, however, presents no obstacle to the descent of the head in ordinary circumstances.

3. Professor Braun describes the following modifications of the mechanism of face-presentations:

(1.) Face-presentations change into occipital-presentations; this scarcely ever happens, except during the period of dilatation.

(2.) The normal rotation of the chin from backwards forwards may first be produced by powerful pains whilst in the vulva.

(3.) The commenced rotation may not be completed, but the face reaches the pelvic floor in an oblique direction, and thus passes through the ostium vaginæ. This rather frequently happens.

(4.) The face leans with the chin upon the coccyx, whilst the mouth and nose press upon the floor of the pelvis, the forehead and greater fontanelle remaining visible in the ostium vaginæ. The calvarium is flattened through the pressure of the anterior wall of the pelvis, and first during the birth the forehead and nose, then the upper lip and chin, pass the perinæum. This is very rare.

(5.) The forehead remains pressed against the anterior wall of the pelvis, the eyes and nose become first visible at the vulva, then the remaining part of the face passes over the perinæum, and lastly, the forehead is driven under the symphysis.

The author then describes a case in which a mature child presenting by the face was born with the chin on the perinæum. After the birth of the head, the back of the child remained directed forwards. The child was dead. In another case the delivery was effected by forceps. The root of the nose first became visible, the chin passed over the perinæum, and then the calvarium and occiput came under the symphysis in completely transposed mechanism. The child was alive.

4. Dr. John Lewis extols the parturifacient action of quinine. Given in ten-grain doses in cases where the os uteri is rigid, employing at the same time

dry cups to the sacrum and warm foot-baths, he looks with confidence for the *speedy* acceleration of labour.

5. Dr. Fordyce Barker, of New York, gives a tabular view of 147 cases in which he resorted to the internal administration of belladonna to shorten the first stage of labour. When the patient was plethoric, Dr. Barker employed the following formula:—Belladonna (extr.), gr. viij.; tartar emetic, gr. iij.; syrup of orange, ℥ij.; tincture of orange, and water, of each ℥j. A tea-spoonful three times a day, beginning several days before the accession of labour.

In other cases he used the following:—Compound tincture of cinchona, ℥iij.; syrup, ℥j.; belladonna, gr. viij. To be taken in like doses.

On looking over Dr. Barker's table, it would appear that the first stage of labour was really shortened, and suffering lessened.

6. Dr. Mendenhall relates a case of profuse uterine hæmorrhage after labour, in which the introduction of the hand, ice, and other means failed. He injected, by means of a catheter introduced to the fundus, about three ounces of saturated solution of persulphate of iron. The injection was kept in the uterus for a few minutes by the hand at the neck. It produced no pain, and not another drop of blood was discharged. The patient recovered without an unpleasant symptom.

7. The annals of the lying-in hospitals supply us with almost all our practical knowledge of the heaviest scourge of parturient women, puerperal fever. The obstetric clinique at Würzburg contributes some lamentable experience. During the first six months of 1860, 188 women only were delivered; 44 were attacked with fever, of whom 14 died. In 20 cases the labour was terminated artificially—namely, fourteen times with the forceps, four times by turning, and twice the child was extracted by the breech. In six of the forceps-cases the mothers fell ill, three dying; two of the subjects of turning also died. The influence of the epidemic was not extended to the children. The character of the epidemic was mostly pyæmic at the commencement, passing into blood-dissolution. The illness began from the fifth to the seventh day with shivering, heat, and quickened pulse. The pulse exhibited remarkable variations, being sometimes 130—140 one day, and sinking to 90—100 the next. The shiverings were frequently repeated, so that the entire disease assumed an intermitting character. The average duration of the disease was from twenty to forty-four days. The treatment consisted chiefly in the administration of quinine and opium. Autopsy revealed purulent serous exudation in the abdominal cavity; diphtheritic exudation or gangrenescence of the cervical and uterine cavities; pus in the Fallopian tubes in two cases; the ovaries enlarged, with serous infiltration in three cases; suppuration of the cellular tissues around the uterus and in the pelvis in three cases; pus in the veins of the uterus and pampiniform plexus in two cases; pus in the lymphatic vessels in one case; pus in the knee-joints in two cases; thrombi in the veins in two cases. The spleen was enlarged in all cases excepting in two; there was parenchymatous nephritis twice; endocarditis and diphtheritis, with perforation of the intestine, in one case.

