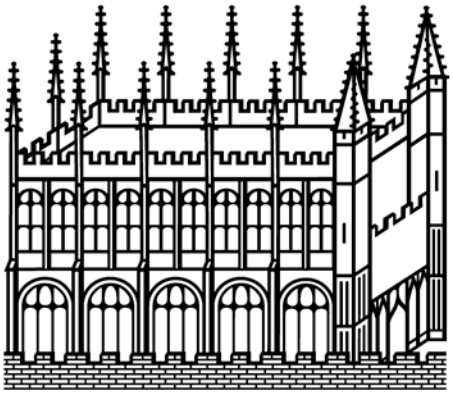


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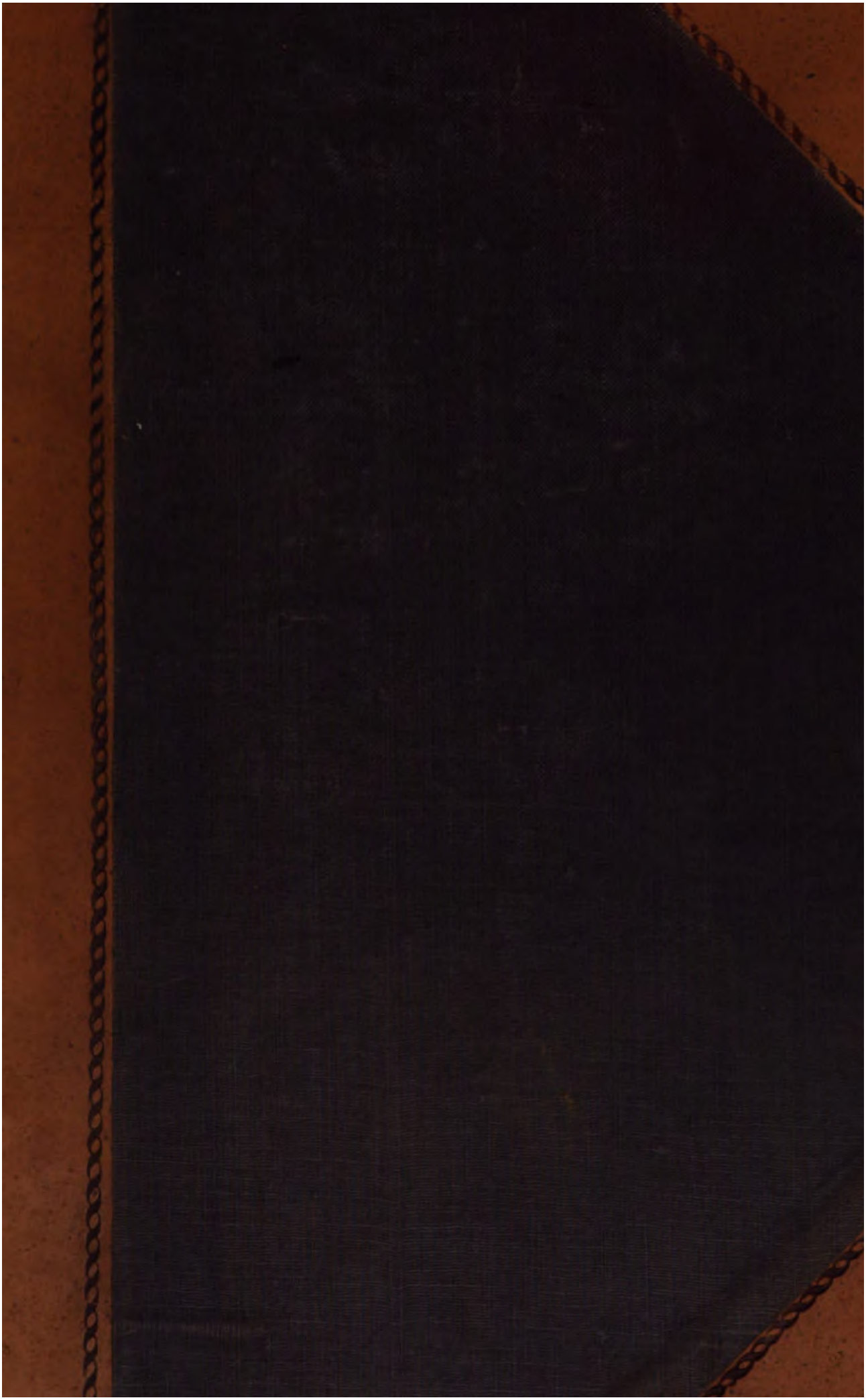
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BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

JULY, 1867.

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1. *Eighth Report of the Medical Officer of the Privy Council*, 1865. 'Blue Book.' 1866.
2. *Report of the Committee on Cholera, Royal College of Physicians of London.* Containing Instructions for Captains of Merchant Vessels. London. 1866.
3. *The Antidotal Treatment of the Epidemic Cholera.* By John Parkin, M.D., F.R.C.S., late Medical Inspector for Cholera in the West Indies. 3rd Edition. London.
4. *The Epidemic Diarrhœa and Cholera; their Nature and Treatment.* By George Johnson, M.D. Lond., F.R.C.P., &c. London.
5. *On the Treatment of Asiatic Cholera.* By Archibald Billing, M.D., A.M., F.R.S., &c. New Edition. London. 1866.
6. *Remarks on the Pathology of Cholera.* By G. H. Barlow, M.D., &c. 'Med. Times and Gaz.' 1866.
7. *A Visit to Amiens to see the Cholera.* By R. D. 'Med. Times and Gaz.' 1866.
8. *A simple Explanation of Cholera, and a rational Mode of Treating it.* By Yod, M.D. London. 1866.

9. *Malaria, the Common Cause of Cholera, Intermittent Fever, and its Allies.* By A. T. McGowan, L.R.C.P. Lond., &c.

10. *Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital.* Vol. III. 1866. London.

11. Various Papers in the 'Medical Times and Gazette,' 'Lancet,' 'Medical Press and Circular,' 'British Medical Journal,' 'Glasgow Medical Journal,' 'Half-yearly Abstract of the Medical Sciences,' 'L'Union Médicale,' 'Medical Record,' &c. &c.

12. *Debate in the Harveian Medical Society of London on Cholera, &c.* 1866.

13. *Microscopical Researches on the Cholera.* By L. S. Beale, M.B., F.R.S. 'Med. Times and Gaz.' 1866.

14. *Reports of the Registrar-General, and Letter of Professor Frankland.* 1866.

15. *Official Memorandum of the Medical Officer of the Privy Council, July,* 1866.

16. *On the International Sanitary Conference, and the Preservation of Europe from Cholera.* By E. Goodeve, M.B., Surgeon-Major Bengal Army, &c. &c. A Paper read before the Epidemiological Society. 1866.

17. *Epidemic Cholera.* By M. Bordier. 'Archives Générales de Médecine.' 1867. Ditto, by M. J. Besnier, ibid. 1866.

18. Papers by Mr. Glaisher and Professor Rolleston, in the 'Times,' 'Spectator,' &c. 1866.

19. *Sanitary Measures and their Results.* By Thos. Shapter, M.D., F.R.C.P., &c. 1866. 3rd Edition. Exeter.

20. *Treatise on Asiatic Cholera.* By John C. Peters, M.D. New York. 1866.

21. *Cholera and its Treatment.* By Warren Stone, M.D. 'New Orleans Medical and Surgical Journal.' 1866.

22. *Statistical, Sanitary, and Medical Reports, Army Medical Department,* 1866. 'Blue Book.'

23. *Epidemic Cholera and Epidemic Diarrhœa: can these Diseases be prevented?* By W. Camps, M.D. London. 1866.

24. *A Scientific Inquiry into some of the Causes alleged to produce Asiatic Cholera.* By the Rev. SAMUEL HAUGHTON, M.D., F.R.S., &c. 'Med. Times and Gaz.' 1867.

25. *Do Smallpox and Cowpox afford any Protection from Asiatic Cholera?* By A. BLACKLOCK, Surgeon-Major, &c. London. Lewis.

26. *Essay on the Nature and Treatment of Cholera, Fever, and Cattle Plague, and on Public Health.* By Dr. TUCKER. Dublin. 1865.

27. *The Cholera Map of Ireland: with Observations.* By Sir DOMINIC CORRIGAN, Bart., &c. Dublin. 1866.

28. *On Malignant Cholera, &c.* By EDWARD CRISP, M.D. London. 1866.

29. *Cholera: its Seat, Nature, &c.* By C. SHRIMPTON, M.D. London. 1866.

30. *The Arrest and Prevention of Cholera, &c.* By A. E. SANSOM, M.B. Lond. London. 1866.

31. *Diarrhœa and Cholera, &c.* By JOHN CHAPMAN, M.D., &c. 2nd Edition. London. 1866.

32. *Cholera: a New Theory.* By C. DUDLEY KINGSFORD, M.D. London. 1866.

33. *Mechanical Treatment of Cholera.* By a Physician. London. 1866.

34. *Notes on Cholera.* By C. MOREHEAD, M.D., F.R.C.P., &c.

35. *Thoughts on the Present Theories of the Algide Stage of Cholera.* By JOHN COCKLE, M.D., &c. London. 1866. Also *The Influence of the Discharges and Nervous Shock on the Collapse of Cholera.* By the same Author. 'Med. Press and Circular.' 1867.

36. *Cholera, its Cause and Infallible Cure, &c.* By J. M. HONIGBERGER. Calcutta. 1861.

IN the course of last year[1] we laid before our readers notices of recent works upon cholera, and we have now endeavoured to complete the subject, which has attained great importance from its recent prevalence amongst us, and from the fact that it has been regarded of late not only in a medical but also in an international if not political point of view.

[1] pp. 129 *et seq.*, and 417 *et seq.*

We quote the following from the report of the medical officer of the Privy Council, which has just been printed:

"The choleraic infection of Egypt in May last, with the return of Mohammedan pilgrims from Mecca, where the disease was epidemic, followed soon afterwards by the spread of the same infection, along each of the several lines of steamboat communication which diverge from Alexandria as a centre, to all the most considerable ports of the Levant and of Southern Europe; whence again in many instances inland spreadings of the disease took place;—this constituted a succession of events which augured badly for the public health in England. And presently, in the quarter where it was being looked for, a first wave of the infection had touched our shores, though happily not yet greatly to harm us. For the first time in our experience of cholera the attack was on the south coast, not, as on former occasions, on our ports which look towards the Baltic: but on Southampton, distinguished among all our ports as the one of quickest Mediterranean traffic, and perhaps also (though this may have been secondarily) on Weymouth, or Portland, or Dorchester.

"Of the epidemic progress which I have just summarily sketched, I do not attempt here to give the innumerable and somewhat intricate details, nor to compare the present course of the disease with steps of former visitations. On that subject, under their Lordships' orders, I requested Mr. Radcliffe, Honorary Secretary of the Epidemiological Society, to compile a special report; and for the purpose, I put into his hands all the abundant information which the Foreign Office had communicated to their Lordships. The elaborate report with which he has recently furnished me, and which I append *in extenso*, gives all the information which has hitherto been obtained as to the epidemic progress, compares the present with former invasions, and gives some interesting supplementary information as to the Mohammedan pilgrimages, in their relation to the present subject. See Appendix, No. 13.

"Of the very small share which England has yet had in the epidemic, the main facts are these. Into Southampton there came, on July 10th, and at intervals afterwards, very suspicious arrivals from Alexandria, Malta, and Gibraltar. In the middle of August, a young woman in the town had a choleraic attack of doubtful nature; on the 22nd of September a labourer had undoubted Asiatic cholera, of which afterwards he died; and from then, for about six weeks, cholera-cases continued to occur in small numbers in and about Southampton, so that on the 4th of November (when the little epidemic might be considered at an end) there had been in all 60 such cases, of which 35 had terminated in death. It is a question whether from Southampton, or in any more direct way, the morbific influence may in August or September have reached Weymouth, or Portland, or Dorchester. I have no proof that any such infection took place, but accidentally I am informed that a gentleman from a distance, who early in August was spending a week in Weymouth, and visiting both Portland and Dorchester, contracted during that

week a diarrhœa, which on his return home developed to severe cholera; and in September there occurred in the neighbourhood of London, the following events, which give peculiar interest to the question. Mr. G— and his wife, inhabitants of Theydon-Bois, near Epping, had been lodging at Weymouth for seventeen days from the 8th September, had visited Portland on the 22nd and Dorchester on the 23rd, and returned home on the 25th. On the evening of the 23rd, Mr. G— had been seized with diarrhœa, sickness, and cramps, which continued more or less through the next day, and left him still unwell on the morning of the 25th. He, however, performed his journey to Epping with his wife. She, during the journey, began also to complain of abdominal discomfort; and this, after her return, developed with gradually increasing diarrhœa to cholera, of which (in its secondary fever) she eventually died on the 11th of October. On the 30th of September (while the last-named patient was still in collapse), one of her daughters, aged 8, was seized with cholera, and in a few hours died. That same night a serving-lad in the house was seized with cholera, and barely escaped with his life. On the 2nd of October, the doctor who was attending them died of cholera after ten hours' illness. On the 3rd, another daughter of the house, aged 16, passed into cholera, but eventually, after some consecutive fever, recovered. On the 5th, a maid-servant got diarrhœa, which, though relieved for the time, relapsed, and became choleraic on the 8th; and she, after some promise of recovery, fell into secondary fever, with which she eventually died. On the 5th, also, a labourer who worked on the premises, but lived apart, was taken with diarrhœa, which passing on to cholera and collapse, killed him next day but one. On the 6th, the head of the house, Mr. G—, who had suffered at Weymouth, and had ever since had relaxed bowels, got a very acute new attack, and died after fifteen hours. On the same day his son was attacked with diarrhœa, and next day was in collapse, but rallied, and finally got well. Also on the 6th the grandmother of the house was similarly attacked; and she, though she emerged from collapse, eventually died on the 14th. On the 18th, a woman living near by, whose only known connection with the above cases was that on the 8th she had assisted in laying out the dead body of the above-mentioned labourer, was taken with choleraic purging, which soon led to collapse, and next day to death. Thus, within a fortnight, in that one little circle, eleven persons had been attacked with cholera—mother, father, grandmother, two daughters, son, doctor, serving-lad, servant-maid, labourer, and countrywoman; and of these eleven only three survived—the son, a daughter, and the serving-lad. Later, in the countrywoman's family, another fatal case. It cannot well be doubted but that the exciting cause of this succession of events was, in some way or other, the return of the parents from Weymouth—of the father, with remains of choleraic diarrhœa still on him — of the mother, with apparently the beginnings of the same complaint. But this is only part of the case, and the remainder teaches an impressive lesson. All drinking-water of the house came from a well beneath the floor of the

scullery; and into that well there was habitual soaking from the water-closet.

"Whether, in intimate pathology, there are any essential differences between the cholera which kills on a larger scale, and the cholera which kills single victims, is hitherto so entirely unknown, that it would be idle to discuss, as a separate question, whether the G— illness, contracted at Weymouth and carried to Epping, was "epidemic" or "sporadic," "Asiatic" or "English" cholera; and, as above stated, I cannot prove it to have been an offshoot of the Southampton epidemic, or otherwise of Mediterranean origin. Certain, therefore, only is this:—that, from the time when Mr. and Mrs. G— returned ailing to their home, the discharges which passed from their bowels gave an additional and peculiar taint to the already foul water-supply of their household, and that thenceforth every one who drank water in the house drank water which had in it the ferment of decomposing diarrhœal matters.

"In relation to these, on the whole, inconsiderable manifestations of epidemic cholera in England, proceedings under directions of the Lords of the Council were taken as follows:

"With the assistance of Dr. Parkes, Professor of Military Hygiene at Netley Hospital, I watched the progress of the epidemic at Southampton, and addressed to the local authorities such suggestions as were necessary. And as soon as information came of the lamentable occurrence at Theydon-Bois, (which unfortunately was not until all the above-described mischief had been accomplished), I instructed Mr. Radcliffe to investigate the facts, and to give such advice as might be useful. Professor Parkes made peculiarly exact inquiry into all the circumstances connected with the beginnings of the epidemic in Southampton, and into the relations of the cases to one another; and I append his report *in extenso* (No. 14), not only for the positive information which it contains, but also as an useful illustration of the extreme difficulty which in all such matters there is in proving or disproving contagional relations. From Mr. Radcliffe's report I append (No. 15) the section which describes in detail the circumstances of the infected water-supply. And here terminates, for the year 1865, the history of cholera in England. What may be the facts of 1866, or what eventually will have been the share of England in the present pandemic diffusion of the disease, are questions on which no materials for exact judgment exist, and where at any rate hope may be preferred to prediction."

The foregoing interesting and valuable extract is but a small portion of the highly important matter, which has been most ably set forth in the report. The change in the route of progress of the disease during the past year, to which we alluded in the preceding number of the 'British and Foreign Medico-Chirurgical Review,' we find treated of at length in the report of Mr. Simon, as well as the increased rapidity with which modern steam lines enable it to travel. At p. 369 we find the following:

"I may be permitted to add, in concluding this report, that the disease in its course and symptoms has not differed in any essential particular in the present as compared with the past epidemics. It has not, as a rule, perhaps, spread so extensively among the populations which have been invaded by it; but its seizures, when the disease has become fully developed, have been as fatal, its course as rapid, as in previous outbreaks."

The disease had not spread throughout the United Kingdom when Mr. Simon's report was printed.

"Proportion of deaths to attacks during the present epidemic:

Brahilov	47·0	per cent.
Odessa	50·4	„
Malta	60·3	„
Gibraltar	54·0	„
Paris (hospitals)	51·6	„
Ancona (city and environs)	57·1	„
England, 1853-4—		
Range	41·0—51·0	„
Mean	45·2	„
Northern India, 1861—		
European troops	63·8	„

Mr. Simon points out that diarrhœa still characterises the inception of the disease in the great majority of cases, and shows the great importance of the recognition and early treatment of this most usual premonitory symptom.

"The great truth," he observes, "taught in this country by the outbreaks of 1847-8 and 1853-4 is again taught by the present epidemic, namely, *that the medical treatment of cholera is successful chiefly so far as it is directed to the prodromic symptoms, and particularly the prodromic (or as it is commonly termed, premonitory) diarrhœa.*

"The measures required, both public and private, to ward off an impending or mitigate an existing epidemic of cholera, are amongst the most certain and definite that medicine teaches. Their trustworthiness was demonstrated in the completest manner by the experience of the outbreaks of 1848-9 and 1853-55."

These means, hygienic and otherwise, are abundantly set forth in the official reports on the epidemics referred to, and in the text-books of medicine.

A very valuable and authentic history of the Mohammedan transport, if we may use the term, of the disease, by, it would appear, the Javanese pilgrims to Mecca by way of Yemen, Gonfundi, and Sait, is given in the appendix to the report, to which we would refer the readers for many most interesting and important facts, as well as for valuable details of the disease as it occurred at Southampton, Epping, &c., last autumn.

In Liverpool and many other places where the disease appeared in greater or less severity, admirable systems of medical relief were organised, while in one or two places considerable tardiness, and some degree of injudicious economy has been practised, to the loss of the community in life and health.

Before concluding the consideration of measures of prevention, we must again allude to the great benefit which would accrue to all by the avoidance of the known predisposing causes of the disease; such as irregularities of life, diet, &c.; deficient sanitary conditions of food, air, water, labour, clothing, and cleanliness. Again, the important part played by swampy land, ill-ventilated and confined localities, evaporation from muddy banks of streams, canals, &c., in fostering the disease in Europe as well as in hotter climates, should never be lost sight of.

Dr. Billing regards cholera as a species of fever: he advocates the use of tartar emetic and sulphate of magnesia in suitable doses; avoids external heat, as hot bran fomentations, &c.; a five-grain dose of calomel, as well as quinine, and nutritious light diet, dry friction, &c. This author finds fault, and, as it appears, with reason, with some of the directions of the Board of Health of 1848; he states, that the interdiction of even ripe and cooked fruits and vegetables by the Board is erroneous, as these useful articles of diet, "good vegetables and ripe fruit, by preserving a healthy state of the blood and secretions, are calculated to give strength to resist an epidemic influence."

Dr. John Parkin advocates the internal exhibition of carbon, either in its simple form or as carbonic acid; and he regards this as a specific, styling his treatment as "the antidotal treatment of the epidemic cholera." An anonymous writer, on the contrary, advocates the internal exhibition of water as the sole cure, and regards carbon in the system as the cause of the disease.

Dr. Calvey,[1] of Toulon, regards the cholera as contagious, and advocates maritime quarantine, inasmuch as it is better to shut one door against its approach, that by land it being out of our power to close.