8. Dr. Richter relates one of those rare cases in which the fœtus appears to be expelled from the mother's body after death. A woman, at the end of her seventh month, was brought home from field-labour, complaining of spasmodic pains in the region of the heart. When examined by a midwife, the os uteri was closed, and there was no sign of labour. She died stertorous next morning. The corpse lay till evening on the bed, and no discharge from the genitals was noticed during the washing and laying out. About sixty hours after death,

some watery discharge was noticed. Putrefaction had set in. Next day, on placing the body in the coffin, a seven-months' child, somewhat decomposed, with placenta, was found between the thighs. The mother's body was distended with gas. The expulsion had been effected through the pressure created by the gases of decomposition, without any preparatory labour-act. The case is represented to be another confirmation of Casper's explanation of delivery after death.

9. In connexion with the foregoing case, we extract the following, related by Dr. Crüger to the Berlin Obstetrical Society. On the 5th June, 1859, a woman, aged forty, was in labour; the conjugate diameter measured three inches. The head was perforated, and vain attempts were made to extract by means of Scanzoni's cephalotribe; the strong bowing of the blades prevented their application. Turning was effected with much difficulty; but still delivery could not be completed. The patient died collapsed next day. Autopsy was performed twenty hours after death. When the body was taken off the bed the child was found, with its face turned upwards, between the mother's thighs. Decomposition of the child had made considerable progress. The body of the mother was decomposed to a degree scarcely usual after several weeks. The abdomen was enormously distended with stinking gas. The uterus, flaccid, was much distended by gas. It was concluded that in this case the child was expelled by the pressure of the gas, and not by any action of the uterus. Dr. Crüger believes that the rapid putrefaction was chiefly due to the decomposition of the blood during life and the hot weather, but also in considerable degree to the change wrought by the continued inhalation of chloroform during four hours, which latter influence he has observed in other similar cases.

III. The Fœtus.

1. *On Umbilical Hæmorrhage.* By A. N. Talley. (Charlestown Medical Journal, Nov., 1860.)
2 *The Formation of Knots upon the Umbilical Cord.* By Dr. Read. (Boston Medical and Surgical Journal, June, 1861.)
3. *Strangulation of the Fœtus through Twisting of the Umbilical Cord.* By L. Bartscher. (Monatsschr. für Geburtsk., May, 1861.)
4. *Congenital Cystic Tumour of the Sacro-perineal Region (obstructing Labour).* By Professor Martin. (Monatsschr. für Geburtsk., July, 1861.)
5. *Congenital Sacral Tumours.* By L. Bartscher. (Monatsschr. für Geburtsk., Feb., 1861.)
6. *A Case of Deficient Formation of Bone in a New-Born Infant.* By Uhle. (Monatsschr. für Geburtsk., June, 1861.)

1. Dr. Talley reports three cases of troublesome umbilical hæmorrhage. Two were treated in the usual manner, not omitting Dr. Churchill's plan with plaster of Paris. Both died. The third case was treated by the application of liquor of persulphate of iron. The bleeding, which had been rapid, was instantaneously and permanently arrested.

2. Dr. Read has communicated to the Boston Obstetrical Society a paper, in which he maintains that the knots found on the cord at birth are always tied by the passage of the child through a loop lying loose at the os uteri at the time of delivery.

3. Bartscher's case of strangulation of the fœtus by knotting of the umbilical cord is interesting in connexion with the late Dr. Montgomery's theory of spontaneous amputation of the limbs of the fœtus in utero. A woman n four labours had each time the cord twisted round the child's neck. In her fifth

pregnancy, she went on well to the seventh month; then she complained of sharp pains in the right lumbar region, during which the child's movements became unusually lively. These pains only lasted a few days; when they ceased the fœtal movements ceased too. The patient next complained of extreme weakness, and had repeated shiverings. Labour came on at the end of nine months. The child was dead, being generally denuded of epidermis. The cord, twenty-five inches long, was mostly riband-like; it surrounded the neck twice, and then was tied in a true knot. This knot was hard to untie; it had so tightly drawn the coils of cord, that the soft parts of the neck were compressed against the spine. The fœtus was much wasted.