Investigations have but added strength to the opinion of those who regarded the water[2] supplied to East London as a most material agent in the production of cholera there.[3]

MacGowan and Fergus condemn purgatives. The former

[1] L'Union Médicale (28 July), 1866.

[2] Since this paragraph was put in type, a careful consideration of the facts observed in the epidemic in East London leads to conclusions of a somewhat contradictory nature with regard to the influence of drinking water on the causation of cholera, as shown in the 'Reports of the London Hospital, 1866.'

[3] 'Medical Times and Gazette,' Sept. 8, 1866.

advocates stimulating emetics, quinine endermically, salt internally, &c.

The reply of the Cholera Committee of the London College of Physicians, to the Lords of the Privy Council, for the guidance of captains of merchant vessels (in the absence of medical advice), when cholera appears on board ship, recommends the use of the "mildest opening medicine, as castor-oil or rhubarb. Glauber's salts and Epsom salts are dangerous." "The common belief that prolonged costiveness should not be interfered with during the prevalence of cholera is erroneous." The necessity of finding out the first stages (diarrhœa) of the disease should be the care of the captain. The moment a case occurs, the man should go to bed, be kept warm, and have, in a little peppermint-water or weak brandy-and-water, ten grains of aromatic powder of chalk and opium, or five drops of laudanum should the former medicine not be at hand.

Large doses of opium or of ardent spirits should be avoided.

If the looseness arise from bad or obviously indigestible food, a dose of either of the gentle laxatives above named should be given.

The diet should be mainly beef-tea or broth, gruel, or rice.

If the purging resembles rice-water, and be attended with vomiting and coldness, opiates and spirituous liquors should be discontinued; water and fresh air should be freely supplied, warmth to the feet and legs, and sinapisms to the stomach, &c.

It is unnecessary here to quote at greater length the other judicious directions contained in the replies of the College of Physicians' Committee. We refer to the obtaining medical advice as soon as possible; to cleanliness, ventilation, disinfection, removal of all excreta, consideration of drinking-water, &c.

Dr. Seghers stated (May, 1866) in the Belgian Academy of Medicine, that as many had perished by cholera in Holland and Prussia, the Academy should, without delay, announce suitable preventive measures for adoption in case the disease should appear in Belgium.

He showed how long experience proved the important benefit derived from separation of the sick, and hygienic measures enforced before and from the first appearance of the disease.

He instanced that at Saint-Nicolas, where for more than thirty years, in an epidemic of typhoïd fever, and subsequently in two epidemics of cholera, great good had been achieved; for out of 25,000 inhabitants, of whom 10,000 to 12,000 were poor and of the working class, there had scarcely been thirty cholera cases, although there were many alleys and many unhealthy dwellings in the town.

The great importance of the system which this author advo-

cates, namely, of receiving persons coming from affected localities into refuges or lazarets—isolated dwellings—in which their sojourn may not exceed a few days, is strongly shown.

Many of our contemporaries,[1] we may observe, have already advocated the great necessity of measures such as those now advocated by Dr. Seghers. The latter says that it was owing to like measures adopted by the Belgian Minister of the Interior that the cattle plague was stopped in its commencement and on each reappearance there, whereas the too tardy adoption of like measures in England and in Holland caused deplorable ravages.

We must admit, with this writer, that much evil has arisen from the want of agreement, among professional men, as to the questions of contagion, &c. He moreover insists on the contagiousness of the disease, stating that the weight of evidence is in favour of that opinion. His views are corroborative of those advanced years ago by Graves, Alison, and others of our countrymen, namely, that there is first a centre of sickness established in any place, whence the disease, be it epidemic or epizoötic, spreads, and it is only by hygienic means we can hope to lessen the number of sick and of victims of the disease.

He advocates the use of tonics and aromatics, in a small quantity of gin, taken fasting, stating that this is also useful in the intermitting fevers so frequent about Antwerp, &c.; in short, our author again endorses the custom and practice of British physicians in the East and West Indies, in Africa, &c.

He controverts the opinion advanced by M. Meyne in the 'Topographie Médicale de la Belgique,' that there exists an antagonism between cholera and intermitting fevers; he contends that experience along the banks of the Scheldt and in other marshy localities demonstrates the coexistence of cholera and intermittent fevers; and he states his belief that there is "une sorte d'identité entre le choléra, la fièvre jaune et la fièvre intermittente pernicieuse, vu leur nature, leur origine et leur siège; elles sont produites par les mêmes causes, c'est-à-dire par des miasmes contagieux provenant de matières végétales et animales en putréfaction; dans l'intérieur des pays où le sol n'est pas marécageux, la maladie n'existe pas, et partout où l'on a transformé le terrain la maladie n'a plus reparu. C'est donc le climat et la constitution atmosphérique qui impriment à ces maladies leur cachet spécifique, car les prodromes présentent à peu les mêmes phénomènes morbides; donc, le choléra est une espèce de fièvre pernicieuse et contagieuse."

[1] Viz., the 'Medical Times and Gazette,' the 'Lancet,' the 'Medical Press and Circular,' &c.

Whitly Stokes considered that many diseases of tropical climates had numerous characteristics in common with those of our own latitudes. Macartney regarded cholera as a form of fever with a prolonged cold stage; Hartley Kennedy had very similar views; Baly, Smith, C. F. Moore, and Byrne have all pointed out qualities and proclivities possessed in common by fevers and cholera.

Munro described the cantonments of Peshawur as the fitting place for the engendering of intermitting fever; and in the parts of the men's quarters more especially malarious fever and cholera first began and continued.

But marshy land, and foreshore of crowded ports and river-sides, are not the only places, in which fever and cholera are easily engendered; decomposing animal and vegetable matter and night-soil, &c., are in themselves quite sufficient to favour and even produce fever and cholera. We would refer to evidence on this point in a report published some years ago, on an outbreak of cholera that occurred in 1853, in close proximity to a depôt of scavengers' sweepings, &c., in Dublin: and an outbreak of fever, fatal to two out of four cases in one family, in a close proximity to a similar depôt, in another part of Dublin, has come to our knowledge, as well as a great number of other cases of fever, and some, quite lately, of diarrhœa, cramps, &c. In further confirmation of Dr. Segher's observations on these points, we would refer to the evidence given by Pettenkofer, Budd, and others, as well as by the statement[1] lately made in the House of Commons, that typhus fever has constantly prevailed in close proximity to a depôt of night-soil in Liverpool contemporaneously with the existence of cholera at another part of the port.

A report on Amiens has appeared in the 'Medical Times and Gazette,' which certainly presents a fearful lesson of cause and effect, as seen in the backward sanitary state of the town, and the great fatality of the cholera there.

About 8-9ths of the mortality fell on the lower orders, living in the conditions which we will summarise from the report thus:—Amiens is rather crowded with many poor working people, and built in a flat marshy valley in which the Somme subdivides into very numerous branches or "rivières," with surrounding limestone hills. "Old, musty, tumble-down wooden houses overhanging the streams," swamps, no drainage properly so called; numerous cesspools fouling the soil for generations past, and all filth summarily cast out in the streams; dirty latrines, vegetable refuse rotting in the sun. Many of the lower

[1] 'British Medical Journal,' July 7, 1866.

classes very poor, ill-fed, and filthy in the extreme. Even the very economy of fuel leads to a serious ill, namely, deficient ventilation from the use of stoves instead of open fireplaces; hence a relaxing and stifling atmosphere in the dwellings of the poor from this cause, coupled with overcrowding.

Though the water supplied to the inhabitants is "admirable," many, it is believed, use the indifferent water of the Somme, containing sewage, at least for cooking and cleansing purposes. Moreover, these people live in the vapours of the river. "Hence it is clear that the cholera at Amiens has sought out its old haunts and its old victims—the low, dirty dwellings on the banks of streams, contaminated with sewage, and inhabited by ill-fed, ill-washed, ill-aired, poor-blooded people. Doubtless it was brought to Amiens in the autumn, and its poison has lingered ever since in the filthy sediment of the rivières."

"So far then (observes the able correspondents of the 'Medical Times and Gazette'), we can picture to ourselves the algide cholera; existing absolutely as an essential and primary malady, not depending on any tangible antecedent disorder in the bowels or elsewhere, and passing into a state resembling that of typhous (or paludal) fever, of which coldness, weakness, defective excretion, and brain disturbance are characteristic. But equally evident was it, that there was another condition, whose occurrence was morally certain to lead to algide cholera, and which passed into it by insensible steps without gap or interval, and that this condition was *Diarrhœa*."

Cases marked by premonitory painless diarrhœa, clean tongue, debility, no headache, are considered at Amiens as suitable for "opiate astringent treatment." On the other hand, cases preceded by

"*Embarrass gastrique*, which we would understand as derangement of stomach and liver; foul, loaded, œdematous, sodden tongue, marked by the teeth; want of appetite, nausea, eructations, and frontal headache. This state might pass into cholera, with no purging, but constipation. These are the cases in which English practitioners would probably begin with a dose of calomel."

This affords confirmation to those who hold the opinion that cholera, like fever, to which it certainly bears many points of resemblance, demands treatment suited to each case, and to the circumstances as to locality, &c., under which it arose.

La suette, a prodrome, or coincident or complication of cholera as last year seen at Amiens, is thus described:—"Debility, *embarras gastrique*, white furred tongue, and copious periodic cold perspirations." And again—

"*La suette* is the true living though degenerate descendant of the old *sweating sickness*, and, like the cholera, keeps to its old

haunts, and destroys the same victims. Three fourths of its symptoms are those of *embarras gastrique*, with obstinate constipation—that is, of a foul state of the alimentary canal, whose epithelial lining, under influence of swamp, filth, and misery, grows abnormally, and may be (on the hypothesis of a cholera poison) the nidus and starting-point of the true algide cholera. The remedies are ipecacuanha, followed by cinchona. Certainly one glimpse at *la suette* gives one broader and truer views of the nature and relations of cholera."

Avoiding a controversial handling of the question of treatment of the diarrhœa of a cholera season, Dr. Barlow points out the action of water as the vehicle for the removal of the effete substances of the blood from the system; ably showing the effects of changes, in several organs, that arrest more or less the depurative process, effected in the normal state of the system, by the due circulation of water and the blood through the system. First, he shows the degree of similarity between cholera and those diseases that arrest or diminish the access of water to the system generally; secondly, the degree of likeness between the disease under our present consideration, and those that hasten the abstraction from the system of the water, instancing specially under the latter heading, Bright's disease, and English cholera.

Dr. Barlow proceeds to show how the abstraction of fluid from the system is prejudicial, and how injection of the veins for a time supplies this want, but how, by returning vomiting or purging, and consequently again loss of fluid, the patient once more sinks.

"The facts may now be thus summed up. Obstruction to the admission of fluid into the circulation causes collapse, with tendency to asphyxia, suppression of urine, and, as a consequence of this suppression, some of the symptoms of uræmic poisoning. Obstruction further on in the course of the circulation, as in the liver, causes scanty urine, and in extreme cases there may be some of the symptoms of uræmic poisoning. Still further on, there will be lividity and scanty urine, with, in severe cases, the pulse of impending asphyxia, the circulation relieving itself by effusion of serum into the peritoneum, and in some rare instances partially by diarrhœa. But if the fluid pass freely through the system to the kidneys, and find a free exit from the system, it may nevertheless, if those are diseased, pass out, leaving behind it the excretory matter which the kidneys should eliminate, and the symptoms of uræmic poisoning are the result. If, however, there be a rapid drain of fluid from the system, as in the instances of diarrhœa which have been cited, we have not only uræmic poisoning, but collapse and other consequences of the defect of fluid in the circulation—the effects being the same, whether the defect arise from obstruction to the access of fluid, as in disease of the duodenum, or from excessive and severe diarrhœa."

It is out of our power to give at greater length the valuable remarks of Dr. Barlow: he, however, with much patience treats at length, what we feel we should not be just to our readers if we did not pronounce as the dangerous teaching of Dr. George Johnson. Dr. Barlow quotes several cases showing strongly the fatal effects of the use of castor oil in the disease, and the effects of the purging incidental to the disease. Having witnessed the disease in hot countries as well at home, we confess no small degree of impatience with the very name of castor oil in connection with it, and we should not have alluded again to it in our present notice, had we not been convinced anew, if we may use the expression, of the extreme responsibility resting with any who permit the promulgation of such doctrines to pass unchallenged: every day's experience of what we witness in practice, both among the hospital sick and among those most prone (at once by their improvident and imprudent modes of life) to the disease—we mean the artisan and lower classes—convinces us of the extreme wrong inflicted upon all by the persistent advocacy of such treatment; inasmuch as though it can have but little effect on those, who have had experience in the treatment of the disease, still we must remember that a large number of our younger brethren are not so forearmed, and therefore forewarned by experience; and we must also remember, that but too many minds are given to following, without reflection, what is ingeniously set forth. We could add to the list of cases quoted by Dr. Barlow and published by other physicians, showing the evil effects of castor-oil treatment: we must, however, content ourselves by commending to our readers the highly useful communications of Dr. Barlow, which that distinguished physician read before the Hunterian Society.

In a case of death from cholera, Dr. Stokes[1] mentions that a loud bellows-murmur was discovered at the upper and middle sternal region, in a young man who had been previously in good health, within a very few hours after collapse had been established. This continued up to the time of death; and on dissection a large coagulum was found in the left ventricle, stretching upwards, and extending through the aortic orifice into the arch of the aorta. The valves of the heart and its walls were found perfectly healthy, so that there can be no reasonable doubt, that the bellows-murmur was of recent production, and was owing to the interference of the remarkable coagulum with the proper action of the aortic valves.

The 'Medical Times and Gazette' thus speaks of M. Worms' opinions:—"With Liebig, M. Worms considers that, losing thus

[1] On 'Diseases of the Heart and Aorta,' p. 124.

its chlorides, the blood loses with them its faculty of producing urea, and becoming denser and denser, becomes less and less readily circulated.

"Organic life at this stage of the disease has, so to speak, ceased; organic decomposition goes on without restitution, the blood has almost entirely assumed the venous character," and the production of caloric becomes too little to make up for that thrown off by radiation.

M. Worms regards the choleraic poison as an alkaline ferment, the action of which is facilitated by all those circumstances which retard a complete oxidation of the blood, or contribute to surcharge it with hydro-carbonaceous products, and impart to it a venous character. The two great systems which are the source of the development of choleraic poisoning are the sanguiferous and the digestive, and M. Worms considers that of these the blood is the first to suffer, and that the disturbances of the digestive system are secondary to the alterations which this fluid undergoes. One reason which he assigns is, that the fœtus *in uteró* may be attacked with cholera, and that then we constantly find the intestines filled, as in others, with the specific exudation.

He thinks that the same conclusion is indicated by the ordinary gradual development of a choleraic attack, which is usually preceded by prodromata, during which precursory period a slow and progressive infiltration of the intestinal tissues and follicles takes place, preparing the way for the discharge of the watery constituents of the blood. The more feeble the cause of the poisoning, and the slower its operation, the longer will be the duration of the precursory phenomena; and the stronger the poisoning, the shorter the preparatory process will be.

He terms the period of *reaction* the stage of *restitution*. He asserts what was suggested in 1854,[1] that many of the effects of the disease were owing to urea in the blood, and especially this is found to be the case, M. Worms asserts, in the cerebro-spinal fluid, the spleen, brain, the muscles of the lower extremity, and in the heart. The cerebral tissue contains the most. A reversal of the flow of water as respects the intestine, so to speak, takes place, and fluid is taken up into the blood, and that which enters the portal venous system is immediately applied to the secretion of bile. This author goes on to point out the gravity of the occurrence of somnolency after reaction has begun, as well as of the efforts of the muscles of the chest becoming visible, the return of collapse, &c. He also alludes to the cutaneous eruption, and even desquamation, which other

[1] See "The Cholera at Finglas," by C. F. Moore, M.D.; 'Dublin Quarterly Journal of Medical Science,' p. 341, November, 1854.

authors have also noticed, as mentioned in our notice in the July number of this Review.

He draws attention to the distinction between the "typhoïd state," "choleraic typhoïd," and "the period of uræmic coma."

He thinks that they should be carefully distinguished as to their nature, and no less as to their therapeutical indications, and this is the principal object of the paper.

Somnolency is mostly not marked by fever-thirst to any great extent, nor yet by any notable alteration of the tongue; he thinks it scarcely analogous to the typhoïd state. Nor is it like the coma of pernicious fevers nor of fatal dropsies, as in these two latter cases the coma sets in all at once, while here the patient loses his mental powers very gradually. The absence of every phenomenon of nervous or circulatory disturbance and the slow march of the affection absolutely exclude all idea of uræmia.

A bloody redness of the whole of the conjunctiva, and hardness and protuberance of the globe of the eye, accompany the somnolent state, and foretells severe reaction. Soon afterwards a vague look, as if the person were drunk, and a lowered upper eyelid (and on examination the ophthalmoscope shows enormous venous development at the bottom of the eye) succeed.

M. Worms regards these conditions as owing to excess of the reparatory process, the veins being yet gorged, while arterial blood is driven into the brain in large quantity.

After death, in such a case as this, turgescence of the brain, with serous effusion between the convolutions and between the brain and the skull was found; also the humours of the eye, especially the vitreous humour, were more abundant and less constant than usual. M. Worms found a tepid epithem to the frontal region, having first removed as much of the hair as necessary, highly useful. His formula was as follows:—Camphorated spirit 150 grammes, solution of ammonia 20 to 25 grammes, infusion of arnica 100 grammes, and hydrochlorate of ammonia 45 grammes. Forty-eight to seventy-two hours of this application usually sufficed, but sometimes it was required for eight or ten days. Out of sixty-five severe cases of reaction he was successful with this remedy fifty-one times. When this phase of the reaction has passed away the typhoïd phase appears, or the uræmic stage, if by that term a febrile process having for its cause and end the elimination from and by the blood of the organic detritus, of which urea is the most readily appreciable representative.

M. Worms adds that possibly venesection or other means might be successful in combating the somnolency; venesection, however, he has hesitated to adopt. Blistering the lower

extremities, warm moderately aromatic drinks, sulphate of quinine, and sometimes also nitrate of potash, he has combined with the fomentation of the head. We previously mentioned M. Worms' advocacy of sulphuric-acid lemonade in premonitory diarrhœa and in the early stages of the confirmed attack. He prefers isolation of cholera cases, uses instant disinfection, by strong solution of sulphate of iron, of all dejections, and treats all the linen with boiling water. By these means all attendants on the sick were, he thinks, saved from attacks of the disease.

The London Hospital afforded valuable aid to those in cholera in 1866. Before the close of August, 509 cases of confirmed cholera were admitted, of whom 54·9 per cent. died. The arrangements, for accommodation of cases, were excellent and ample for those seeking admission; 138 beds were available, while but 114 cases were in hospital at any one time.

All the closets attached to the cholera wards were locked up, and all the excreta were received into vessels containing carbolic acid, removed from the hospital, again mixed with a solution of carbolic acid ($\frac{1}{2}$ oz. of strong acid to 1 quart of water), and buried five feet under the surface of the ground. Carbolic acid, in the form of powder, was sprinkled about the wards, and complained of by the nurses as causing headache and sore-throat to some extent.

All soiled linen from the cholera wards was placed in tubs with solution of chloride of lime, and, after soaking, was washed with carbolic acid soap. No damage was caused to the linen in this way.

In persons under 10 years of age,				66	recovered and	56	died.
,,	,,	20	,,	44	,,	40	,,
,,	,,	30	,,	50	,,	56	,,
,,	,,	40	,,	41	,,	52	,,
,,	,,	50	,,	17	,,	39	,,
,,	,,	60	,,	8	,,	20	,,
,,	upwards	.	.	2	,,	18	,,
				228		281	

Of 20, of all ages, treated prior to 19th July, 17 died; of 30 treated in the last week of August, there died but 12. Of the 509 cases, 47 per cent. were females. The mortality decreased week by week from 85 per cent. in the first to 37 per cent. in the last week.

Of the whole number admitted, 22 per cent. were under ten years of age. In the early part of the epidemic, persons were for the most part only brought in when in extreme collapse, and

either died shortly after, or at the commencement of reaction. The consecutive fever, which resembled typhus in many respects, proved fatal in a large number of cases; and, even in those who recovered from it, the convalescence was very protracted.

The severity of the cramps was greater in this epidemic, to a marked extent, than in previous ones. The comparative non-use of opium, at the present time, probably had something to do with the greater prevalence of cramps in this epidemic as contrasted with that of 1849. Cases occurred in which each of the usual symptoms of cholera was absent: thus in some cases collapse was the chief feature, purging or vomiting, or both, being absent; in others the sickness was most severe and incessant, often proving fatal, especially during the consecutive fever.

Great care was taken in the diagnosis to discriminate between cases of diarrhœa and cholera.

No fatal case of diarrhœa, rightly so called, occurred in the London Hospital.

Diarrhœa did not at all invariably precede cholera.

Nearly 10,000 out-cases of diarrhœa were treated up to the end of August at the London Hospital. If it had lasted any length of time with watery discharges, astringents were used, and, judging from the comparatively small number who were subsequently admitted as in-patients, with good results. Where there was much abdominal pain, with slight diarrhœa or offensive evacuations, castor-oil with a few minims of the tincture of opium was given, to be followed by astringents if necessary.

Out of a very large staff, but five nurses took the cholera; of these, four died. Of eleven laundresses, one was attacked and died. Of the former, three of the fatal cases were non-resident, as was also the laundress.

But one case of all those in the general hospital, which contained the full number of patients, took the cholera; and this was a child that had been moved to an attic where the first case of cholera had been received: it died.

Dr. Fraser thinks saline injections with a very small quantity of alcohol, as used by Mr. Little, a valuable resource in extreme collapse, where the alimentary canal has lost all power of assimilation.

Mr. Little (London Hospital) points out the necessity of using the injection of the veins before collapse has continued long, as in this case clots have usually formed in the heart, and even extended far into the veins.

His observations merit notice:

"Only patients with no apparent chance of recovery have been injected—cases of extreme collapse—all of them pulseless at the wrist, livid, with low external temperature, and having lost quanti-

ties of fluid, either by purging or vomiting—generally by both. The fluids used were defibrinated blood, serum, salines, and salines with alcohol. The blood used was sheep's, kept warm by placing the vessel containing it in hot water, defibrinated by whipping, and strained. . . . . I used defibrinated blood at the request of Dr. H. Jackson, but only in two extremely bad cases. It had no favorable influence, even temporarily, and seemed to embarrass and hurry the respiration. The cases were such that no conclusion, either way, can be drawn as to the use of blood."

The serum used was also from sheep. In one case, in which it was used pure, it appeared of use; but the patient's age, 64, "was probably fatal to him." Mr. Little used a saline fluid for injection, consisting of chloride of sodium 60 grains, chloride of potassium 6 grains, phosphate of soda 3 grains, carbonate of soda 20 grains, distilled water 20 ounces. These proportions, with 2 drachms of pure alcohol to the pint of water, were used in most cases, and in all the successful ones, of late as well as in 1849. It was injected at a temperature of 110°, or very nearly so, as it cooled somewhat whilst passing through the apparatus. At first a syringe was used, but latterly a funnel and an india-rubber tube with a nozzle at one end was used, and the fluid was allowed to flow in by gravitation.

Mr. Little gives the results in fifteen cases, four of which were successful; he also mentions other satisfactory cases since his report was in print. Dr. Woodman, who had charge of the Limehouse District Hospital, Wapping, reports favorably of the saline injection; he used it three times with marked benefit for a time in each case, and in one recovery. All these cases were pulseless, and one also insensible.

Mr. Little selected a vein at the elbow:

"There was generally little difficulty in finding a vein, but sometimes none was visible through the skin. I found it best to expose the vein and pass a probe under it before opening it. The fluid from the apparatus being now turned on, washes away any blood issuing from the opening, which the operator can then clearly see, and runs little risk of pushing the nozzle into the cellular tissue, or up the sheath of the vein."

Dr. Fraser, of the London Hospital, regards a true case of cholera collapse to consist in having the nose, tongue, and breath cold, the urine suppressed, and the voice reduced to a whisper. "Under these conditions the patient rarely recovers . . . . . When they are absent, the following may be more or less present, and yet recovery may take place:—Inaudible heart's action, absence of pulse, shrivelled skin, the washerwoman's hand, skin bedewed with a cold and deathlike moisture, vomiting, purging, cramps, &c."

"I consider that this epidemic began suddenly, raged fiercely, culminated sharply, and declined rapidly, as cholera has hitherto generally done," &c. "No new symptoms were observed, but a greater variety in different cases were noticed; for instance, some had purging and no vomiting, some vomiting and no purging; to each cramps were sometimes added, and in some cramps were the chief symptom."

He dwells on the fact that on this occasion it was, as before, the ill-clad and ill-fed poor, those especially residing in unwholesome dwellings that were the chief and early victims, &c.

Astringents and stimulants were not in much favour. Castor-oil, Tartar emetic, and Sulph. Magnes., in small doses, were still less so. Saline lemonade was much more liked. The Hyd. c. Cretâ, with small doses of ipecacuanha and Dover's powder, were found more useful.

"Baths at a temperature of from 98° to 104° Fahr. were given with most marked effect in about 130 of the worst cases."

In some cases of extreme collapse no relief was afforded by the warm baths.[1] For incessant vomiting all sorts of remedies were tried: nutrient enemata did not give much relief; creasote pill was of use in three cases, bismuth and hydrocyanic acid in one, and ice was beneficial in many. Bread-and-butter was retained by a child of twelve that begged for it after vomiting all fluids as soon as swallowed.

One grain of sulphate of quinine and 15 minims of Tinct. Ferri Mur. in 1 oz. Aquæ was thought of use. The grey powder ipecacuanha and Dover's powder as given above constituted, with chlorate of potash drink and saline lemonade, and attention to the state of the bowels and to diet, the usual treatment in the reaction stage.

If drowsiness, with flushed face, congested conjunctivæ, &c., came on, sinapisms or blisters to the nape of the neck were often very efficacious. In cases of severe headache from the first, cold irrigation was found of some use.

Collapse frequently merged insensibly into reaction.

Of fifty-two deaths in males, twenty-five perished collapsed, and twenty-seven in the reaction stage; whereas the proportions in forty-five fatal cases in females were thirty-three in collapse and twelve in reaction.

"Strong reaction was characterised by sharply defined patches of dusky redness in the cheeks; highly congested conjunctivæ; a heavy, sleepy aspect, with the vacant look of fever; eyes half closed, with pupils turned upwards and inwards; dry hard tongue, denuded of epithelium; sordes about the lips and teeth; very laboured respi-

[1] We may observe that this was also the experience during March in Paris.

ration; and, especially in lads and children, a preference for the prone position. Hebetude of mind, from which the patient could not be roused, and in some cases violent delirium, accompanied this state, which usually proved fatal."

The local variations of temperature in this state were very remarkable.

The thermometer proved that the temperature cannot be estimated with any degree of certainty by touch.

The extremes of temperature were 91·2 and 105·6. During reaction the temperature was below normal throughout, sometimes even lower than it had been at an earlier period of the attack. During favorable reaction there was frequently remarkable slowness of pulse, 54 being a very general rate. In one lad of nine years, apparently doing well, the pulse was 45. It often intermitted in bad reaction; the seventh beat was invariably wanting in one fatal case. Nothing was found post-mortem to account for this.

The extremes of respiration were 12 and 44. A roseolar rash in the reaction stage occurred in some favorable cases, being observed in some instances to be preceded by an increase of temperature, which again fell as the rash disappeared. Œdema of the feet, and spasms of the hands and feet, occurred in one boy who had roseolar rash with violent reaction; he died.

A girl, who was also one of the roseola cases, had no œdema, but spasms in hands and feet, and recovered. As we noted in our abstract of the report on cholera at the Hôpital St. Antoine, in a previous number of the 'Review,' enlargement and suppuration of the parotid and submaxillary glands occurred in some cases consecutive of cholera, so in the London Hospital similar affections were observed under like conditions. Dr. Ogle at St. George's and other observers likewise have noted the occurrence of urticaria and other eruptions among the sequelæ of cholera.

At the London Hospital several cholera cases aborted, and all such proved fatal; one at the full time was delivered naturally and recovered; three other cases at about full time died undelivered; one child born at full time was dead, as were three others who were removed by the Cæsarian section immediately on their mothers' deaths.

The secretion of milk continued in four cases that were suckling, and proved very troublesome. The subsequent history of the infants was not known; mothers and children being, of course, separated on the former taking cholera.

Noises in the head often occurred, and in one case formed the first symptom.

Affections of the cornea by prolonged exposure from the partially open lids were common; in one case total blindness from rapid sloughing of both corneæ took place in a female child.

Coryza frequently occurred in reaction, especially in children.

Menstruation continued unchecked in cases where it had commenced before the supervention of cholera. In all females there was a muco-purulent discharge from the vulva; in adults it was stained with blood, though not at the menstrual period; in one case the quantity of blood was considerable.

In reaction and recovery the specific gravity of the urine varied from 1005 to 1017, the average of all the observations being 1006.

Even on the return of this secretion after suppression had lasted several days, the specific gravity of the urine was remarkably low.

It is well to remember that the secretion of urine often returned before the power of voiding it. Almost always the urine was acid, the acidity being greatest in that first passed in reaction.

In almost half the cases albumen was present in varying quantities. This, however, soon disappeared, except where there was reason to suspect old renal disease.

Nitric acid gave with the urine, in many cases, a brilliant ruby colour.

In about one third of the cases uric acid crystals and urates were found in considerable quantities. No oxalates were observed.

There was found in most cases epithelium from the kidney, ureter, and bladder; and in the female from the vagina, as if a general desquamation set in with reaction.

Disintegrated cells, granular, hyaline, and rarely true epithelial casts were found. A remarkable proneness to decomposition, and a tendency to become full of the minutest forms of animal and vegetable life, were observed in all the specimens of urine.

The discharges from the bowels are fitly described as rice-water, and separable on standing into a milky-water-like supernatent fluid and flaky sediment; the former presented a tinge of grey, was semi-transparent, and in some cases contained albumen. The sediment was of flakes of coagulated mucus, having embedded in it numerous molecules and granules—many of them in active motion—and cells of various sizes, and in various stages of development or decay. Some resembled the colourless corpuscles of the blood; others were twice as large, and exhibited nuclei without the help of reagents; and some, filled with refractive granules, were undistinguishable from the well-

known granule-cell. Well-defined cylindrical masses of granular matter, probably casts of follicles, but without anything like limiting membrane, were also seen.

The normal epithelium of the bowel was almost uniformly absent in the evacutions during life.

The discharge of blood from the bowels always portended a fatal result.

The vomit contained buccal and pharyngeal epithelium, granular matter, and various vegetable cells from food.

Mr. Mackenzie, assistant resident medical officer at the London Hospital, has observed that the occurrence of cramps, if severe, will raise the temperature in the axilla as much as 2° Fahr. The more hurried the respiration, the higher the internal temperature, as found in the rectum or vagina, will be.

In a little boy who died in imperfect reaction, the temperature attained the unusual height of 101·8 externally, and 106·2 internally.

Mr. Mackenzie regards the elevation of temperature post-mortem as corresponding with the length of the illness and the stage at which it proves fatal. He mentions cases of spasms occurring in the limbs after death, and observes that they occur in the parts affected with cramps during life. (It occurs to us that these post-mortem spasms might involve medico-legal questions in some extreme cases.) Aphthous sores and vesicles of the mouth, lips, and tongue, and sore-throat are common, especially in cases attended with Roseola cholerica. Severe pain in the epigastrium is also common in the stage of reaction.

Capillary bronchitis, attended with thick creamy pus in the minute tubes, was not very uncommon, and formed a very serious complication of the reaction stage.

Ulceration of the cornea occurred in some cases.

Atropine and Calabar bean act upon the pupil in cholera.

The state of the pupil varies much in cholera; but in the collapse it is not widely dilated, as in the cold stage of ague.

Dr. Woodman, to whose interesting report on the Limehouse Cholera Hospital we have before alluded, says—

"The results of our post-mortem examinations are, to a great extent, contradictory. In opposition to the generally received opinion, the lungs of those dying in collapse were often congested; while in one case of death, during reaction, they were shrunken and bloodless. The heart contained blood in both sides (chiefly the right), but in one instance the ventricles were contracted and empty. The gall-bladder was invariably full. The mucous follicles in the intestines were generally remarkably prominent. Chronic renal disease had considerable influence on the mortality, especially in those who died in reaction."

Dr. Letheby's report on the epidemic in London last year (1866) shows the difficulties to be met with in attempting to account for the localisation of the disease. The great mortality referable to Saturday's dissipation proving fatal on Tuesday is shown by a table constructed from the Registrar-General's Reports for 1866. The same circumstance has been observed in Dublin and other localities during the present epidemic, as well as in previous visitations.

Dr. Lionel S. Beale thinks it probable that the victims of cholera were suffering chronic structural change in the *tissues* of the intestines for months, and in some cases for years, before death.

Cholera is constantly connected with removal of the columnar epithelium from the villi. "Although there may be no actual diarrhœa, this epithelium is found in quantity in the intestine after death." In some cases where death rapidly occurs in cholera "by sudden stagnation of the blood in the capillaries of important organs, there might not be time even for the removal of the epithelium from the villi, just as we may have death from smallpox or scarlatina without any eruption."

"The process affects the mucous membrane of the gall-bladder and larger gall-ducts; that of the bladder, ureters, and pelvis of the kidneys, as well as that of the Fallopian tubes, uterus, and vagina. In short, there seems a tendency to the removal of epithelium from the surface of all the soft, moist, mucous membranes; not, it must be remembered, of the epithelium which is specially concerned in *elimination*, but rather of that which lines the ducts of glands and cavities which may be included in the category of the ductal portion of the different secreting glands."

Dr. Beale goes on to state,—

"The follicles of the mucous membrane of the stomach and intestine, of the salivary glands and pancreas—the tubes of the liver, kidney, and other glands, still retain their epithelium; nor have I been able to demonstrate in these varieties of glandular epithelium any appearances peculiar to cholera. Indeed, so far as I have yet been able to observe, it would be extremely difficult to distinguish many secreting cells taken from the bodies of cholera victims from perfectly healthy cells."

On the other hand, Dr. Parkes and Dr. Gairdner assert that the separation of epithelial cells results from post-mortem mechanical maceration.

Dr. Austin Flint, of Bellevue Hospital, N. Y., has published a very able refutation of the so-called elimination theory of cholera. He concludes with the following:—

"The importance of the bearings of the theory of elimination on

the treatment of cholera cannot be too much considered. If this theory be accepted, it is never desirable to interfere with the vomiting and purging, but, on the contrary, both are to be encouraged. To think of this practical result of the theory must cause a shudder to one who holds to the doctrine, that the great source of danger in cholera is the loss of the blood-constituents contained in the choleraic effusion. It is to be feared that the influence of the able papers by Professor Johnson on the minds of practitioners will lead to the loss of not a few lives."

"Another practical result legitimately follows the acceptance of the theory of elimination, viz., the premonitory diarrhœa is not to be arrested. We need not go beyond the consideration of this result to disprove the theory. Certainly, there is no fact in practical medicine better established than that cholera is prevented by arresting the diarrhœa which so often precedes the development of the disease. Let this fact have its due weight in the reflections of physicians on the theory of elimination. The impossibility of reconciling this fact with the theory, renders the latter untenable."

The International Sanitary Conference has published its reports and suggestions for the preservation of Europe from cholera, and we are indebted to Dr. Goodeve for an interesting summary of the proceedings. The latter comprise measures for the prevention of the development and spread of cholera, by purification, by sanitary measures, and by restriction. The Commission aims at the improvement of localities where cholera appears at all times more or less endemic, the exercise of efficient sanitary control over pilgrims and their places of meeting, over ships and other modes of conveyance, and a sort of quarantine of surveillance over travellers by sea and land—the latter to extend to cases of diarrhœa as well as of cholera, and to persons coming from infected places;—these steps are all based on the position, so generally recognised, of the transmissibility of the disease from person to person.

A most important feature which has been kept in view by the Conference is the increasing frequency of cholera in Persia, epidemics of it having occurred in ten out of eleven years from 1851 to 1862.

In Egypt, too, we may add, the same tendency has occurred, though not at all so frequently.

It cannot be concealed that great difficulty and expense will attend the proceedings requisite for the carrying out of these recommendations. The benefits to be derived from the prevention of the spread of disease would far outweigh and amply repay with interest the trouble and expense incurred. Moreover, the great importance of improved sanitary measures even in an educational point of view cannot be doubted. Late reports from Upper Egypt of the breaking out of the plague, and the great

mortality as well as the degraded state of the slaves, &c. there, and the horrid fact of throwing those dead of that disease into the Nile, the source of water-supply for all purposes to the whole of Egypt,—all call loudly for the prompt and energetic interference of Europe in that land of "slavery and abominations," as it is well termed by our contemporary.[1]

Truly, also, the state of India calls for our most active interference, nearer home so to speak, and we trust the Sanitary Commission in that country will be productive of great improvements.

Persia has set a good example in having twice prohibited pilgrimages at a time of cholera.

Dr. Warren Stone regards cholera as not personally contagious, and as not capable of being shut out by quarantine. "I think," he says, "though, that the filth and excrements of cholera patients in a confined place will generate the disease." He gives several remarkable instances of the propagation of cholera after exposure to the atmosphere of a ship in which cholera cases had occurred.

He does not regard the cause of scarlet fever, measles, small-pox, and "even plain, vulgar, intermittent fever, as less obscure than that of cholera." He does not think that referring cholera to a blood-poison helps to clear up the matter, and "accounting for disease by reference to the sympathetic nerve is explaining an inexplicable phenomenon by an inexplicable theory, or, in other words, by bare assumption." Dr. Stone shows forcibly the value of regular employment, a fair share of wholesome vegetable diet, with good meat and a sufficiency of stimulants, in times of cholera; he regards a craving for unwholesome food and drink as among the premonitions of the disease. In the early derangement of the bowels, he gave quinine, calomel, and opium, and sent the patient to bed: he allowed animal broth and a little brandy-and-water. If the discharges became copious and exhausting, he used astringents, as kino and moderate doses of opium. He looks on stimulating and heating substances, as well as sinapisms and hot salt, as causing distress and exhausting the patient; small doses, as half a grain or a grain, of calomel he has seen of much use, given every half-hour or oftener, "dropped on the tongue" until ten or twelve grains are administered. He attaches much importance to cold sponging, and the internal use of cold or iced water. We must, however, remember that our author wrote at New Orleans, and make allowance for climate accordingly. Moreover, we cannot now assent to his statement that the pathology of cholera has no

[1] The 'Lancet,' Feb. 23rd, 1867.

anatomical character. On the whole, we would, however, commend Dr. Stone's paper as well worth perusal. Mr. Simon in his 'Official Memorandum'[1] dwells upon the vast importance of at once disinfecting all matters, which the patient discharges from his stomach and bowels, and of preventing their subsequent admixture with other filthy substances or fluids, whereby the whole would become greatly more dangerous, as a means of propagating cholera, even though diffused through large volumes of water. The patient, his bedding, towels, clothes, &c., are all, in like manner, capable of spreading the disease. The memorandum shows how local circumstances powerfully co-operate in favouring the spread of the disease, if even a single case of the disease, perhaps of the slightest degree, occurs in a neighbourhood.

The document goes on to state that the only permanent safeguard consists in the existence of absolute cleanliness, and efficient works of sewerage, house-drainage, and water-supply. Useful instructions are given for disinfection of water to be used for drinking, &c. Boiling and the use of Condy's red disinfectant fluid are the means directed; the processes should be performed each day on the daily supply of water. Filtration cannot be trusted alone, but is a useful addition to either of the foregoing processes.

Mr. Glaisher has set an inquiry on foot as to variations in the height of subsoil water at periods of epidemic diseases, in the endeavour to ascertain the correctness of the statement of Professor Pettenkofer as to the dependence of the latter on the former. It is a fact that certain forms of fever have been observed in the marsh districts of England, &c., to depend on drought, long since. The fatality of fever among our troops in the Peninsular war, when marching in the dry bed of a river, is also a well-known fact. We do not, however, desire to detract from the merit due to the learned professor for his very important researches.

Professor Rolleston, of Oxford, not long ago published a very useful and excellent letter in the 'Spectator,' showing, in a very clear manner, several important practical points of sanitary science. He reviews Pettenkofer's five conditions for the spread of cholera, two of which depend on personal and three on local causes.