4. Professor Martin describes a case of labour obstructed by a large tumour attached to the sacrum of the child. At first the labour was supposed to be rendered tedious by the excess of liquor amnii, for fifteen quarts of foul-smelling waters were discharged. The head and trunk were delivered without difficulty, but although the midwife pulled with great force for some time, the breech could not be extracted. She said the child had breathed several times, but when Dr. Rosenthal, who communicated the history to Professor Martin, arrived, it was dead. The extraction was effected with great difficulty. There was a large quantity of serous fluid in the abdomen. From the lower part of the sacrum depended a large tumour, projecting beyond the nates, covered by the skin of the back, above and below by that of the genitals, perineum, and thighs. A section of the tumour showed it to consist of a medullary, reddish-grey, very vascular substance, traversed by bands of connective tissue. This connective-tissue framework served as a stroma for the contained medullary mass, in which again were a crowd of various-sized cysts. Cretaceous deposits were found in considerable number throughout the mass.

5. Dr. Bartscher gives an useful systematic account of congenital sacral tumours. Their seat and contents are, he says, various. 1. In one tumour there has been observed ventral hernia, that is, a hernia of the contents of the abdomen projecting between the latissimus dorsi and the obliquus externus. 2. Cysts with sero-lymphatic fluid or collections of cystoïds or cysto-sarcomata, steatomata, fungus, &c. Most frequently the cysto-sarcoma in the sacral region consists of a fibroïd, vascular mass, in which isolated cysts are enclosed. These cysts are either simple, or the inner wall of the mother-cyst contains small free cysts on projecting stalks, or small prolyp-like tumours (cysto-sarcoma proliferum); or there spring from the smooth thick wall of the cyst condylomatous excrescences (cysto-sarcoma phyllodes). 3. The sacral tumour may contain an intra-fœtation. 4. Hydrorachis in rare cases forms a sacral tumour; this may exist either with or without spina bifida.

Dr. Bartscher has met with but one case of sacral tumour. The following is a summary: In April, 1837, a primipara was delivered by forceps of a strong boy. A considerable tumour stretched from the right sacro-iliac synchondrosis and the first false vertebra of the sacrum, over the sacral notch to the left, and below over the os coccygis to the anus. From this attachment, as a stalk, the fundus of the tumour reached to the hip-joint. It was hard, without fluctuation, at the lower part knotty; it was not painful or transparent, nor could it be diminished by pressure. The fundus began to excoriate, so that on the eleventh day Dr. Bartscher determined to remove the tumour. There was considerable hæmorrhage from these arteries. The tumour weighed six ounces; on section it presented a firm fibrous tissue, in the midst of which were several cysts containing a serous turbid fluid. The child recovered.

6. Uhle relates a case in which a dead but mature child was born, in which only a small plate represented all the bones of the head. The bones of the

face were also defective. The ribs were mostly broken. The extremities were distorted, much too short. Everywhere the bones were easily broken on motion, and a piece of bone could be ground to powder with the nail. In other respects the body was well-formed and of normal size. The food of the woman during gestation had consisted exclusively of potatoes and cider; she had had no meat or greens, and little bread.

The following papers, in addition to those analysed above, may be referred to as of interest:—

Studies on Pemphigus of the Cervix Uteri. By Dr. JOULIN. (Gazette des Hôpitaux, 1861.)

Clinical Lectures on Abdominal Collapse, or Shock from Abdominal Disease. By ROBERT BARNES, M.D. (Lancet, August, 1861.)

On Fibrous Tumours of the Uterus: a Thesis presented at the Concours for Aggregation in the Section of Surgery and Midwifery, to the Faculty of Medicine. (Paris, 1860.)

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MEDICAL INTELLIGENCE.

Incurables and the "Workhouse Visiting Society."

How in a measure to alleviate the distress, and generally to ameliorate the condition of the large class of our fellow-men and women to which, in every parish and union of the land, the melancholy epithet of incurable is attached, is a question which the above-mentioned Society, amidst its numerous good works, is attempting practically to solve. The Society, through its active and benevolent Honorary Secretary, Miss Louisa Twining, has addressed to the various Boards of Guardians a letter suggesting a plan by which the 30,000 or 40,000 *patients* (not properly paupers), who, owing to cancer, consumption, and other incurable affections, annually die in our workhouses (which from their construction and general condition are totally unfit, as a rule, to meet the sufferings of so large a section of the community*), may have afforded to them all the