"The first personal condition is the presence, in the place in which cholera is to spread, of the particular and specific cholera poison, cell, or ferment, which originates in the *rejectamenta* of choleraic patients, and also in the excreta of healthy persons who have come

[1] 'Official Memorandum of the Medical Officer of the Privy Council,' July, 1866.

from choleraic districts. The second personal condition is the receptivity or susceptibility, often self-superinduced, of the person to be infected. The first local condition is a porosity and permeability to air and water of the subsoil. The second is the presence, at a greater or less depth from the surface of this porous subsoil, of what Pettenkofer calls *Grundwasser*—though he tells us that his opponents will not use his nomenclature—and what we call 'springs,' *and* also '*land*-springs' or 'subsoil water.' This second local condition is specially deadly when the level of the '*springs*,' or of the land-springs,' as the case may be, has just fallen unusually low, after having been previously unusually high. The third local condition, without which the diffusion of cholera is impossible, is the presence, more or less diffused, in a subsoil of the character specified, of those organic matters which modern sewage whirls away from our precincts, but which ancient slovenliness left to fester all around its houses in cesspits and middens. Now, in this as in many other cases, the sins of the fathers are visited upon the children, and soils may retain for almost indefinite periods the taint of organic impurities which were allowed to soak into them even generations ago."

Immunity from cholera cannot be expected on the sudden adoption of sanitary measures, but in all cases safety can only be reckoned upon "if these arrangements have been at work for a sufficiently long time."

Drs. Pettenkofer, Griesinger, and Wunderlich have published, in a small work, some valuable observations, of which Professor Rolleston has given a short *résumé*. They specially recommend sulphate of iron, for economic and chemical reasons, for the disinfection of sewage,

"Which will keep up in it an acid, in preference to the chloride of lime, which possesses and produces an alkaline reaction. For actual experience has shown that a particular atmosphere is necessary for the life of the cholera-germ, and this particular atmosphere is furnished by the alkaline exhalation of decomposing human sewage. The establishment of *Observirungs-spitals* for persons afflicted with premonitory diarrhœa is recommended in the pamphlet as well as in the papers published in the 'Zeitschrift.' Four per cent. only of persons so affected and so removed to 'houses of observation' were found to pass on into confirmed cholera—a result sufficiently confirmatory of the recommendation.

"But in these days, when English doctors and doctrines differ so widely as to treatment, it is important to say that the Germans, like most of our East Indian practitioners, recommend small doses of opium as the best medicine for precursory symptoms."

The Germans, while they regard choleraic excreta as the source of cholera, look on the general impurity of water as more favouring the spread of cholera "than by becoming the vehicle for the specific cholera-germ." At the same time, they

do not positively declare themselves, in this pamphlet, "to be the opponents of Dr. Snow's explanation" of such facts as have been put on record by the Registrar-General lately. The valuable labours of Dr. Budd at Bristol, and of other British physicians, in sanitary measures, are alluded to with just commendation by Professor Rolleston.

M. Bordier[1] (pupil of M. le Dr. Gubler) regards the occurrence of premonitory diarrhœa as a proof of power of resistance to the cholera-poison on the part of the organism. "Cholera" he applies to cases profoundly overcome with the poison from the first.

The disease presents three chief phases:—1st, "spoliation par des voies variables," which we might probably render as deprivation of vital fluid in different ways; 2nd, galvanisation of the vaso-motor filaments of the sympathetic; 3rd, consecutive relaxation of the vaso-motor.

Termed cholerine when mild, and cholera when severe, it is also called choléroïde—that is, when modified by previous illness.

Cholera may present characters by which it may be designated—sweating, meningeal, syncopal, "foudroyante," tetanic, typhoid. Cramps vary in frequency and intensity in inverse relation to the age and in direct relation to the muscularity of the sick person. They often yield to sinapisms or electricity [or, we might add, to chloroform applied on lint, or opium internally, or to chloroform inhalations.—*Reviewer*].

Diarrhœa and vomiting often alternate; the former depresses sooner than the latter. Always acid at first, the vomited fluid subsequently becomes alkaline, entirely bilious: they constitute then a true flux of the bile—"cholirrhée." When this ceases, jaundice often comes on.

The stools are alkaline, containing elements albuminous in their nature, resembling serum, yet differing from it. Many kinds of infusorial life are found in the stools, which perish when the matters grow cold. Dryness of the mucous and serous membranes follows the evacuations. The skin at this stage readily ulcerates and sphacelates, owing to its anæsthesia and loss of elasticity.

During the algide period, if urine is secreted, nitric acid turns it blue:[2] albumen, also, is present in quantity. On reaction, the blue and the albumen disappear. Uric acid and urea are now in excess; results of very complete oxidization and of sugar

[1] 'Archives Générales de Médecine,' Février, 1867; 'Epidémie Cholérique de 1866 à l'Hôpital Beaujon, par A. Bordier, Interne des Hôpitaux.'

[2] See p. 22. "Violet, purple, in some cases almost black, or a variety of colours."—Dr. H. Weber, 'Medical Times and Gazette,' August, 1866.

sometimes present in quantity. The glycosuria is connected, no doubt, with passive hyperæmia of the liver, which organ is often much enlarged at this period.

The gall-bladder is often found distended with a mucous liquid. The spleen is also enlarged.

Progressive paralysis of the mouth and of the alæ nasi is a frequent and almost always fatal complication, mechanical asphyxia supervening—a termination stated by M. Bordier not to have been heretofore described.

A considerable hyperæmia of the synovial membranes sometimes accompanies reaction, and the mucous membranes in the reaction stage often secrete pus copiously.

Convalescence is sometimes retarded by contraction of the extremities, and paralysis and wasting of the muscular system (*la paralysie amyotrophique*).

The remedies that seemed most useful in the formidable congestions of reaction were sulphate of quinine, coffee, taking blood, and belladonna.

From the earliest appearance of cholera at Southampton in 1865, from the interesting report of its progress in Gozo,[1] from the accounts of its outbreak in Dublin,[2] and from accounts from other quarters, we learn facts confirmatory of its contagiousness or portability by the person, &c., and of its tendency to cling to localities or individuals suited, as it were, for its reception.

The recent epidemic, which still[3] smoulders in some localities in England, Ireland, &c., has presented peculiarities differing from the features of the disease of 1849 and 1854. Of these we regret to observe that its malignancy[4] was rather, on the whole, increased—that very fatal effects were produced in a relatively shorter time—that most marked benefit was obtained in London generally from the active measures of hygiene adopted there for some years past; while (as observed by the Registrar-General) negligence and ignorance, wherever existing, entailed tremendous loss of life at home and abroad.

Dr. Camps advocates the use of quinine, salicine, or arsenic, as prophylactic against epidemic cholera and epidemic diarrhœa.

Surgeon-Major Blacklock advances the idea that smallpox and cowpox may afford protection from cholera.

[1] 'Statistical, Sanitary, and Medical Reports of the Army Medical Department,' vol. vi, 1866.

[2] 'Medical Times and Gazette,' Dec. 15th, 1866.

[3] December, 1866.

[4] This hardly applies to 1849.

Dr. Tucker advocates the treatment of cholera by salines, as proposed long ago by Dr. Stevens.

Dr. Dudley Kingsford puts cholera down to the agency of phosphorus "in some form!"[1]

"A Physician" advocates the keeping the blood in circulation in cholera by mechanical motion ("rocking or see-saw;" and when we see that the rest of the treatment of this anonymous writer is keeping the patient in a temperature of 120° to 150°, and rubbing the body with brine, we will be excused from recommending the rest of the practice).

Dr. Howe refers the epidemics of cholera of 1831-32, 1848-49, and 1866, to lunar influence, but leaves out the epidemic of 1854. The latter would appear not to tally with the periods necessary for the perfection of this theory.

Although the epidemic has subsided, isolated cases still demand the vigilant attention of the authorities. This is one of the lessons of the year; for, while cholera raged between the Tower, the Isle of Dogs, and Victoria Park, over the area traversed by the Blackwall Railway, and the Great Eastern Railway as far as Stratford and West Ham—among a population in uninterrupted communication, through lines of streets, the Regent's Canal, and the Thames, with the rest of London and the surrounding districts, to which many of the infected persons living in the east of London fled,—the conflagration was strictly circumscribed within well-defined bounds. The fierceness of the disease extended thus far, and no farther, although the movements of the people were as free as air. The futility of quarantine lines is so well known, that their establishment was never proposed in London. The supply of impure water was at once arrested; the water-engineers grew careful; the sewage was now less inefficient than it had ever been before, for South London was drained; the health officers became vigilant; premonitory diarrhœa was treated; every case as it occurred was published to the world; and the cholera excreta were destroyed by carbolic acid and other disinfectants. The disease was communicated by contact with the poison in a few cases, but its general diffusion was stopped.

This is the secret of the success of London in controlling an epidemic that during the year has been so fatal in continental cities.

The Old Ford reservoir supplies six districts in the east of London, and West Ham and Stratford, sub-districts, with a population of 531,921, of whom 4104 died of cholera in the twenty-three weeks from 30th June to 1st December, being in the proportion of 77 deaths to 10,000 living.

[1] Dr. Honigberger claims inoculation of quassia as a cure for cholera.

The rest of London, exclusive of the above-named districts (and not supplied from the Old Ford reservoir), contains 2,566,882 persons, of whom only 1819 (or 7 in 10,000 persons living) died of cholera in the same period. While the general result of the costly drainage-works of London and of the improved water-supply is thus satisfactory, it is well that a commission has been appointed to inquire into the state of our rivers as to pollution from sewage, &c. We would suggest also that a full investigation into the facts of the outbreak of cholera, in October last, at Woolwich and Plumstead, near the southern outfall sewer, be held.

Dr. Chevers[1] gives a most appalling, yet, no doubt, a perfectly true picture of local causes of disease in the Delta of the Ganges, to which we must refer our readers. The wonder, indeed, is, not that cholera should issue forth from such a "maelstrom of death," at intervals carrying mortality to the remotest regions, but that it ever ceases doing so.

The abstract we quote from above gives an instructive paper, by Mr. D. J. Moore, on the great impurity of the water-supply in Bengal generally.

In reply to the Council of the Epidemiological Society, a large number of the distinguished members of the medical profession, including the presidents of the Royal College of Physicians and of the General Medical Council, the directors-general of the Army and Navy Medical Departments, &c. &c., stated their opinion—1. That it is, on the whole, unadvisable that cholera patients be admitted into the ordinary wards of general hospitals or infirmaries. 2. That cholera patients can be safely admitted into special wards in general hospitals, due precautions being taken; and therefore, that it is desirable, as an important means of providing accommodation for the destitute when attacked, that the authorities of these institutions grant this valuable benefit to the public. 3. That it will be often necessary that special hospitals be provided in aid or in lieu of general hospitals and infirmaries.

The Council also would recommend places of refuge for the poor inmates of unwholesome dwellings and localities, though unattacked with cholera, when cholera has appeared.

Dr. Crisp observes, alluding to Dr. George Johnson's spasmodic theory, "It is a mere assumption, unsupported by proof." But, assuming that Dr. Johnson's theory of spasm of the minute arteries is correct, what a labyrinth of difficulties we get into if we take this view of the question! If these pulmonary arteries possessed muscular fibres, and they were in a

[1] 'Indian Annals of Medical Science,' 1866. See the 'Half-yearly Abstract of the Medical Sciences,' 1866, vol. xliv, p. 5, &c. London: J. Churchill and Sons.

state of spasm, what is so likely as opium to relieve them? And could the advocates of the opium treatment, if they are converts to Dr. Johnson's spasmodic theory, have a better proof of its correctness? But, unfortunately for them, Dr. Johnson ignores both opium and the warm bath, &c. Further on, Dr. Crisp states that what he regards as the hyperæmia seen in some organs is "due rather to the loss of the *vis a tergo*, the failing powers of the heart, and to the thickened state of the blood," &c. He also dwells upon the important effects of the non-passage of bile (in general, in cholera) into the intestines.

It is not our province to follow this author into his appendix on the cattle plague, or his letter on homœopathy, and the present state of the medical and veterinary professions, &c.

Dr. Shrimpton asserts that cholera is not contagious; advises that the poor be treated in their own homes, except the worst cases, which must be sent to hospitals. He disbelieves in disinfectants, but highly approves of cleanliness.

Dr. Peters insists on a long-continued and most rigid quarantine, on cleanliness, and on disinfection, as the means for preventing the spread of cholera.

Though giving some of the opinions of many celebrated authors, as well as of others of less note, the work of Dr. Peters is not correct in some respects. Thus, at page 156 we find the following:—"One of the most striking features of cholera is, that all the discharges are acid—none have ever been found to be alkaline; the stools as well as the vomits;" &c. The reader will find this statement does not agree with the researches of M. Bordier given in another part of the article: it also varies from those of Pettenkofer and others.

It is hardly necessary to remark that the views of Dr. Peters on quarantine are impracticable in the present age of locomotion.

Dr. Haughton alludes to the idea of some, that the cholera in 1832, 1849, and 1866 has relation to meteoric phenomena, recurring at intervals of seventeen years.[1]

"Although," says Dr. Haughton, "the influence of such periodical astronomical causes in aiding the conversion of endemic disease into epidemic disease cannot be denied, simply because it can neither be proved nor disproved, yet it is inconsistent with the rules of inductive science to admit a cause for a phenomenon which is only a possible cause, so long as a probable cause exists whose influence is real. We are, therefore, entitled to reject such causes so long as we are prepared to assign more probable causes for the phenomenon, which in case of cholera is always possible; and, even at the best, the astro-

[1] Dr. Howe, the author of this idea, ignores the epidemic of 1854: it would interfere with his theory.—*Reviewer.*

nomical causes of cholera would reduce themselves to meteorological causes of wide-spread influence; for it is only by influencing the constitution of large regions of the atmosphere that meteoric showers or other astronomical influences could make themselves felt."

The facts of the first appearance of cholera in Dublin in 1866 are then stated; its victims within the year numbering 1193. Dr. Haughton traces out the spread of the disease to the case of Magee, a woman "who imported the disease from Liverpool, July 26;" and we have no doubt that the succeeding papers of this clear and scientific observer will form, with what has already appeared, a valuable addition to our knowledge of the workings of this dire disease.

In Paris, in 1865, warm baths were not approved, on trial, in cholera cases. Dr. A. Clarke, physician to the London Hospital, regarded them as beneficial. Injection into the veins was approved of by some in extreme cases of collapse; others did not approve of the measure. M. Jules Besnier[1] attributes the asphyxia of cholera to pulmonary congestion, and the deposit of a reddish, viscous matter, forming a kind of jelly, on the surface of the bronchial mucous membrane. In this tenacious matter the miscroscope revealed a considerable number of cells, some narrow and elongated, others larger and irregular, and both kinds provided with vibratile cilia at one extremity.

[2]The *braise de boulanger*, a sort of charcoal remaining from wooden fagots used in heating ovens in France, was used as filter-beds for water. Sulphate of iron was used to disinfect *les fosses d'aisance*, sewers, &c. Ammonia, contrary to the ideas of Pettenkofer and others, was recommended as a gaseous disinfectant.

Carbolic acid should be placed at every aperture by which air escapes from a cholera ward.

Sir Henry Cooper regards arrest of the diarrhœa stage of cholera by astringents as of the first importance:

"It is the duty of those in authority in cholera epidemic seasons to search out and arrest *all* cases of diarrhœa by the organisation of a sanitary police for the detection of the disease, and its treatment in its earliest stages."

The Calabar bean was proposed as a remedy in cholera, but was speedily abandoned, as causing enteric hæmorrhage and adding to the danger of the cases.

Dr. Morehead objects to the dogma that "in India it should be a rule to treat every cholera patient in a tent," arguing that heat and cold, dryness and moisture, as well as good nursing and

[1] 'Archives Générales de Médecine,' Sept. 1866.

[2] 'Gazette des Hôpitaux,' Sept. 22, 1866, and 'Med. Times and Gazette,' Sept. 29th, 1866.

ventilation, must be taken into account, especially in a country so extensive, and so varied and variable in its climate, surface, &c., as India is.

There are some other points in Dr. Morehead's pamphlet worthy of attention.

Although by no means new treatment, some practitioners attributed benefit to the use of warm enemata in cholera.

Dr. D. Menzies used ice in small portions in cholera in India with good effect. [In some cases at home also it appeared useful.]

Dr. M'Cloy, of Liverpool, recommends Dr. Johnson's castor-oil treatment, and condemns the ice to the spine treatment as "miserably unsuccessful."

So fatal were the results following the trials for a while made by some junior practitioners in Dublin with castor-oil, that the physicians of some of the hospitals open for the reception of cholera cases declared that they would not continue to admit cases so treated.

Mr. Sanson's little work contains some useful rules on disinfection, and the agents to be used. He refers the train of cholera symptoms to the influence of "a real, actual poison," irritating the great sympathetic nerve. He would seek to combat its effects by sulphite of soda or carbolic acid, given internally in properly diluted doses. Too much stress is laid by this writer on anatomical states of the vascular system of which we have by no means sufficient proof, and the agents above named are too slow in their action for the disease under consideration. Many of the author's suggestions as to diet, good nursing, &c., are well worth remembering.

Professor Frankland, in a communication to the Registrar-General, states the result of his investigations into the nature of cholera evacuations. Water may become seriously contaminated with choleraic matter without the presence of the latter being indicated by chemical analysis; and, secondly, water so contaminated is not completely deprived of this impurity either by filtration or passage through animal charcoal. It still remains to be proved to what particular constituent of choleraic dejections the propagation of the disease is due; but it is obvious that if the propagating matter be a germ or an organism, it must be in suspension, and not in solution.

Sir Dominic Corrigan, without altogether denying the existence of contagion in cholera, argues that the comparative infrequency of the spread of the disease in 1848 and 1849 in Ulster and Leinster, and the fact that every town in Connaught and forty-two out of forty-seven towns in Munster suffered from

the disease, is against the contagion theory, as the latter districts are remote from the chief traffic routes. On considering, however, that the vast majority of those who travel to and from the cities and districts in England for harvest-labour, &c., and whose habits of life render them most likely to suffer from the disease, dwell in Connaught and in Munster, it appears to us rather to favour than otherwise the contagion theory of the disease.

The sound advice given by this author in regard to the vast importance of improving, as far as possible, the general health of every one during the prevalence of cholera, cannot be too forcibly impressed upon the whole community.

Dr. Shapter, of Exeter, has been long known to the profession and the public as a writer on cholera, especially in connection with Exeter. He looks hopefully on early treatment of the diarrhœa which usually precedes the disease, and points out the great value of sanitary measures.

He regards the consecutive fever as more an accident than as a necessary part of the disease, for often "the worst cases of collapse are recovered from rapidly and without any such fever." "Collapse from other causes, as long exposure to severe cold, is often succeeded by a form of fever precisely similar in all its main features."

His experience is in favour of opium early in the disease, and of mercury either with or without opium in the advancing stages of the disorder. In advanced collapse and in consecutive fever, he regards opium as obviously and immediately injurious. If nausea and vomiting, however, continue persistently, he thinks a dose or two of opium useful.

Dr. Cockle's little work is a valuable *résumé* of the leading theories of the disease, evincing accurate acquaintance with the literature of the subject.

"That cases may do well without our aid, we sometimes see, for cholera has its *plus* and *minus* forms. But, face to face with the disease in its more serious shape, eye-witnesses of the telling influence of the oft-frightful drain, does it impress us as nature's act to cure, or carry with it one character of safety? Do patients tell us, at each escape of fluid, they feel relieved or have their strength increased? for such are the results that should attend eliminative or critical discharge. If they do, their looks belie them strangely. In such a state, no matter how we regard the disease, fever or flux, irritation or poison—no matter how we treat it,—calomel to restore the bile, salines to moderate the congestion, or opium and astringents to repress,—again and again we seize the symptomatic indication, and try our best, each in his way, to stop that vomiting and purging, alike the symptoms and the danger of epidemic

cholera.[1] Is this mere routine, or the settled conviction of reasoned experience? If we are wrong, we have at least for our consolation the example of our betters; take that of the late Dr. Graves, a physician of more than ordinary fame, or look abroad and search opinion there;[2] and in the face of such teaching, it must indeed require overwhelming evidence in favour of an opposite procedure; for the experience of able men, when theories clash, must be the fitting and the only guide. One closing argument in favour of the arrestive plan may be drawn from the state of the epithelial lining of the gastro-intestinal tract: the larger the drain, the greater the destruction, and the more tedious and intractable its repair."

The debate on cholera in the Harveian Medical Society of London has been published. It contains the opinions of the several speakers on the occasion, which, however, it would be very difficult to introduce in the form of a review. Several of the members advocated excellent measures of a sanitary nature. The introduction by Dr. Drysdale, the secretary, contains a good outline sketch of the chief features of the disease.

Professors Pettenkofer, Griesinger, and Wunderlich, and Dr. Macpherson, "are perfectly agreed that cholera is propagated by means of subterraneous waters."

Dr. Klob, according to intelligence from Vienna, "has, with the use of a microscope of from 800 to 1000 magnifying power, discovered in the rice-water evacuations millions of microscopic fungi, which in appearance differ little from the ordinary European forms; and that they form the basis of the frightful malady, and that cholera is easily propagated by their means, can scarcely any longer be doubted."

As if to impress upon the profession of medicine the great importance of pressing upon "the powers that be" the vast interests at stake in the promotion of state or preventive medicine, accounts are almost daily received, showing the great necessity for such steps as will lessen the growing tendency to

[1] Unless some singular error prevails, the arrestive treatment yields by far the best results. According to Boudin, 'Traité de Géograph. et Statist. Médicales,' p. 366 (1857), the mean mortality is thus stated:—

Evacuant treatment	.	71·7 in 100	To understand the exact arrangement of his tables, the original work must be referred to.
Stimulating „	.	54 „	
Alterative „	.	36 „	
Astringent „	.	20 „	

[2] "Ainsi, en général, plus les évacuations sont copieuses et prolongées, et plus le choléra est fatal."—Majendie, p. 133.

"C'est surtout à diminuer les évacuations qui doivent tendre les indications symptomatiques. Rien ne mène plus rapidement à la ruine des forces, rien ne hâte davantage le progrès des plus redoutables symptômes, que la fréquence des vomissements et des selles. Non-seulement alors toute médication, toute réparation, sont impossibles, mais il en résulte encore un épuisement général et une perte absolue de toute résistance vitale, par les déperditions et par la lassitude que causent les besoins sane cesse renaissans de ses déjections continuelles."—'Rapport de l'Acad. Roy. de Paris,' 1831, p. 77.

grave disease. Thus, in some parts of the West Indies, we learn, cholera, yellow fever, and smallpox prevail together; in Naples smallpox rages;[1] while in Jersey,[2] as in other localities still nearer home, as we have in another part of this paper stated, cholera has broken out. Several of the Jersey cases appear, as was also the case in other places last year, to have originated at the "wake" of two of the first persons dead of the disease, no less than ten of the persons who attended the wake having died of the disease.

Though the disease is stated as being prevalent close to St. Brieux in Brittany, no importation of the disease has been traced in the Jersey outbreak.[3]

Dr. Chapman asserts that ice to the spine is the remedy in cholera and diarrhœa, reasoning upon the theory that these diseases depend on the vascular condition of the spinal cord, and the power of ice to control that condition. We would ask for his evidence of this condition of the spinal cord. He overlooks the changes recognised in the blood in such cases as he asserts are under the control of ice to the spine. That cold to the spine has an influence in hyperæmia of the spinal cord, is not a new statement.

Dr. Chapman will have seen, long ere this, that the comparatively successful results of cases classed as cholera in Russia depended not upon the cooler climate, as suggested by him, but upon the principle pursued there of classing as cholera, cases of choleraic diarrhœa.

Having lauded the efficacy of his treatment in almost all "the ills to which flesh is heir," he says, "I am aware that the large claims put forward in this section on account of the remedial method in question are likely to be met generally with scepticism, and not seldom with ridicule," &c.

We cannot regard him as the author of what he styles "a newly discovered general law."

Dr. Cockle is far from denying the contagiousness of cholera under certain conditions, doubting only whether it be through the medium of *recent excreta*.

He regards cholera as the result of some special agent profoundly modifying the function of the extensive gastro-intestinal tract, rapidly radiating back a paralysing influence on the great nervous ganglia connected with the spino-sympathetic centres, and suddenly abstracting from the circulation, under such depression, a large amount of blood constituents, &c.

[1] 'Lancet,' Feb. 23rd, 1867. [2] Ibid.

[3] Since this was put in type, it has been thought that importation was engaged in the production of the disease in the island.

SUMMARY.—The importance of the many questions arising out of the recurrence of the cholera has led, as may be seen by the heading of this article, to a great amount of thought and of writing, as well as to an European Conference, and has thrown into action numerous governmental and scientific bodies.

As in previous epidemics, our knowledge at present points to the removal of those conditions which have been seen to favour the production and spread of the disease, as well as to the arrest of the earlier phases of the malady, which recent experience, as well as that of former times, teaches us are the most hopeful for treatment.

The undaunted courage of the profession, as well as its zeal and perseverance in the face of danger and difficulties, have added considerably to our knowledge of the disease and of its sequelæ.

In the latter we frequently find evidence of the engagement of the nervous centres; but whether such disorder is attributable to the disease in itself, or to the serious changes in and loss of so much of the constituents of the blood, is not clearly known.

There is much in the symptoms, both concurrent and succeeding (eruptions of the skin, &c.), to lead to the supposition that cholera is allied in its nature to the results of irritation of the stomach and nervous system by poisonous ingesta; at the same time, it appears to possess a considerable relationship to some diseases of malarious origin, as well as to those classed as exanthemata.

In the opinion of the International Sanitary Conference, and many other observers, water-closets should never be within houses, nor should the latter be in communication with sewers.

The Conference recommends the "dry system" in lieu of that of water-closets.

Almost all regard intercommunication of mankind as the means of spreading the disease.

The predisposing causes of the disorder are of two kinds—individual and local.

At the present moment, no people living in what is termed a state of high civilisation can reckon upon immunity from cholera.

It is the duty of every country to use its utmost endeavour to lessen, and if possible, and as soon as possible, to remove the sources whence the disease springs or spreads.

The occurrence, in some parts of the United Kingdom, of a form of disease most generally fatal in a few hours, attended with shock to the system, collapse, with frequently evidence, both symptomatic and post-mortem, of disease of the brain and

spinal cord, with more or less, in some cases, of tetanic symptoms, attended often with feelings of extreme cold and with vomiting, and in some cases with purging, and by the occurrence of numerous patches of dark-coloured blood effused either just under the cuticle or through the whole thickness of the integuments, is not a little remarkable.

In some of those cases, sphacelus of portions of integuments and other soft parts occur.

It is for the most part in the young, or in persons in the prime of life, that this form of disease has appeared, in this point as well as in some others bearing some analogy to cholera.

The symptoms often commence at night or in the early morning, and with great suddenness.

Apparently clinging to some localities—in some instances, at least, very low in hygienic conditions—the disease has not afforded much, if any, evidence of contagiousness.

Several cases of a like nature occurred in the early part of last year.

In some instances a distinct urticaria occurred, mixed with vesicles[1] of tolerably large size, intense itching causing some of the sufferers actually to tear themselves.

Cerebro-spinal apparently in its nature, the disease just mentioned possesses sufficient analogy to cholera to claim this short notice here; and its study in connection with that of the several stages of cholera, and of the consecutive fever of the latter, promises, we think, to throw light on each form of disease.

The tendency is at present to take enlarged views of disease, and its modifications by climate, race, &c. In this way we are led to consider cholera as well worthy, in every way, of the close attention of observers, being convinced, as we have already said in previous numbers of the 'Review,' of its importance, not only on its account of its being at least one of the most formidable plagues, but also of its tendency to throw light on the morbid action in other diseases.

[1] See Hebra, 'Diseases of the Skin,' New Sydenham Society, 1866, vol. i, p. 395.

Review II.

On Malformations of the Human Heart, &c. With Original Cases and Illustrations. By Thomas B. Peacock, M.D., &c. Second Edition. pp. 204. 1866.

Cardiac malformations have proved an attractive subject for medical writers and readers since physiology and pathology received that impetus by the discovery of the circulation they have never since lost. How little previously was known as to the normal or the abnormal structure of the heart is illustrated by the fact that cases of malformation were assumed to represent the healthy type of structure by some of the most celebrated of the opponents of Harvey. Readers of medical history will remember that Gassendi and Cæcilius Folius both quoted instances of permanently patent *foramen ovale* as evidence that the blood naturally passed from the right to the left side of the heart by a direct communication. Even Cæsalpinus, one of the trio who shared the honour of having, before the time of our great countryman, most nearly approached the true doctrine of the circulation, retained the Galenical notion, which we may surmise had its origin in the study of an imperfect organ, that the *septum ventriculorum* was permeable; although, in justice to the anatomy of the sixteenth century, it should be remembered that the solidity of the partition in the normal condition had been asserted by Berengarius and proved by Vesalius. Since the time of Harvey, however, cardiac malformations and malpositions have attracted peculiar attention. First observed and noted among the curiosities of nature, they have acquired a far higher interest and importance with the growth of physiological and embryological science. The light thrown on them by the revelation of the phases through which the fœtal organ passes in the course of its development, and by a study of the permanent conditions of the heart in the Hæmatocryal Vertebrates, enhances the fascination which the observation of deviations from the ordinary course of the circulation must have for the student of physiology and animal mechanics. It might be expected, therefore, that a large mass of materials would exist for a more or less complete treatise on the subject; and, in fact, the medical journals of Europe and America abound with cases of malformation. In addition, there are many valuable essays on the subject by writers of the highest eminence in this country—as Farre, Williams, Todd, and Norman Chevers; and on the Continent, Meckel, Louis, Gintrac, Andral,

Tiedemann, Förster, Bouillaud, Breschet, and many other distinguished men, have contributed to its literature. Without in any measure detracting from the just repute of these, we believe that we are only stating the acknowledged truth when we assert that, for comprehensiveness and completeness, the work the title of which stands at the head of this article is without a rival. Good wine, however, needs no bush, and we believe that the highest recommendation we can offer of Dr. Peacock's valuable and full treatise is a short, and therefore necessarily imperfect, analysis and summary of its contents.

The first two chapters are devoted to the consideration of misplacements of the heart and cases of deficiency of pericardium. The former are divided into misplacements within the thorax, and those in which the heart is situated external to that cavity. Of internal misplacements there are two: *Dexiocardia,* where the heart is placed in a position on the right side of the chest corresponding with that which it normally occupies on the left; and *Mesocardia,* where a malformed heart remains in the median position in the thoracic cavity it held in early fœtal life. Transposition is the more common of the two, and it is frequently accompanied by transposition of all the other viscera. But this is not invariably the case, neither are the arterial connections of the transposed heart always the same. In many cases the heart is well formed, and the vessels retain their natural relations; or, without any malformation of the heart itself, the arteries may be misplaced relatively to the ventricles; or, where the heart itself is malformed, other irregularities may coëxist. The course of the aorta varies in cases of this kind. Sometimes it crosses over the right bronchus, and passes down to the right of the vertebral column; or it has been observed to cross over the right bronchus and behind the lower end of the trachea over the bodies of the vertebræ to its usual position. When the aorta lies to the right of the spinal column, the great vessels arising from the arch may or may not be transposed. Of misplacements external to the thoracic cavity, some very curious examples are given under the heads of *Ectocardia pectoralis, Ectocardia abdominalis,* and *Ectocardia cervicalis;* the most remarkable perhaps being the case of a soldier, given on the authority of M. Deschamps, of Laval, in whom after death the heart was found occupying the place of the right kidney, the vessels arising from it passing through an opening in the diaphragm into the thorax. Dr. Peacock has collected several undoubted examples of absence of pericardium, although, as he observes, some of the recorded cases have probably been instances of universal adhesion. In many of the genuine instances, a rudimentary pericardium in the form of a crescentic

fold of serous membrane containing fibrous tissue has been observed at the upper and right side of the heart. This was so in the cases exhibited by Drs. Baly and Bristowe at the Pathological Society, and in one examined by Dr. Peacock, who offers the following explanation of its occurrence:

"I do not know that the mode in which the pericardium is developed has been made the subject of investigation; but from examining the adult heart, it would appear to be a continuation of the fibrous sheath of the vessel to the diaphragm and over the heart. When the membrane is fully developed, and the layers passing in front and behind the heart come in contact on the left side and become adherent, the sacs of the pleura and pericardium will be distinct; but if the growth be arrested, so that the two layers do not become united, the heart will lie in the pleural cavity, and the pericardium will only be represented by the crescentic fold, consisting of fibrous tissue covered on each side by pleura, which has been noticed as existing at the right side of the heart in nearly all the recorded cases."—Pp. 12, 13.

The classification of malformations adopted by Dr. Peacock is founded partly on the period of fœtal life at which the development of the organs becomes arrested or perverted, and partly on the degree of impediment to the circulation which the abnormity occasions. The former ground of classification was adopted by Friedberg in 1844, but was also chosen by the author in the first edition of his work at a time when he had not seen Friedberg's treatise. It is one that would naturally present itself as soon as the normal development of the organ had been traced, and it is certainly more philosophical and preferable to other classifications founded on the admixture or non-admixture of black and red blood, malformations deviating qualitatively and quantitatively, &c. In the author's first division are placed malformations depending on an arrest of development at that period of fœtal life when we are reminded by the single auricle and ventricle of the permanent condition of the organ in fishes. There are now many cases on record of children who have survived their birth some days, or even weeks, in whom the heart has consisted of two cavities, an auricle and a ventricle, and a common arterial trunk has supplied both the pulmonic and systemic circulations. One of the earliest noticed of these cases was brought before the Royal Society by Mr. Wilson in 1798: the child lived seven days, and, though occasionally livid, was generally pale. There was an absence of the lower part of the pericardium and of the tendinous portion of the diaphragm. The heart rested on the liver, and was contained in a sac, the sloughing of which caused the child's death. There were a single auricle and ventricle,

and one arterial trunk, which divided into two branches—one giving off the pulmonary arteries, the other ascending behind the thymus gland, and then giving off the usual aortic vessels. A higher stage of development has been reached when a partial division of the auricle is present, although the heart is still strictly biloculate. Of this grade of arrest the author has collected several examples, one of the best marked being a case related by Dr. Vernon in the thirty-ninth volume of the 'Medical and Chirurgical Transactions.' We would notice, *en passant*, that in two cases of biloculate heart, life has been prolonged into the third month after birth.

The next ascensive step is that in which the heart consists of three cavities—a right and left auricle and a ventricle, suggestive of its permanent condition in *Siren*, *Menobranchus*, *Axolotes*, &c., and, in fact, of the general type which prevails throughout Batrachia, Ophidia, Lacertilia, and Chelonia; albeit the partial ventricular septum existing in the single ventricle of the three latter orders indicates, perhaps, more closely the next grade at which an arrest may take place. Dr. Peacock states that instances of this kind of malformation, although very rare, are of more frequent occurrence than the biloculate heart. In these cases there is more or less complete separation of the auricular sinuses, and there are generally two auriculo-ventricular apertures. The ventricle is undivided, or presents a very rudimentary septum. The aorta and pulmonary artery generally arise separately; but in some cases the pulmonary artery, though existing, has been found impervious at its orifice, its branches transmitting blood to the lungs from the aorta through the ductus arteriosus. The aorta and pulmonary artery also may occupy their ordinary positions, or they may be transposed, as in a case described by Breschet, in which also there were two communicating arteries in place of the ductus arteriosus, and two descending and two ascending cavæ —the latter peculiarity, which has been more than once observed in connection with malformed heart repeating the bi- pre-caval type, which is common to reptiles, birds, and the lower mammalia. It would appear that life may be prolonged to a much greater extent in the case of this than in that of the former stage of arrest. In an instance noted by Kreyzig, the subject of a triloculate heart lived twenty-three years.

The conditions hitherto noticed constitute the author's first division of malformations, viz. those consisting in arrest of development occurring at an early period of fœtal life. In his preface, the author specifies from the fourth to the sixth week as the period when the biloculate condition would by arrest of development be stamped on the growing organ. But whilst

accepting the author's division of these malformations, it should be borne in mind that the normally developing fœtal heart probably never consists of two auricles and one ventricle; for, according to some of the best authorities, in the progress of normal development the inter-ventricular septum is formed first, commencing about the fourth week and being complete about the eighth, whilst the septum between the auricles does not commence until the ninth week. The malformed heart consisting of three cavities would, therefore, be the result of arrest in growth of the inter-ventricular septum at a period when the two auricles still form one cavity.

Of the malformations consisting in arrest of development occurring at a more advanced period of fœtal life, the first group defined by Dr. Peacock is one in which the heart consists of four cavities; but one or both septa are imperfect, the pulmonary artery and aorta being more or less completely developed. Incompleteness of the ventricular septum varies in degree from the condition in which it is only a muscular band projecting into the cavity, to that in which only one or two small apertures exist at its upper part. In most cases the imperfection will be found at the base, where the division of the cavities is last effected, and where a triangular undefended space almost devoid of muscular fibre indicates the permanent communication between the two aortic ventricles in Chelonia. When one or more openings occur at this spot, if there be no source of obstruction on the right side, so that the relative proportions of the two ventricles be not interfered with, the flow of blood will be from the left to the right ventricle, as indicated by the greater size of the openings on the side of the former. Also, as was first pointed out by Dr. Thurnam in a specimen in the museum of the Royal College of Surgeons, as the apertures on the right side are immediately below the auriculo-ventricular opening, the folds of the tricuspid valve may become expanded and permanently sacculated by the abnormal current of blood. Deficiencies may exist in any part of the septum cordis, although Dr. Peacock believes that perforations between the left ventricle and the infundibular portion of the right are very rare. Together with the deficiency in the inter-ventricular partition, there may or may not be an incomplete septum between the auricles; or, on the other hand, the auricles may be imperfectly divided, whilst the ventricles are completely separated. There may be merely an unclosed state of the foramen ovale, or an aperture in the auricular partition; or the septum may be scarcely developed and the valve entirely absent, and various intervening degrees of deficiency may be found. Occasionally, the aperture exists at the base of the heart, where the septa between the

auricles and ventricles should unite, and thus intercommunication is established between the four cavities. A case of this kind is related by M. Thibert: the man lived to adult age, and presented no sign of disease of the heart until six weeks before his death. Dr. Peacock readily acknowledges that perforations in the septum ventriculorum may be the result of disease; in fact, he himself states that he has seen two cases in which disease existed at the undefended space at the base of the left ventricle, which would have led to perforation had life been prolonged; but he combats the idea advanced by Bouillaud, that most of the cases quoted by the latter author were produced by morbid processes after birth, and he appeals to the condition of the apertures as establishing a ready line of demarcation between the congenital and the morbid cases. In many of these cases of imperfection in the septa, there are, of course, associated other deviations from the normal condition, such as obstruction at the pulmonary or other orifice; but it is a remarkable fact that in other instances large apertures have been formed after death in one or other of the septa in persons who have died from causes unconnected with the heart, and who during life had shown no sign of cardiac disease. Of course, in such cases the organ was otherwise normal.

A second group is formed by cases in which there is defect of the inter-ventricular septum with constriction or obliteration of orifices and misplacement of the primary vessels. An incomplete septum ventriculorum is often found to deviate from the normal position, so as to give rise to misplacement of the aorta and pulmonary artery; and such deviation is commonly found coëxisting with obstruction to the flow of blood through the pulmonary artery, more rarely with constriction at one of the auriculo-ventricular orifices or at the aortic aperture.

"In one of the most interesting forms of anomaly, the deviation of the septum is to the left, so that the right ventricle is of large size, and the aorta arises wholly or in part from that cavity; and this condition is most generally associated with obstruction to the passage of the blood from the right ventricle. The source of obstruction in such cases may be situated either—1st, at the outlet of the pulmonary orifice, or at the free edge of the valves; 2ndly, at the line of attachment of the valves to the fibrous zone, or the termination of the infundibular portion of the ventricle; 3rdly, in the course of the pulmonary artery; or, 4thly, at the commencement of the infundibular portion of the right ventricle, or the point of union of the infundibular portion and sinus."—P. 32.