* To what extent this is the case may be learnt from a very touching communication on "Destitute Incurables in Workhouses," read at the Social Science Meeting at Glasgow, September, 1860, by Miss Elliott and Miss Cobbe. (Transactions, p. 836.) Miss Cobbe has also lately read a communication on the same subject (which, however, we have not seen) at this year's meeting of the same society, held at Dublin; and Miss Louisa Twining has just read a kindred one at the meeting of the British Association held at Manchester (which also has not come before us).

necessaries, and minor but substantial comforts, which are contemplated in the regular Incurable Hospital, such as that maintained at Putney.* The following quotation from the letter of the Society explains the whole project:

"Let all persons in the workhouse suffering from acute and distressing disease, such as dropsy, consumption, cancer, &c., be placed by themselves in wards apart, to be called the wards for male and female sick and incurables. In these particular wards let private charity be admitted to introduce whatever may alleviate the condition of the inmates.

"On the passing of such rules by any board of guardians it would follow that the members of the Workhouse Visiting Society would raise and employ the moderate subscriptions needful to convert these wards into suitable and comfortable hospitals for incurable.† Under the sanction, and with the co-operation of the surgeon, and in concert with the other officials, they would provide good beds for the bedridden, easy chairs both for those who cannot lay down and for those who ought for some hours each day to leave their beds, the salaries of trained nurses if required, and such other trifles, as lemonade, extra tea, books, &c., as may seem desirable.

"It is submitted that this little plan possesses the following recommendations:—

"While relieving much suffering at small cost, it involves no injustice to the ratepayers, as it does not call on them for any extra expense whatever.

"It cannot encourage pauperism, seeing that no person will incur mortal disease to profit by it, and if it be suspected that any patient could be supported by his friends it would remain in the power of the board, as at present, to deny him admission after examination by the relieving officer.

"In conclusion, as it is undoubtedly in the power of each local board of guardians to frame for the regulation of its own workhouse such byelaws as are above respectfully suggested, we beg to urge your favourable consideration of them. Should you be willing to accede to the plan, we hereby offer the services of our society in carrying it into execution in raising and applying the needful subscriptions. In unions where no lady visitors of the society at present reside, it will no doubt be possible for the chaplain or guardians to find other ladies willing to carry out the plan with the sanction and approval of the board."

This scheme we believe to be eminently judicious and highly feasible, and we heartily wish it God-speed; but experience teaches us how impracticable Boards of Guardians are.‡ Of this we had an example when the Nursing Committee of the Epidemiological Society so fruitlessly addressed them, under sanction of the Poor Law Board, on the subject of training qualified nurses in the workhouses for the sick within and without their walls.§

* In addition to the Putney Incurable Hospital, another is about, we understand, to be founded in the metropolis, under the auspices of Lord Raynham, the Lord Mayor, and Mr. G. Moore. There is, doubtless, urgent need of several such institutions.

† In connexion with the plan for destitute incurables here detailed, a fund has been opened for the assistance of visitors in those Unions where the proposal may be adopted and yet the local subscriptions prove inadequate. To this Central Fund for Destitute Incurables one gentleman has given 100*l.*, and others have promised further contributions.—Communications to be forwarded to the Secretary of the Workhouse Visiting Society, 3, Waterloo-place, S.W.

‡ The result of this application on the part of the Workhouse Visiting Society has so far been, we understand, satisfactory—that is, in six or seven unions the proffered help has been accepted for carrying out the suggestions made, and in other cases the authorities have been led to carry them out to a more or less extent without adventitious pecuniary help.

§ We take the present opportunity of calling special attention to an exceedingly interesting, plain-spoken pamphlet, on 'Nurses for the Sick, with a Letter to Young Women,' by Miss Louisa Twining, in which she points out the dignity and desirableness attached to the vocation of "a nurse," showing the pressing want of nurses being multiplied in England, and strongly recommending the adoption of this calling, even on utilitarian grounds,

Hospital Statistics.

We are glad to be able to announce the projection of a scheme by which there is a "possibility" of our hospital statistics throughout the country being ultimately carried out upon a wide and uniform plan. Such a scheme would obviously not only tend to ensure greater accuracy of detail, but considerably increase the value of the general results; and any one who considers and deplores the extent to which valuable phenomena are each day utterly lost to science for the want of being duly gleaned, will welcome any proposition having as its object the preservation and co-ordination of medical data.