The cases in which with stenosis of the orifice of the pulmonary artery there are imperfection of the ventricular septum and a partial origin of the aorta from the right cavity, are very

numerous; in fact, as was remarked by Dr. Farre, they are probably more common than any other kind of deviation. The recorded cases, however, differ in important respects. In some the foramen ovale is closed, but in the majority it is found open; or there may be an aperture in the auricular septum. The amount of misplacement of the inter-ventricular septum varies: in some cases only a small portion of the aortic orifice communicates with the right ventricle; in others the aorta is placed immediately over the incomplete septum and communicates equally with both cavities, and in some instances it has been found arising wholly from the right ventricle. The ascending aorta, when it communicates with the right ventricle, is always of large size. The ductus arteriosus in some instances remains pervious. The obstruction to the flow of blood through the pulmonary artery, if it be at the outlet of the orifice, may be due to disease or malformation of the pulmonic valves. The number of segments may be reduced to two by the union of contiguous segments; or the valve may form a perforated diaphragm across the artery, having a funnel shape, from the pressure of the current of blood; or the orifice may be a mere slit, or triangular, or rounded. Constriction at the outlet of the *conus arteriosus* may depend on disease of the fibrous zone or of the adjacent structures. Obstruction may also be due to disease and consequent constriction in the pulmonary artery itself, the amount varying of course considerably in different instances. In all these cases the pulmonary artery will be found smaller than natural, and its coats thin and transparent. The trunk of the vessel, however, will be commonly larger than the orifice, and this when from the closure of the ductus arteriosus no blood from the aorta can pass into it. The infundibular portion of the right ventricle is also found imperfectly developed; but the remaining portion of the ventricle—the sinus—becomes hypertrophied, and the right auricle is also found dilated and its walls thickened. In persons who survive for some years, it is common to find that similar changes have taken place in the left ventricle, and the valves on the left side are not unfrequently found diseased or irregular. Some of the French pathologists have referred the hypertrophy of the right ventricle observed in such cases to the entrance of aërated blood into the cavity through the perforation of the septum. Dr. Peacock, however, argues that where there exists obstruction to the exit of blood from the right ventricle, the current will pass from the right to the left cavity, and no aërated blood will enter the former. No doubt, the true explanation is to be found in the share of the systemic circulation thrown upon the right ventricle. An equal amount of hypertrophy and dilata-

tion may result when there is obstruction at or near the pulmonic orifice without any perforation of the ventricular septum or any entrance of aërated blood. The relations of the incomplete septum, the malposition of the aorta, and the obstruction to the exit of blood by the pulmonary artery, have been variously explained. In the first edition of this work, Dr. Peacock advanced an explanation which, as he says, seemed a necessary sequence to the view held by Dr. Hunter. It was, that if there existed an obstruction to the transmission of blood through the pulmonary artery into the ductus arteriosus at the early period of fœtal life when the septum ventriculorum was incomplete, the blood would necessarily pass through the aperture into the left ventricle, and the same cause would make the septum in its growth deviate to the left, and so the aorta would communicate with the right ventricle. Drs. Carl Heine and Halbertsma, however, support a different theory—a modification of Meckel's—as to the relation of these conditions. Meckel supposed that the incompleteness and misplacement of the septum are primitive defects, and that as the blood found ready exit through the aorta, the pulmonary artery became more or less abortive, and the orifice contracted. The latter view is supported by the position of the aorta at an early period, when it is more to the right than in the mature fœtus; and it also affords an explanation of those cases in which the aorta as well as the pulmonary artery arise wholly from the right ventricle, and another class of cases in which, without any valvular defect, the pulmonary artery is abnormally small. On the other hand, it throws no light upon the diseased condition of the pulmonary valves which is so frequently observed, and it leaves the nature of the original defect unexplained. Dr. Peacock, therefore, is still disposed to regard the obstruction at the pulmonary orifice and the deficiency of the septum as cause and effect, and he ascribes the malposition of the aorta "partly to arrest of development, by which the earlier position of the vessel is retained, and partly to the obstruction to the flow of blood through the pulmonary artery and consequent distension of the right ventricle, causing the septum to deviate to the left."

An entirely impervious condition of the pulmonary artery is a more aggravated form of this kind of malformation. The obliteration may be caused by the complete union of the valves, or a portion of the artery may be obliterated and converted into an impervious cord. Such obliteration of the pulmonary orifice may take place at different periods of fœtal life; either before the completion of the septum ventriculorum—in which case the septum remains incomplete, the aorta communicating with the

right ventricle, and this is the case in the majority of instances—or at a later period, when the partition is fully formed, and the aorta has its origin wholly from the left. If the obliteration occur at the earlier period, the right ventricle becomes large and powerful, and the aorta derives its chief supply of blood from it, the left cavities becoming at the same time disproportionately small. If, on the other hand, the septum be fully formed, the right ventricle and the right auriculo-ventricular aperture become diminished, whilst the left cavities take on increased size and strength. If the septum be incomplete, the foramen ovale may or may not be closed: if, however, the septum is entire, the foramen ovale is necessarily to some extent pervious. Generally, when the pulmonary artery is obliterated, the blood is transmitted to the lungs from the aorta through the ductus arteriosus. In a minority of cases the ductus arteriosus is also closed. In one case described by Dr. Chevers, the pulmonary artery is supposed to have received its blood from the left subclavian; in another case the supply furnished to the lungs through the ductus arteriosus was supplemented by other vessels arising from the aorta. In these two cases the patients survived to the ages of nine and ten. Faulty development of the branchial arches at an early period of fœtal life must be the direct cause of obliteration of the pulmonary artery and the ductus arteriosus.

Another group of cases are those in which there is constriction at the commencement of the infundibular portion of the right ventricle. The infundibular portion may be separated from the sinus by undue development of the ordinary muscular band to which the cords of the tricuspid valve are attached, or by thickening of the endocardium and subjacent fibrous tissue. This condition probably affords the true explanation of the majority of cases of so-called supernumerary ventricle. The heart really resembles that of the turtle, in which there are three imperfectly separated ventricles, a right and left systemic and a small pulmonic ventricle in front, giving origin to the pulmonary artery. The right systemic ventricle of the turtle is the analogue of the sinus of the right ventricle in man, and the pulmonic ventricle represents its infundibular portion. The malformation here referred to, as was pointed out by Mr. Grainger, is to be ascribed to irregular development of the two portions of which the ventricle originally consists, by which their separation becomes much more decided than it should be. The defect may be associated or otherwise with an incomplete condition of the septum, according to the period of fœtal life at which the defect occurs.

Other forms of malformation are found in defect of the inter-

ventricular septum, in consequence of disease of the tricuspid aperture, and of the aortic and mitral orifices; or there may be complete obliteration or atresia of these orifices. Lastly, as a rare anomaly with defect in the septum, the pulmonary artery may arise from the left ventricle. Associated with this there may be an aorta arising from both ventricles, or from the left only. A case reported to the Pathological Society by Dr. Dickinson might at first sight be considered an instance of the latter, although it was really an instance of complete transposition of the auricles and ventricles.

"The subject of the defect was a male child, who died at the age of three and a half years, and had been cyanotic from birth. The heart was situated on the right side instead of the left, and the aorta made its turn from left to right, giving off the innominate trunk on the left side, and the separate carotid and subclavian arteries on the right. The auricles and ventricles were transposed; the pulmonary auricle opened into a small cavity, the analogue of the left ventricle; and this again communicated with a large cavity corresponding to the right ventricle, though placed on the left side, from which both the aorta and pulmonary artery arose. There was also a very marked constriction between the sinus and infundibular portion of the right ventricle. The sinus was large, and gave origin to the aorta; while the infundibular portion was reduced to a very small cavity, situated immediately anterior to the origin of the pulmonary artery." —Pp. 97, 98.

Such cases of complex deviation can only be accounted for by assuming arrest of development at an early period, together with irregular evolution of the aorta and pulmonary artery from the single arterial trunk and the branchial arches.

Dr. Peacock's third primary group of malformations includes those which are produced during the later periods of fœtal life. These he divides into "defects preventing the heart undergoing the changes which should ensue after birth," and "defects which do not interfere with the functions of the heart at the time of birth, but may lay the foundations of disease in after-life." Under the former division, we have premature closure of the foramen ovale and premature obliteration of the ductus arteriosus, and permanent patency of these passages. Numerous instances of these defects are given by the author. Our limits, however, will only permit us to notice his views on the mode of closure of the foramen ovale. The ordinary theory is that the closure of this opening is the result of the floating up of the valve above its margin, and the retention of the fold in apposition with the isthmus by the pressure of the blood entering the left auricle after the establishment of the pulmonary circulation. Permanent patency of the orifice has, on this theory, been

ascribed to undue distension of the right auricle, produced by impediment to the flow of blood from that cavity, causing the valve to be pushed away from the septum and preventing adhesion. Dr. Peacock, however, asserts that an examination of the hearts of young children at different periods after birth will prove that the obliteration of the passage is effected—

"1st. By the shortening of the cornua, and the drawing up of the fold of the valve considerably above the edge of the foramen.

"2ndly. By the approximation of the cornua, so that the width of the upper edge of the valve is greatly reduced. And,

"3rdly. By the diminution of the opening itself."—P. 108.

This process, he believes, takes place through the agency of muscular fibres which, a few weeks or months after birth, can readily be traced proceeding from the fasciculi which form the walls of the left auricle in the course of the cornua of the valve, to be expanded in the curtain. These, by their contraction, draw up the curtain so as to cause it to overlap the edges of the opening and contract the cornua. He believes, therefore, that the closure is an active rather than a passive process, and that the muscular fibres spoken of contract partly in consequence of the traction exercised upon them by the expansion of the cavity of the left auricle, and partly by the more stimulating quality of the blood which reaches the structure. A faulty supply of blood to the left auricle interfering with its due expansion, or a deficiently aerated blood, might therefore, in the author's opinion, be the causes of permanent patency. He allows that the continuance of undue pressure on the right side of the valve may be the cause of non-adhesion of the valve to the margin of the opening, and the production of the oblique passages which not unfrequently exist. But he asserts, with truth, that the ordinary theory does not account for the closure of the valve which is sometimes found in cases in which there must always have been greater pressure on the right than on the left side.

Cases of congenital irregularity of the valves, although not interfering with the functions of the heart at time of birth, not unfrequently lay the foundation of disease in after-life. Such irregularities resolve themselves into malformations, and excess and deficiency in number of segments. Under this head the author offers an explanation of the existence of excess of the segments of the semilunar valve, founded upon the analogy of the structure of the valves in the bulbus arteriosus in the American Devil Fish (*Cephalopterus giorna*) and in other cartilaginous fishes. In a preparation of the heart of the former to be seen in the museum of the Royal College of Surgeons, it

will be found that six imperfect valves which exist in one portion of the bulbus arteriosus at its termination appear as three well-formed semilunar valves. If it be true, and from this analogy it would appear not improbable, that each curtain of the semilunar valve in man originally consists of two portions, redundancy of segments would result from arrest of growth, and thus fall into the same category with other malformations.

The last of the author's primary groups is composed of cases of irregularity of the great vessels. These he treats of under the heads of—1st, Transposition of the aorta and pulmonary artery; 2nd, Descending aorta given off from the pulmonary artery; 3rd, Deviations from the natural arrangements of the venous trunks; and, 4th, Irregularities of the coronary arteries and veins. The first-named of these conditions has been already alluded to in connection with other malformations, and it is most frequently found coexisting with defective septum ventriculorum. This is not, however, invariably the case. In an example submitted to the Pathological Society by Dr. Ogier Ward, the septum was entire, but both the foramen and duct were pervious.

The mode of formation, symptoms, effects, and medical management of this class of affections are treated of in a separate chapter. With regard to the mode of formation, setting aside those cases in which, from the early condition at which arrest of development takes place, it is impossible to detect the cause which has given rise to it; in a large remaining class we are able to trace the conditions which necessarily lead to malformation. Thus, if, at a period when the septum is fully formed, disease and consequent contraction of the pulmonary orifice ensue, patency of the foramen ovale will be the necessary result; and if obstruction take place before the completion of the septum, that structure will remain incomplete, and, as already shown, the position of the aorta may be affected thereby. Irregular division of the arterial trunk and evolution of the primitive arterial arches will explain, as far as they are explicable, transposition of the aorta and pulmonary artery, and other irregularities in the great vessels.

The symptoms and effects of malformation on the duration of life are subjects of interest to the practitioner. How little can be done to alleviate many of these cases, is but too evident from the fact that the author sums up the medical management in two short pages. Under the head of symptoms, the cause of cyanosis is discussed at considerable length. Dr. Peacock adopts the opinion advanced by Louis, Valleix, and Stillé, that cyanosis is due to venous stasis, in opposition to the view of Dr. Hunter that the condition depends on the admixture of the

venous with the arterial current, the latter being that which has obtained most advocacy in this country. That Dr. Hunter's explanation is not universally applicable, appears to us proved by the fact that cyanosis has not always been observed even when, from the existence of great imperfection, there can be no doubt that a free admixture of the currents of blood must have existed from the time of birth. Recent disease of the heart and lungs will often lead to the development of cyanosis in cases of this kind, and the post-mortem examination alone reveals the existence of the congenital defect.

In taking leave of this work, we would only add that the author has enriched it with the details of no less than eighteen original cases, and, in addition, has given analyses of or references to all the principal examples of the abnormal conditions of which he has treated. The present edition contains also eight admirable lithographic plates. We congratulate Dr. Peacock on the execution of his very valuable work. It is a performance which reflects credit on the author, and through him on the Society of which he has lately been president.

REVIEW III.

On Joint Diseases; their Pathology, Diagnosis, and Treatment; including the Nature and Treatment of Deformities and Curvatures of the Spine. By HOLMES COOTE, F.R.C.S., Surgeon to St. Bartholomew's Hospital, Consulting Surgeon to the National Orthopædic Hospital, &c. London: Hardwicke. 1867. 8vo, pp. 299.

THE study of joint diseases has occupied the attention of many of our most distinguished surgeons; and if we recall the names of those who have contributed to our knowledge of this subject, we shall find that they include some of the brightest ornaments of modern surgery. Indeed, this could hardly fail to be the case; for diseases of the joints are of such frequent occurrence, they are so serious in their results, and their pathology presents so many points of interest, that it would have been strange if they had not attracted a large share of professional attention. The joints of the human body are so numerous, they are so constantly in action, and their mechanism is so delicate and complicated, that it is not wonderful that they should be liable to a great variety of mischances. But in this country, where the different forms of scrofula are so common, the

diseases of joints present a still higher degree of importance; for it is hardly too much to say that of the many cases of joint disease which come under the notice of English surgeons, a majority have their origin in that state of the constitution which, for want of more accurate knowledge, we call the strumous diathesis. Hence it has happened that the attention of English surgeons has been directed in an especial manner to the subject of articular diseases. Many and various are the opinions which have been held, and the plans of treatment which have been advocated. While some have recommended the least possible amount of interference, others have boasted of the benefits to be derived from capital operations; and it seems that we are only now approaching the time when we shall be in a position to adjudicate upon the merits of these rival systems.

The present is an opportune moment for the publication of a work upon joint diseases. Any surgeon who, like Mr. Holmes Coote, can look back over a period of twenty-five years spent in practice, must have heard several totally distinct plans of treatment discussed, and must have seen different methods of practice tried. He must have had opportunities of contrasting the depletory and the tonic systems; he must have seen what can be done by simple rest, what by mechanical appliances, and what by operations; and if he can give us a sound opinion upon these various plans of treatment, and can help us to arrive at a just estimate of their respective value, he will have done something to advance the cause of scientific surgery in one of its most important branches.

Now, we think Mr. Coote may fairly claim to have done this, and we look upon his work as a valuable contribution to our modern professional literature. Besides being surgeon to one of our largest metropolitan hospitals, he has had special opportunities of studying the diseases of joints at the National Orthopædic Hospital, and he has given us an excellent *résumé* of his experience in the volume before us. The excision of diseased joints is a practice which has been on its trial for a sufficient length of time to allow us to form an estimate of its value; and assuredly the advocates of such operations cannot complain that they have not had a fair hearing. But while some surgeons have been testing the value of excisions, others have been trying what can be done, even in the most severe cases, by rest and the use of mechanical appliances; and our author, from the long experience which he has had both at a general hospital and also at one specially devoted to the treatment of deformities, has had unusual opportunities of forming an opinion upon the merits of these two plans of treatment.

It is, therefore, with no small interest that we have read Mr.

Holmes Coote's book; for we cannot help thinking that it serves to show in what direction professional opinion is moving in regard to the treatment of joint diseases, and what is likely to be the prevailing practice for some time to come. In his preface our author says—"I have viewed with regret a somewhat growing inclination for the early performance of operations, particularly resections; and, much as I admire the spirit of conservatism with which that proceeding is characterised, as opposed to amputation, yet must confess a misgiving as to its general applicability, and feel a desire that the advantages to be derived from mechanical treatment and the enforcement of complete rest should be tried more continuously and with yet greater patience." This opinion is one which we are inclined to think has been gaining ground rapidly of late, and which will probably be endorsed by a large number of his professional brethren.

In the very outset of his work, when dealing with the classification of joint diseases, our author makes some remarks which give us an insight into the large and philosophical view that he takes of his subject:

> "In most works on 'diseases of joints,' we read of inflammations, whether acute, subacute, or chronic, as occurring in the synovial membrane, cartilage, bone, or ligament; and this comprises the whole, or nearly so, of the pathological conditions. With this classification I find fault. In the first place, it omits all mention of deformities, whether congenital or acquired; it does not touch upon changes dependent on errors of nutrition; and it assumes the existence of inflammation in some tissues, such as cartilage, where such disease never occurs; or in others, such as ligament, where its presence is only secondary, and dependent on inflammation spreading from other sources."—P. 3.

Here, then, we see that Mr. Holmes Coote is determined to depart from the beaten track of previous observers, and that he is led to do so in conformity with the most advanced views of modern pathology. A short time ago, all the diseases of joints were held to be of an inflammatory character. They were all supposed to have their origin in inflammation affecting one tissue or another. But now we recognise various morbid alterations which are not preceded by a state of inflammation; and we believe that such conclusions are the result of that searching and discriminating investigation which characterises the pathology of the present day.

Our author includes, too, in his treatise, the deformities as well as the diseases of the joints. We are glad to see that such is the case; for there is no sufficient reason why a work upon

the diseases of the joints should deal with their acute conditions only, and stop short of their ultimate results, which are often just as difficult and troublesome to treat. Indeed, the chronic diseases of joints are so tedious, that the general hospitals can seldom afford to admit them, and hence they have almost become a special branch of practice. But there is no sufficient reason why this should be the case. It is in the highest degree desirable that all students should see the lamentable results which, after the lapse of a few years, frequently follow the acute diseases of joints, and that all surgeons should be able to treat these chronic affections. We are glad, therefore, to find that our author has included within the scope of his work both classes of joint diseases—both the acute which are treated at our general hospitals, and also the chronic ones, which have hitherto, in too many cases, been handed over to the department of orthopædic surgery.

Mr. Holmes Coote very justly points out that the rules which guide our practice in public institutions ought not to be different from those which we follow in our private practice. "Surgeons in public practice should not despond of success, nor fly at an early date to so serious a measure as amputation or the resection of a joint, because the parts do not speedily become sound. We dare not act so in private practice." (P. 6.) And then he goes on to say, "One object of this work is to call attention to the natural processes of repair, to point out how much may be done, even in the most unpromising cases, by combining mechanical with general treatment, and the strict enforcement of rest." (P. 6.) These are principles which are coëxtensive with the field of surgery; they do not belong to any one department, and there is no reason why surgeons should not avail themselves more generally of the various appliances which experience has shown to be useful in these cases.

The author's remarks upon simple atrophy and hypertrophy of cartilage are not without their interest. The non-inflammatory morbid conditions of joints, the changes which are dependent upon alterations of nutrition, are subjects which have only recently been investigated, and of which our knowledge is still far from being complete. The paragraphs upon this subject in the volume before us will be read with interest and profit. Passing on from the simple nutritive changes which take place in cartilage, Mr. Holmes Coote proceeds to discuss at some length the pathology of the so-called ulceration of that structure; and he shows that the treatment by rest and gentle measures is that which is most in conformity with the true nature of the case. The practice of forcibly setting a limb in its right position is one which our author condemns in strong terms, and

he points out how seldom it can be employed with advantage. Certainly, if we reflect upon the anatomy of the joints, and consider that they fall into the flexed position as being that which gives the patient the most ease and comfort, we cannot expect to alter their direction without inflicting severe injury upon parts which are often in a highly sensitive state, if not in a condition of active disease. We believe, as a general rule, that rough and forcible methods of treatment are almost always unscientific, because they are opposed to those slow and gradual processes by which nature effects alterations, and, in many cases, completes cures.

On the vexed question of excision of the knee-joint, the opinion of such a man as Mr. Holmes Coote can hardly fail to be interesting to our readers. "I am no strong advocate," he says, "for the operation of resection of the knee-joint under any circumstances, and disapprove of it almost unconditionally in children, in whom the limb afterwards becomes year by year weaker, more withered, and less equal to the opposite member." Our author then proceeds to give the history of a case which has fallen under his notice, and perhaps our readers will pardon us if we quote it at length. The interest of the subject must be our excuse if it seems rather a long extract.

"On February 14th, 1863, one of my colleagues amputated, at St. Bartholomew's Hospital, the left thigh of a boy, æt. 12, who had undergone the operation of excision of the knee-joint in another hospital, in consequence of scrofulous disease of the articulation, about nine months previously. The condition of the boy before the second operation was as follows:—He was not much emaciated, nor had his face lost all trace of the usual florid colour. The left lower extremity was an inch and a half shorter than the opposite; the thigh was hot and swelled; sinuses led from the incisions about the knee to dead or denuded bone. There was some mobility at the knee, the limb was utterly useless, and the opinion was general that the sooner it was removed by amputation the better. Upon making the usual incisions, it was found that the tissues were much infiltrated, and some considerable sinuses extended upwards among the muscles of the thigh.

"The hæmorrhage was troublesome, in consequence of the generally increased vascularity of the limb. The examination of the amputated parts, including the seat of the resection, was full of interest. In the first place, very little of the bony surfaces of the tibia and femur had been cut away at the time of the resection; their usual outline remained: therefore, although the epiphyses had been left, arrest of development was still a consequence of the operations. Secondly, the greater part of the synovial membrane was left behind in a thickened, discoloured, and pulpy state, keeping up the effects of disease in full activity. Thirdly, there were numerous scrofulous

abscesses in the popliteal space. Fourthly, there were two or more points where the surface of the tibia was in a state of carious ulceration or necrosis. Fifthly, the union between the tibia and femur was incomplete, being partly fibrous and partly osseous; but the bone was dark-coloured, soft, soddened and spongy, and unsuited to effect firm union. Sixthly, the opposed surfaces were not in proper rectilinear apposition."

"Could any result have been less satisfactory? But, it may be said, such cases are exceptional. I am not prepared to acquiesce in that statement. In December, 1862, another surgeon amputated the left lower extremity of an infant on whom excision of the knee-joint had been performed a year before in another hospital: and a third case occurred in my own practice, in which the parents declined any further operation, although the limb was short, withered and useless, and the child could walk only with the aid of crutches."—P. 139.

On the subject of spinal curvature, Mr. Holmes Coote's opinion cannot fail to have weight from the large experience which he has had at the National Orthopædic Hospital, and his remarks upon the early and incipient stage of this disease will be read with interest. It will be seen that he condemns the present methods of education, especially among girls, and that he lays great stress upon the value of free outdoor exercise and plenty of it. This is a point which can hardly be too often or too strongly enforced at the present day: for as knowledge increases, and branches of learning are multiplied, it becomes more and more necessary to protect the young against overstudy, and to claim for them a fair share of healthy and invigorating exercise in the open air.

"We regard a state of general debility as the predisposing cause to lateral curvature of the spine. The exciting causes are numerous: the repeated maintenance of one constrained position for many hours in the day, as in playing the harp, drawing, or writing, particularly if the patient be shortsighted and obliged to bend the head lower than usual in order to read print; standing on one leg; the forced position of many industrial pursuits, such as dressmaking, lace-manufacturing. In patients affected with rickets, the condition of the bones is such that they yield readily in any direction.

"The training of growing children is often inconsiderately carried out. School-girls should not rise at 6 a.m., and have two hours' lessons before breakfast; the mind is not in a fit state to profit by such exercises, and the bodily trial is frequently shown by faintings which no effort can control. The time allowed for meals and outdoor exercise is insufficient, amounting to about two or three hours in the whole; while the two large items of nine hours each in the twenty-four are given up respectively to tuition and to sleep. The decorous walk along the high road gives none of that invigorating

freshness which the active games of boys induce, and hence in many instances an amount of listlessness supervenes; the waste of material being slight, the appetite becomes small and capricious, and nutrition is not well carried out. It is in such a state of body as this that the curvature of the spine is apt to commence."—P. 172.

In speaking of the so-called hysterical affections of joints, our author makes some remarks which we are inclined to think that most readers will entirely agree with.

We shall conclude our notice of this interesting book by extracting a case which is narrated by Mr. Holmes Coote. It will serve to show that, although the whole tendency of his work is in favour of mild and gradual treatment, yet, when the circumstances clearly warrant it, he is not unwilling to adopt more forcible measures. And yet this very case goes to prove the soundness of his general practice; for, although the extension was successfully made, and seemed to be justified by the immediate result which followed it, yet in the end it appeared that it must have been attended with serious injuries which brought about the death of the patient.

"In 1862, a young girl, æt. 18, was under my care in St. Bartholomew's Hospital, suffering from talipes equino-varus of both feet, and general spasmodic contraction of the knees and hips, following an extensive burn on the buttocks, received in infancy, by which the equilibrium of the nerve force to the lower limbs had been disturbed. While an infant, she submitted without a murmur, under her mother's care, to her calamity; but when the menstrual function commenced, the annoyance proceeding from the fixed, contracted condition of the thighs became intolerable. There were difficulties in the way of obtaining an expensive apparatus, and the girl herself was morbidly anxious for immediate relief. Accordingly, I directed that she should be rendered insensible by chloroform, and proceeded to forcible extension. The resistance readily yielded; the limbs became straight, and it was found that the articulations of the hip and knees were normal. An opiate was given, and for some days all went on favorably. Within a week, however, swelling of the lower extremities supervened; patches of erysipelatous inflammation spread from part to part, and the patient died comatose. Although no examination of the body was allowed, I believe that in the extension some important veins were torn."—P. 201.

This notice has already extended to sufficient length, and yet we feel that we have done scanty justice to Mr. Holmes Coote and his treatise. It well deserves a careful perusal, and it will be found a most interesting and instructive volume. Accurate and discriminating diagnosis, combined with early and scientific treatment by means of rest and mechanical appliances—

these are the principles which our author endeavours to enforce. And, assuredly, if they were better understood and more generally carried out, they would go far to limit the number of operations required for joint diseases, and so far remove one of the opprobria which still attach to modern surgery.

Review IV.

1. *Notes for a History of Sanitary Legislation.* By Edwin Lankester, M.D., F.R.S. 1867. Pp. 15.
2. *Report on the Sanitary Condition of the City of London for the Year* 1865-1866. By H. Letheby, M.B., M.A., Ph.D., &c., Medical Officer of Health for the City of London. 1867. Pp. 56.
3. *Special Reports by Mr. Orton, Medical Officer of Health, upon the Cholera Epidemic of* 1866, *to the Limehouse Board of Works.* 1866. Pp. 16.
4. *A Report on the Water Supply of Oxford, presented to the Local Board, October* 23, 1866. By Gilbert W. Child, M.D., of Exeter College, Officer of Health to the Board. 1867. Pp. 18.
5. *Remarks on Legislative Measures now in progress concerning Dwellings for the Labouring Classes.* By H. W. Rumsey, F.R.C.S., &c. London, 1866. Pp. 14.
6. *On the Vital Statistics of Birmingham and Seven other large Towns.* By William Lucas Sargant. 1866. Pp. 24.
7. *Our Sewer Rivers.* By George Greaves, M.R.C.S. 1866. Pp. 24.
8. *Matter—its Ministry to Life in Health and Disease; and Earth, as the Natural Link between Organic and Inorganic Matter.* By Thomas Hawksley, M.D. Lond., M.R.C.P., &c. London, 1867. Pp. 72.

In relation to sanitary science, a new era seems to be opening. Never has there been a time when the public attention has been more directed to all that concerns the health of the people, and never has the Government exerted itself more to meet the

existing wants. Nor is this surprising, taking into account the circumstances which have led to the change from previous apathy and neglect to watchfulness and active exertion such as we now witness. The most important of these, probably, have been the severe epidemics by which of late years the country has been visited, exciting universal alarm; and, hardly second to them, the inquiring spirit of the times, the advances made in physical sciences, with the consequent tendency to refer such visitations to natural causes, most of them controllable, and the smaller number, if not controllable, at least mitigable by the observance of the laws of health. The pamphlets which form the above heading afford corroboration of what we have just stated. We shall notice each briefly, commencing with Dr. Lankester's.

In these 'Notes' we have an interesting and valuable contribution towards a history of sanitary legislation, showing how, from the earliest historical period, a necessity had been felt and responded to for enacting rules for the prevention of disease and the preservation of health; and how, in different ages, in the progress of time, and in different countries, they have been more or less insisted on,—strongest and minute as laid down in the Levitical law, especially addressed to personal habits and articles of diet,—less precise and severe amongst peoples living under more free and less exacting codes, such as the Greeks and Romans,—with them chiefly directed to two great objects, water-supply and sewage; and later, after the fall of the Roman Empire, in the mediæval times, a total abnegation of all rules of the kind, or little but residuary traces of Roman sanitary legislation. With the revival of letters and the improvement of the social condition of Europe, the subject of public health again received attention, and in a degree very much in proportion to the enlightenment of the several nations; first in Italy, next in Germany and France, and latest, in a systematic manner, in England. In a partial way, however, it would appear that our country was not altogether backward, as, so long as 800 years ago, in the time of Richard II, an ordinance was enacted prohibitory of the pollution of rivers, drains, &c.; another in the reign of Edward II against selling "muzzled swine-flesh," &c.; and in the reigns of Henry VI, Henry VII, and of Elizabeth, for the inspection and cleansing of sewers, against the slaughtering of cattle in towns, and against the overcrowding of dwellings. For particulars, and especially as showing the slow and desultory manner in which sanitary legislation has advanced in our land of freedom, we must refer our readers to Dr. Lankester's 'Notes,' which are brought up to the present time. We need hardly remark, it is so well under-

stood, that with the exception of what we owe to a few distinguished individuals impelled by humanity and directed by science, its progress would have been slow indeed, had it not been expedited by the occurrence of epidemics, creating alarm and too often panic, especially plague and cholera. The influence of the last-mentioned disease is most remarkable in the effect which it has had on our official authorities in rousing them to exertion. Since its first outbreak, Act after Act of a sanitary kind has followed each other in rapid succession; and it is satisfactory to find that where they have been enforced, the results have been in a marked manner beneficial.

Of this we have satisfactory proof in Dr. Letheby's ably drawn up 'Report on the Sanitary Condition of the City of London,' for the past year. Two results, as pointed out by him, may be mentioned: one, the reduction of the death-rate during the last ten years to the extent of about 11 per cent.; the other, the low aggregate in the City, 22·2—much less than the common death-rate of the Metropolis and the large towns of England, viz. 24·3. It stands, indeed, exactly at the average proportion (22·2) for the whole of England for the last ten years. We quote the following table, showing the proportional death-rate in the several districts for the past year compared with the last ten years.

Death-rate per 1000 *of the population.*

	1866.	Average of 10 Years.
East London Union	23·8	26·4
West London Union	26·8	28·2
City of London Union	18·2	21·3
Entire City	22·2	24·8
All London	26·2	24
Districts of Chief Towns	26·7	24·3
Country Districts	20·3	19·9
All England	23·4	22·2

Another proof is afforded in the diminished mortality from alvine disorders at each successive visitation of cholera. "In 1848, for example, when your sanitary powers were enlarged, cholera was among us, and at that visitation the mortality in the City from alvine disorders was at the rate of 219·2 per 1000 of the deaths, or 63·5 per 10,000 of the population. Six years afterwards, in 1854, there was another visitation of the epidemic; but the sanitary condition of the City had been so improved, that the mortality from alvine disorders was reduced to 69·8 per 1000 of the deaths, or 19·7 per 10,000 of the living. And now, at the close of 1866, when we have to record the

results of another visitation, we find that the mortality has been only at the rate of 42·8 per 1000 of the deaths, or 9·5 per 10,000 of the population." It is further shown, as stated by Dr. Letheby, "in the low death-rate from cholera in the City during the past year as compared with that of other European cities which the epidemic visited. Here the mortality from the disease has not exceeded 7 per 10,000 of the population; whereas, in all London it has been at the rate of 18 per 10,000, in Paris 39, in Amsterdam 42, in Naples and in Vienna 51, and in Brussels 163."

So far as seasons influence mortality, it is noteworthy, as pointed out by him, that the highest rate is in the winter months, when the inclemency of the weather takes effect on the aged and feeble; whilst in former times it was greatest in the warmer months; "the higher temperature," as remarked by Mr. Simon in 1853, "acting in some sort as a test of defective sanitary conditions, and giving to the several local causes of endemic diseases an augmentation of activity and virulence." The same, we may add, is witnessed in the United States of America, especially the Southern.

The following remark as to meteorology we quote as worthy of attention:—"On comparing the meteorology of the year 1866 with that of the cholera years 1849 and 1854, very striking differences are perceived. In the year which has just expired, the average temperature throughout the country has been rather low, and the barometric pressure has been less than usual, while the quantity of rain and the force of the wind have been excessive. In 1849 and 1854 the phenomena were nearly the opposite of these; for the weather was generally calm, the rain-fall less than the average, and the temperature and barometrical pressure ran unusually high. As far, therefore, as these phenomena are concerned, we are still unable to perceive that there are any special atmospheric conditions peculiar to the epidemic."

It may be right now to notice the work done, the exertions progressively made, to which the improved sanitary condition of the City may be attributed. The following table, showing the number of houses inspected and orders issued for sanitary improvements in the City in each of the last eleven years, is well adapted to answer the purpose:

Years.	Homes Inspected.	Orders Issued.
1856	5,401	1,215
1857	5,924	2,031
1858	7,786	1,721
1859	9,587	1,984
1860	9,448	2,472
1861	9,425	2,518
1862	8,693	2,455
1863	9,089	2,443
1864	10,700	3,179
1865	11,008	2,331
1866	12,213	3,068
Yearly average	9,025	2,311

Dr. Letheby thus describes the sanitary work of the year:—"I have to report to you that there have been 12,213 inspections of houses, of which 3271 have been inspections of the registered common lodging-houses. These have resulted in the issuing of 3068 orders for sanitary improvement, comprising 4225 separate items, of which 2080 were for the cleansing and lime-whiting the interior of houses, 845 for cleasing and repairing closets, 588 for amending the water-supply and cleansing butts and cisterns, 426 for repairing and trapping sinks and drains, 206 for removing dust and other refuse, 12 for abolishing ashpits, and 12 for abating smoke nuisances." It is very desirable that the full meaning of the term "nuisance" should be clearly understood. Dr. Letheby remarks—"Until recently, the nuisance in all sanitary acts had a very limited signification. It was merely applied to such a state of premises, ditches, gutters, watercourses, privies, urinals, cesspools, drains, or ashpits, or to any animal so kept as to be in either case a nuisance or injurious to health; but now it includes any house or part of a house so overcrowded as to be dangerous or prejudicial to the health of the inmates; any factory, workshop, or workplace not already under the operation of any general Act for the regulation of factories or bakehouses not kept in a cleanly condition, or not ventilated in such a manner as to render harmless, as far as practicable, any gases, vapours, dust, or other impurities generated in the course of the work carried on therein that are a nuisance, or injurious or dangerous to health, or so overcrowded while work is carried on as to be dangerous or prejudicial to the health of those employed therein; and any fireplace, or furnace, or chimney, not being the chimney of a private dwelling-house, sending forth black smoke in such quantity as to be a nuisance. The Act also provides for the inspection of shipping within the jurisdiction of the nuisance authority—a ship or vessel being regarded in the same manner

as if it were a house within such jurisdiction, and the master or other officer in charge as the occupier."

The same Acts, those of 1855 and 1860, which have so much enlarged the definition of a nuisance, have also considerably added to the power of the local authorities, especially in dealing with lodging-houses and the overcrowding of a house, or improperly occupying a cellar.

Though so much has been done in the City in sanitary reform, more, it would appear, remains to be accomplished, requiring additional means and greater exertions; and even with fresh aids, Dr. Letheby, we are sorry to see, expects only partial success, owing to the deplorable state of the dwellings of the poor, the little public concern there is for their domestic wants, and the degraded state to which they are reduced. It is painful to read the paragraph relating to them. We quote only his concluding words: "Such, indeed, is the perversity and ignorance of the poor in all these matters, that they can hardly be got to use anything in a proper manner which is provided for their cleanliness and comfort;"—adding, "it is high time that some means should be used to improve their habits in this respect, for they add considerably to the difficulties of enforcing sanitary measures, and give a plausible excuse to the passiveness of landlords."

Various instructive statistical tables with a minute meteorological one, are appended to the report, and also a copy of the "Regulations for Houses and Parts of Houses in the City of London which are let in lodgings or occupied by more than one family."

The two reports, the titles of which follow Dr. Letheby's, we owe to cholera. Dr. Child's contains some good observations on water, and the impurities, the contaminations to which it is liable, and on the modes of detecting them. He shows in a satisfactory manner that, besides organic matter of an oxidizable kind, there may be others present capable of resisting oxidation, and which, therefore, cannot be detected by the ordinary means employed, such as the addition of a permanganate.

As regards the water supply of Oxford, of which his opinion on the whole is of a favorable kind, he lays stress on the danger of the principal source becoming contaminated and poisonous from percolation of sewage from the adjoining buildings; a contamination which, he states, has already taken place in many of the wells of the city, in situations where the pervious bed of gravel, resting on the impervious bed of clay, is of little depth.

Mr. Orton, in his report on the outbreak of cholera which occurred last year, and was so fatal in the eastern districts of

London, considers that it was not owing to water, as had been supposed, and had been believed on the authority of the Registrar-General. The water bearing this charge was that of Old Ford, conveyed through the mains of the East London Company. He considers the origin of the epidemic unknown, its travels equally a mystery, and the attempts vain to trace out the positive causes of its special localisation. The tabular view which he gives of the several districts of the eastern division is interesting. We quote his comment thereon:

"The action of a poison—a water-poison or what not—is uniform over a community; it treats all alike under like conditions. But this Old Ford water acts capriciously: it skips about finding victims; here its devastations are heavy, but there passes by multitudes untouched. Poisons, as a rule, lay low more readily the weak, the poor, the half-starved, and the destitute; but these classes in the cholera epidemic have stood the shock well, they have come foremost out of the fray. It has crushed the scanty population; it has passed by the overcrowded, spared the poor, laid low the rich; terrible in the houses of the wealthier districts, merciful in those of the impoverished; scarcely felt on a hill, while awfully destructive in a valley."

He remarks, whatever dispute there may be about the origin of the epidemic, there can be none about the localities where it was most rife;—streets "either built over slush or filth; or the floorings and joists rest on the earth itself, or have but a very few inches above it .... places *where always has come fever,* just as they have lately been, in *turn,* the fatal resorts of cholera." The Building Act, he well insists, "is at fault for all this;" and he gives instances in proof.

Putting aside the water theory, he adopts the atmospheric, which, with special localisation as an aiding cause, he holds to be proved by a chain of consistent and co-operating facts. He says, "The water-*drinking* poison has been mistaken for the water-*inhaling* poison." We quote the following paragraph in illustration and for the facts therein stated.

"I have already expressed an opinion that the great local nuisances have probably had their part much more than the water in the production of cholera. The Lea Cut and Regent's Canal were intolerable during the hot weather of June, when on one day the thermometer was 165° in the sun. Then look at the Bow Creek, into which was pumping all this time the sewage of 70,000 inhabitants, including Stratford, West Ham, and all about Victoria Dock. And am I to be told that such abominations have had nothing to do in bringing about the epidemic? Then add to the list the numerous factories on Bow Common, and more or less throughout the eastern district; the comparatively stagnant water in

the docks, as well as that charged with organic matter in and about the poorer dwellings at a low elevation, in some cases only two feet above, and vast numbers many feet actually below, Trinity high-water mark; and there is to be found an aggregate of filth, I believe I am not wrong in saying, not to be equalled by all the rest of the metropolis. At this time, the Thames at Greenwich was 68°; so that from a large tidal stream to stagnant water like the canals, it was probably here at 80° of heat. It is a fact that organic matter in water at 60° undergoes a fermentation; poisonous gases are thrown off, and deadly vapours; so that it would require slight calculation to estimate the influence of heat on these masses of liquid filth, continuing for days together, and then conveyed exactly in a line with the cholera field. The records of medical science afford plenty of cases in illustration. The outbreak of choleraic diarrhœa, attended with many deaths, at the Carlisle Lunatic Asylum, is a case in point. Here the sewage of this institution had been thrown over a heavy, clayey land in the neighbourhood, and a favouring wind did the mischief. It was found when the wind was away from the asylum the disease was at once relieved; and then, again, when in an opposite direction, disease and death increased."

The conclusions Mr. Orton has arrived at we think it right to quote, believing, as we do, that they are deserving of all consideration, the evils the recommendations are proposed to correct being so great and fatal in their effects.

"1. That all rivers, docks, canals, and other deposits of water, should be kept as rigidly free as possible from pollution; and all factories, yards, and works of every description engaged in operations on animal matter, or giving rise to offensive effluvia, should be at once abolished, as standing perils, in certain atmospheric conditions, to the health of the localities.

"2. That the Building Act calls for revision, under which there is a demand for a clause by which no new house should be held as habitable unless certified by a sanitary authority.

"3. Wholly demolish certain streets, courts, and alleys, as incapable of sanitary repair and unfit for habitation.

"4. In houses of another class, but with defective structure, already pointed out, make it imperative on the owner to place a layer of concrete under the floor (a perfectly practicable thing), or other impermeable composition, to secure the inmates from pestilential and other dangerous exhalations of the earth."