A plan of hospital statistics, in which uniformity in classification, nomenclature, &c., is to be observed, has been suggested by the Sanitary Section of the International Statistical Congress (founded upon a recommendation submitted to the Congress by Miss Nightingale); and in accordance with this a meeting has been held at Guy's Hospital for the purpose of considering the advisability of the suggestion, and to determine (if thought desirable) how it may best be carried out. At this meeting delegates from eight of our large London Hospitals were present, and came to the following resolution—viz.: "That it was desirable to recommend that the metropolitan hospitals should have a book of registration wherein an appointed officer should enter certain particulars relative to the mortality and sojourn of each patient admitted; and that the Statistical Society of London, or some other suitable body, should be asked to collect and publish such details furnished by the several hospitals." It was also determined to encourage every hospital to make its own special annual report, containing statistics of disease and treatment.

We strongly hope that the result of this meeting may be the fair trial of the suggestions thrown out, and that this trial may lead in the end to something even more comprehensive than that which the suggestions embrace. Much certainly has been done of late as to the collecting of rich stores of facts in our public hospitals, and by general contributions to the press: much yet remains to be done. The profession and the public have no right to be content until some practical system of universal application shall have been set on foot, according to and by which as many as possible of the medical and surgical cases in our hospitals, infirmaries, workhouses, dispensaries, &c., as well as more of the many cases in private practice, may be efficiently and uniformly recorded and digested, and rendered accessible for collation. Such a scheme should of course, if carried to perfection, have direct reference to the various "*modes of treatment*" resorted to.*

Medical Literary Society.

Under this title an association has been formed,† with the object of discussing "questions of public medical, medico-ethical, and medico-jurisprudential

in place of that mostly unhealthy and miserable, and too often morally dangerous one of milliner and dressmaker—one which also is daily becoming more precarious by reason of the increasing popularity of the "sewing machine." The pamphlet contains a slight sketch of the origin and progress of our institutions concerned in the training of nurses, and also some excellent observations as to the possible means of further meeting the general want above alluded to.

* Here we cannot help adverting to the utter *waste* of whole treasures of medical facts which occur in that extensive field of medical experience afforded by the infirmaries or sick wards of union workhouses. Nothing is known of them outside the walls of the institution; and indeed comparatively nothing is known, as far as we can learn, of the entire state of the workhouse inmates, sick or well, to the various ratepayers by whom their support is maintained. We shall not apologize for reminding our readers of some observations which we made on Dr. Milroy's statements on the subject in the April No. of this Review, p. 453.

† Mainly under the auspices and through the zeal of Dr. Sieveking.

interest," special reference being had "to topics on which it is expedient to establish a clear understanding between public and professional opinion, as represented by the medical, legal, and general press. Communications from members to the Society are to be in the forms of papers to be read, or of resolutions definitely worded and supplemented by oral tradition. The following is a list of those eligible for ballot as members:—Editors of medical journals and other periodicals, past editors, literary gentlemen, physicians and surgeons occupying public positions, and other such members of the learned professions as may be able to further the general objects of the Society.

The president is to be elected annually, and one of the secretaries must of necessity be a medical man.

Such an association, if actively worked, is calculated to be of the utmost service to all classes of men, having earnest of an abstract and practical benefit, by affording an immediate opportunity to the various professions of gathering actual needful information from each other; and at the same time promising good by bringing minds of opposite characters, and variously moulded by different methods of training, into direct and energetic contact.

It is not too much to anticipate that the periodical meetings of members of our profession with literary men and influential representatives of other vocations, and free discussions upon points of common interest, in a manner contemplated by the rules of the Society, will yield not only much social gratification, but also materially tend to prevent and remove those prejudicies, fallacies, and misunderstandings which, partly real and partly imaginary, are wont at times to arise between the medical profession and the general public.

The present secretaries of the Society are Mr. Hume Williams, barrister-at-law, of Johnson's-buildings, Temple, and Dr. Stone, of Vigo-street, to whom communications respecting the Society are to be sent.

Royal Medical and Chirurgical Society of London.

At a special meeting of this Society it has been unanimously resolved: "*That the council from time to time appoint committees from amongst the Fellows of the Society, for the purpose of investigating questions of scientific medical interest; that such investigations be carried on at the expense of the Society, and that the reports of such committees be published in the 'Proceedings' or 'Transactions,' as the council may think fit.*"

It cannot, we think, be contested that the view taken by this Society as to the *rôle* which it is entitled and ought to play (as intimated by the abovenamed determination) is a correct and important one: it may, indeed, if efficiently carried out, along with other advantages, lead to the Society's becoming that responsible centre to which many doubtful questions of public interest, involving nice scientific and jurisprudential considerations, may be referred, of which in several high quarters a want has been experienced on more than one occasion. We need hardly remind our readers that a system of referring any important communication, or the recommendation of any novel mode of practice, &c., to select committees before general sanction or approval is conceded, has been long adopted by many foreign societies, and is found to work well. We only hope that in the present case the members of the suggested committees may not fail to work together as permanently as in the societies of other countries, and thus the scheme may not drop through for want of co-operation, as was found to be the case when, some years ago, an analogous mode of work was instituted, and to a certain degree set on foot, by the Pathological Society of London.

Quarantine.

Referring to an article in the previous number of this Review, in which the attention of the profession was called to the abuses and inefficiency of quarantine, as at present conducted, we are glad to find that the subject has again been brought under the notice of the National Association for the Promotion of Social Science recently held in Dublin, and that resolutions were there passed which, if acted on by our Government, cannot fail to be productive of good.

At the same time consideration was given to another important matter: the defective sanitary state of ports and of merchant-shipping, and of the condition of the crews, amongst whom there is much more sickness and a greater mortality than is commonly supposed.

Dr. Milroy, as heretofore, took a lead in these inquiries. He introduced them by reading an elaborate report prepared by the committee appointed at Liverpool three years ago, and which we are glad to find has been ordered to be printed by the House of Commons. On its appearing, we shall not fail to notice it, convinced as we are that, like other great evils of long standing to which the world has become accustomed, it is only by perseverance and importunity those of quarantine can be abolished.

Proposed Amendments of the Scotch Lunacy Act.

A meeting of the medical profession for the purpose of considering such amendments, has been held in the Hall of the Royal College of Physicians, Edinburgh, Dr. Alexander Wood, in the absence of Professor Christison, being in the chair. In his opening speech the chairman alluded to the active interest which, during the passing of the Lunacy Act, Dr. Christison had showed therein, specially mentioning the suggestion which he had made regarding the "emergency certificate," a feature which distinguished the Scotch measure from the English, and which enabled them to put dangerous and urgent cases of lunacy under immediate control, without waiting for the formalities of a sheriff's warrant, and which therefore gave them a greater power of promptness of action and of control over the patient in the earlier stages, than they could by possibility have had but for the introduction of this certificate.

The following resolutions were proposed and passed at the meeting, after considerable discussion on some of them:

"1st. That the present medical certificate is unsuitable. That it should simply bear that the undersigned medical men separately visited and examined (A. B. on such a date), and found him to be of unsound mind, and requiring confinement in an asylum. That this certificate, coming from two qualified practitioners, appears to be amply sufficient, and to require no statement of facts to be appended to it."

"2nd. That the period during which a person may be confined on an emergency certificate is too short, and should be extended from twenty-four hours to three days."

"3rd. That many cases of excessive intemperance depend on disease, and constitute a form of insanity. That such cases cannot be treated without confinement more or less strict. That in the present condition of the law such treatment is frequently unattainable. That some cases of the kind require treatment by confinement, not different from that enforced on other insane persons. That for many more a different system of treatment is desirable. That although such a system of treatment has already been established in various institutions in Scotland, into which persons are admitted with their own consent, yet it seems necessary in certain cases to afford the means of enforcing admission into such institutions, and that such institutions should be licensed subject to the jurisdiction of the Lunacy Commissioners, and conducted under such regulations as the Act may direct."

"4th. That the existing form of the sheriff's jurisdiction in cases of insanity is objectionable, as tending to delay and obstruction in the admission of cases of urgency into asylums, and also as interfering with the physician's province, which is to judge of the circumstances under which the treatment in asylums is required; and that the security of the public requires only that the competency and good faith of the medical men signing the certificate should be placed beyond suspicion. That the Lord-Advocate be requested to take these circumstances into his consideration, with a view either to the amendment of the sheriff's jurisdiction, or to its being replaced by some other provision for accomplishing the object in view."

"5th. That a memorial embodying the opinions of the committee be presented to the Lord-Advocate; that a committee be appointed to prepare this memorial; and that in the event of his Lordship agreeing to introduce into a Lunacy Amendment Bill provisions in accordance with these general principles, the committee be authorized to confer with his Lordship on the subject."

A committee was accordingly appointed to carry out the objects of the resolutions.

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