Mr. Rumsey's paper, which we have next to notice, is deserving of careful perusal, especially by those who are anywise concerned in exercising control in the enactments of legislative measures. His remarks, which have for their object the improvement of the dwellings of the labouring class, generally in their wretched condition so much needing improvement, are well arranged under five heads. First, to secure salubrity, that

the site of new buildings should be carefully selected. Secondly, with the same intent, that crowding, the undue aggregation of inhabitants in a limited area, should be strictly avoided: he lays it down as a rule, that each person should be allowed 48 square yards, or 100 to the acre; and that lofty blocks—the piling-up of living animals layer upon layer, thereby tending to vitiate and to obstruct the free circulation of air—should be prohibited; and that in all new arrangements, suburban districts, as much as possible, should be selected, rather than old sites in already crowded parts of towns. Thirdly, that plans should be prepared and building regulations enacted—a code, in brief, for the whole of Great Britain, or a General Building Act, adequate to secure the erection of dwellings of proper materials and of wholesome construction, under the direction of proper authorities. Fourthly, as regards jurisdiction, that it would be better to give power to boards of guardians, at least in the country or to county magistrates, than to local boards, for the administration of any Act authorising the building of dwellings for the poor. Lastly, as regards the department of Government in which powers should lodge to initiate and control proceedings under any proposed Act, that a committee of Privy Council on public health, aided by the advice and experience of the medical officer of that council, would be likely to be more efficient than the Home Office, already overburdened with duties.

Mr. Rumsey does more than offer suggestions: what he proposes he supports by facts, the correctness of which cannot be questioned; and he answers also objections, most of them made from limited views and an imperfect knowledge of sanitary science.

The next pamphlet, Mr. Sargant's, is entirely statistical. The tables, which constitute its principal part, are formed on data chiefly derived from official returns and the Registrar-General's reports. The seven large towns in addition to Birmingham, of which the "vital statistics" are given, are Sheffield, Wolverhampton, Manchester, Leeds, Liverpool, London, Bristol.

The only table that is suitable for quotation, and our limits permit us to extract, is that showing the yearly death-rate per 1000 (the average of ten years) of the inhabitants of the several towns, keeping apart the boroughs and registration districts.

Ten Years' Death-rate.	Liver-pool.	Man-chester.	Salford.	Leeds.	Bir-mingham.	Bristol.	Hull.	London.
1. In Registration Districts	33	31½	28½	28	26½	24½	24½	23½
2. In Boroughs	30	29½	26	26	23	24	24	23½

The other tables there appended illustrate many interesting particulars, such as the population of the respective towns since 1801, with the absolute increase and rate of increase in each decennial period; the numbers living at the date of the last census at different ages; the density of the population and death-rates and birth-rates; the death-rates at different ages; the excess of such death-rates over those of the whole country; the numbers who die from phthisis, cholera, and other diseases; lastly, the proportion of deaths from each cause to deaths from all causes.

These contents of tables we thus enumerate for the sake of those of our readers who take an interest in inquiries of the kind; and as Mr. Sargant's paper is published in a well-known and easily accessible journal (that of the Statistical Society of London, March, 1866), they will have little difficulty in referring to it.

We could have wished that the author had dealt more largely than he has done with the hygiene of the subject—the circumstances such as sanitary science regards conducive to health and disease, and as making the difference in the proportional mortality of the several towns: but this, we need hardly remark, is a task beyond the scope of a pamphlet.

From the introduction, containing cursory observations connected with the tables, we shall select a few particulars.

The increase of the population of the great towns during the present century is one of the most remarkable features in their history. It would appear that Norwich in 1752 had a larger population than any of the now greater towns, with the exception of the capital: at that time it had 36,000 souls; whereas, twenty years later, Manchester, including Salford, had 27,000; Birmingham little over 30,000, and Liverpool less than 35,000; and yet, in less than a century, these three towns, including Salford, taken together have from 92,000 increased to a million and a quarter. The general increase, it would appear, has with tolerable regularity kept pace, as might be expected, with the increase of commerce and of our manufacturing industries.

Referring to the "ages at death," the results he arrives at are in some respects different from those commonly accepted. Thus, whilst admitting that there are more male deaths on the whole than female, he states that "in the first five years of life there is a larger excess of male deaths; that from five to ten years of age, the male excess is very small; that from ten to fifteen, the male excess is decided: but that at fifteen the tide turns, and until twenty the female deaths are largely in excess; that from twenty to thirty-five, the male and female deaths are equal; and that after thirty-five, the male deaths are again in excess. He

adds, in a note, the "singular fact" obtained from Mr. Makeham, that among the *males* of the *upper* classes from the age of sixteen to the age of twenty-three, the annual rate of mortality rises rapidly from about eight to a maximum of fifteen per 1000, after which it gradually diminishes." If this be confirmed, it would be a desideratum to ascertain the cause or causes of it.

Of the diseases which he adverts to, we shall name only two for brief remark—cancer and phthisis. The proportional mortality from the first is higher than we could have anticipated, being, as stated, one per cent. of all deaths, and twice as great of females than of males. The latter is what might have been expected, but hardly what follows, viz., that locality, whether in town or country, little affects the rate—that being remarkably uniform; leading to the conclusion that, as regards the etiology of the malady, it has little to do with external circumstances or habits of life.

Of phthisis, it would appear that through the whole country there is an excess of female deaths of one in thirteen, or nearly eight per cent.; but in all towns, with the exception of Wolverhamption, the excess is on the male side: and yet, taking the two sexes together, the disease prevails less in several great towns than elsewhere. Here, again, how much is etiological research to be desired!

Of the papers which remain for notice, that of Mr. Greaves on the sanitary condition of Manchester—for that is its main topic—may be considered as the appropriate complement of the preceding, inasmuch as he discusses the causes, the existing state of things productive of disease, all of a kind remediable, and owing to which Manchester, as he states, with an abundant supply of excellent water, and other advantages as regards climate, soil, and facilities for drainage, instead of being, as it ought to be, one of the healthiest of our large towns, lies under the opprobrium of being, next to Liverpool, the most unwholesome, as judged of by the amount of disease and its rate of mortality, and this an increasing rate, the deaths until recently in certain portions of it having been more numerous than the births. Well may the author designate the town an apparent "sanitary puzzle," inasmuch as, besides its local advantages, nowhere, he says, amongst provincial towns, has more money been expended on sanitary improvements, and "nowhere else have such persevering, systematic, and enlightened efforts been made to spread the knowledge of sanitary science among all classes of the community, and no community, perhaps, has had louder or more abundant warnings from official sources on its sanitary condition and the probable consequences of permitting it to continue unimproved." From this point of

view, Manchester may well be held up as an example and warning; for where is the town, large or small, throughout the country, which is not more or less open to the same remarks? Mr. Greaves, in his forcibly written paper, undertakes to solve this "sanitary puzzle." He connects it with the shameful condition of the dwellings of the labouring class and their surroundings, incompatible with the laws of health, and with the greatly mistaken judgment of the town authorities, who resisted an attempt which was made to correct the evils arising from the disgusting system (disgusting from neglect of them) of open "middens" and ashpits. Mere visitors, who are acquainted only with the best parts of the town—its well-built streets, handsome shops, palatial warehouses, noble public buildings—can ill imagine the contrast of the worst, as described by the author, the parallel of which, as he says, has only existed in some walled town of the middle ages:

"Narrow streets; courts closed at one end, or at both, with the entrance in the side; cottages built back to back, with no possibility of thorough ventilation; entries often more or less flooded with water, or rendered almost impassable by filth; houses dirty to an almost inconceivable extent, and so dilapidated as to be totally unfit for human habitation; horses and donkeys kept in the very houses, and pigs in close contiguity to them, and even greater and almost unspeakable abominations;—such are some of the scenes which recent investigations have revealed to myself or to sub-committees of this society. And the inhabitants have been found to match their places of abode. No untaught savage could exceed, in filthiness of person or clothing, men and women who, during the present epidemic fever, have come into the hospital under my superintendence."

The details which the author further enters into of the disgusting horrors are hardly to be credited, were they not so well authenticated. That he has not exaggerated in his account of them, we have the authority of the late Dr. Southwood Smith, who visited Manchester in 1861, and in his report on its state described what he had actually witnessed. His concluding reflections well deserve to be quoted:

"This inspection of Manchester has been a very melancholy labour. It has been so partly from the sense of physical weakness—the distressing though transitory feeling of indisposition invariably felt after a few hours of such work—a feeling increasing day by day, but far more from the mental depression caused by the sight of the discomfort and wretchedness of so many thousands of people who might be healthy and happy, and who would be so if the richer classes, whether in authority or not, would give some small but earnest and intelligent attention to the condition of the creators of their enormous wealth."

That disease, and especially febrile diseases, should be rife in a population so wretchedly situated, is what might be expected: it is, indeed, surprising that these diseases have not prevailed to a greater extent and with a greater destruction of life.

The author, as we think, has not only solved the problem—"the sanitary riddle" in question—but has also done good service in offering suggestions for the correction of the evils; and we hope he will live to have the satisfaction of seeing them acted on and carried into effect. It would be out of place here to detail and discuss them. We will only add the expression of our wish that Mr. Greaves's paper may be widely circulated, and, amongst other and more obvious reasons, for one in particular, as showing the propriety of Mr. Rumsey's recommendation of not giving local authorities too much power in regulating sanitary matters.

The wish just expressed, we are glad to see, is likely to be fulfilled by the recent article by the same author published in the 'Quarterly Journal of Science' for April, 1867, and which, written in a popular and energetic style, is well adapted to bring conviction to the narrow-minded and ignorant of the necessity of carrying out sanitary reform, regardless of vulgar prejudices, and slothful indolence and carelessness, which more or less prevail not only amongst the lowest of the low, but even amongst tradesmen and shopkeepers, who are so commonly elected to perform executive duties in our towns.

The last paper we have to notice, that of Dr. Hawksley, written, like the preceding, in a popular and spirited style, is, like it too, deserving of a wide circulation, the intent of its author being not only to point out existing abuses contrary to the laws of hygiene, but also a method of correcting them.

Following Baron Liebig and other authorities, he shows how great are the evils of the waste and abuse of human excreta, starving our agriculture, defiling our rivers, and contaminating more or less the air we breathe. His great remedial measure is the substitution of earth-closets and earth-cisterns for water-closets, to which he attributes the *gravamen mali*. According to a bill of costs which he brings "for money-loss alone against our present unnatural and unphilosophical hygienical management," for England and Wales the amount is over 45,000,000 per annum. The particulars, as drawn up by him, are the following:

	Per annum in England and Wales.	Per annum in London.
	£	£
The money value of the productive labour lost by the premature deaths of the people in consequence of bad public hygiene, especially impure air and water. Cost of funerals for the prematurely deceased. Cost of the sickness of others ill from the same preventible causes, but not dying. Expense to the community of the widows and orphans of the prematurely deceased.	32,318,672	4,847,801
Value of the refuse organic matter employed to produce these disastrous results, and lost to the land and the purposes of health and plenty. Lowest estimate.	10,000,000	1,500,000
Cost of guano and bones imported . . .	3,000,000	
	45,318,672	
First cost of the new sewers to carry on the destructive system, say £5,000,000; on this a sinking fund at five per cent. per annum, to repay the principal in twenty years, will equal an annual tax of	250,000	
Interest at four per cent. ditto	200,000	
		£6,347,801
To these losses, for the estimate of which we possess some data, there are others to be added, the extent of which is unknown; for example, the cost of maintaining the great sewerage system, how much per annum.	?	?
Cost of new water supply from Windermere, or Wales, to supplement the failures of the sewers and the wasted waters of our rivers.	?	?

Whether these calculations are altogether correct or not, unquestionably there is some truth in them. Unquestionably, it is a great desideratum that night-soil, that all animal excreta and offal, should be utilised, and chiefly in the most advantageous manner by restoring their fixed elements to the soil from whence derived, accompanied by their volatile ones, so beneficial to vegetable life and growth, so injurious to animal life.

Whether, too, the method advocated by Dr. Hawksley, of using earth instead of water as a medium for the removal of the excreta and their application to the land, be as practicable and economical as it is rational, we are not prepared to offer any decided opinion. We cannot be so sanguine as he is, when we consider the great difficulties which must be encountered in any attempts to change the system and abolish altogether the use of water-closets. He makes light of the means of supplying the earth that would be needed for carrying out his plan. Others, who have given attention to the subject, view it differently, and question its practicability, taking into account the

quantity of earth required, the labour of collecting it, the labour of distributing it, and many other difficulties. Mr. W. Longman, in his report on the drainage of Berkhampstead, enumerates these under four principal heads, and very many minor ones, as many as twelve, concluding "that, without denying or affirming the applicability of the system to isolated places or special establishments, it is inapplicable to the mixed community of a town."

That the dry plan *per se* is preferable to the liquid plan, we are satisfied; and though it may be impracticable for universal application, experience has, we think, already proved that it is not universally inapplicable,—and, indeed, in a limited way it has been decidedly successful.

This partial success is sufficiently encouraging to warrant further and more extended trial of it; and we are of opinion that its advocates, meritorious as are their exertions in attempting the correction of a great evil, would deserve more our thanks, were they to view the new method as an experiment in progress, the results of which are somewhat uncertain, rather than as an experiment, completed with results that cannot be questioned.

Review V.

1. *The Journal (Quarterly) of Cutaneous Medicine and Diseases of the Skin.* April 7th, 1867. London. Pp. 120.
2. *On Diseases of the Skin, including the Exanthemata.* By Ferdinand Hebra, M.D. Vol. I. Translated and Edited by C. Hilton Fagge, M.D. 1866. Pp. 396.
3. *A Practical and Theoretical Treatise on the Diseases of the Skin.* By George Nayler, F.R.C.S. London. 1866. Pp. 292, with 7 Plates.
4. *On the Neglect of the Study of Diseases of the Skin in England.* By Tilbury Fox, M.D., M.R.C.P. (Reprinted from the 'Edinburgh Medical Journal,' November, 1866.) Pp. 14.
5. *On Diseases of the Skin; a System of Cutaneous Medicine.* By Erasmus Wilson, F.R.S. Sixth Edition. London. Pp. 930, with 16 Plates.

The appearance of a quarterly journal of cutaneous medicine simultaneously with a new edition of Mr. Erasmus Wilson's

larger book, and an English translation of Hebra's work by the New Sydenham Society, marks the present as a most fitting time to review the position and prospects of cutaneous medicine in our own country. England is entitled to claim the largest share in the advancement, during the last fifty years, of the study of perhaps the most neglected of all departments of medicine. Hebra, indeed, yields us the palm in this respect, and it must be therefore a matter of deep anxiety that we should continue to occupy that foremost position which we have gained and used since the days and through the labours chiefly of Willan and Bateman in the reformation of dermatology. The concentration of attention towards the subject of cutaneous medicine is daily on the increase; and whilst it brings to light the fact, that in no subject is there more empiricism, and a greater deficiency of the knowledge of pathological details, it also convinces us that of late years not only have Continental students enjoyed superior advantages in the study of the subject—a fact in no sense creditable to us, seeing that we have set up a standard of medical education which, in regard to completeness, we are wont to consider unequalled elsewhere—but that the foreign schools are rivalling us, we had almost said have distanced us, by the originality of their researches and the success of investigations of a philosophic character. Whilst for a long time in Germany and in France diseased conditions of the skin have received more and more attention, and it has been acknowledged that they present ample scope for the highest scientific research, they have received less and less attention in England: they have not been allowed, as has been fairly said, "ingress into our general hospitals; and when they could not, for mercy sake, be excluded altogether, they have been huddled together into the crowded and ill-smelling outer rooms so grudgingly allotted by our hospitals to the service of their out-patients." The truth is, we are getting somewhat behindhand, and our prestige is jeopardised in the matter. We need only pay a visit to the St. Louis Hospital in Paris, or Hebra's clinique at Vienna, or to the appeal now constantly and freely made to Continental writings and researches in proof of this statement.

No man can visit the skin wards of Hebra at Vienna without confessing that he has little regard for any teaching which is within the student's reach in England, feeling not only, as might easily be shown by figures, that in one branch at least our hospital *régime* is sadly deficient, but that thanks cannot be too freely accorded to the very few whose researches and writings have kept us from actual disgrace; and foremost amongst them stands Mr. Erasmus Wilson. At Vienna there are about 250 beds in Hebra's clinique; the professor gives

lectures daily in addition to bedside explanations, and one is struck with the great variety of disease presented to the students at a glance, so to speak,—a variety in which the majority of the details and grades of the same malady are fully portrayed and readily compared, so that it is possible to see the life of a disease in a few visits, or at least the materials for comprehending it, and for drawing fair conclusions as to the prevailing aspects displayed by this or that disease, and for arriving at an approximate estimation of the influence of age, sex, habit, constitution, diet, hygiene and other circumstances exist side by side within the same walls; in fact, a sufficient number of cases exist for instituting comparisons on a scale of some magnitude. The economy of such an institution as that of Professor Hebra as regards the student's time is manifest and indisputable. The same advantages are possessed and conferred by the Hospital St. Louis, and institutions in other Continental cities. The English schools labour under corresponding disadvantages, and the student's college time runs out before he can hardly appreciate facts which are made elementary in the Vienna and French schools in consequence of the vast opportunity of observation afforded by the latter. Indeed, we in England have nothing comparable, and stand justly condemned by the verdict given by the experience of the mass of practitioners, that in regard to the study of cutaneous medicine we are needlessly neglectful. That we may be able to investigate any given case with precision, and appreciate the effect of influences that play upon it, it is necessary that it should be under constant observation; and this can only be done in the wards of an hospital. Dr. Tilbury Fox, in his pamphlet, states that there is no ward set apart for skin diseases in any of our large general hospitals. We have taken pains to ascertain the number of cases of skin diseases admitted into the several hospitals of London, and can certainly affirm that they are not sufficient, especially as no special courses of lectures are given for teaching purposes. There is, we are glad to think, some prospect of reform: not only have we an increase of young and honest workers, but some of our schools have shown a disposition to make special arrangements for the study of, and instruction in, cutaneous diseases. This is very much needed to counteract the tendency to specialism of a separative character. This is the day of specialties, and nothing could have been more opportune than the far-reaching address delivered by Mr. Bowman very recently before the British Medical Association, in which he observed that "specialties are natural products of a period of progress, and of certain favorable external conditions of society, and, as such,

should be allowed free course to develope themselves according to their tendencies;" and these words are the signal given by a prudent and philosophic man of the *fait accompli*, that we must recognise certain specialties that will at least for some time to come be more and more developed and fostered by the community at large. It is easy to place a crooked interpretation upon the above quotation. Specialists and specialism ought to be part and parcel of, to be kept in check by and to work hand-in-hand with, the great central educational hospitals; and we are second to none in the degree of our condemnation of the principle of special institutions existing apart from general hospitals.

Up to the present time, as regards dermatology, our general hospitals, we are free to confess, have not sufficiently cared for the necessities of the age, the wants of the community, or the tendencies of observers. There has been to some extent an excuse for the establishment of skin hospitals; but there can be no doubt that they have been founded, not with the view of advancing scientific research, but to further the views of individual practitioners, and they have scarcely contributed in any way to the common stock of knowledge. Recently a solitary attempt has been made to facilitate clinical observation and teaching, and to supply a deficiency felt by students generally; but we hope that, ere long, the need of such an arrangement beyond the walls of our general hospitals will not exist, and it is with a view of furthering this desirable condition that we have referred to the subject in these pages. We hope that the indications of the formation of special skin departments at our hospitals will not disappoint us, but that we shall soon see perfect provision made for the instruction of students in cutaneous medicine, and thus render wholly unnecessary the foundation of special skin hospitals: we shall then possess the fullest ground for condemning them, by taking away every pretext to which those connected with them cling as a justification of their procedures. Cutaneous medicine may fairly claim to be ranked amongst those subjects that demand a sound knowledge of pathology, and few branches of our art are capable of affording more useful lessons, or of aiding to unravel the cause and therapeutics of disease. The student leaves the hospital generally without being able to recognise the most common diseases of the skin, and the general practitioner does not hesitate to declare his incapacity regarding them; but the profession as a body, careless heretofore, are now becoming fully alive to the confusion which belongs to the subject, and display a lively disposition to encourage every attempt to place cutaneous medicine on a better footing; and we one and all place the stamp of our approval

upon honest special investigation. Under these circumstances, everything is favorable to the establishment of skin cliniques, and the carrying this out upon the principle of centralisation. It is gratifying to note also, that various examiners are showing a most praiseworthy inclination to require that the man who presents himself for his diploma possess a satisfactory knowledge of skin diseases—something more than the power to attach a certain name to a certain appearance, and associate with this as the means of cure a drug which, by the consent of empiric fashion, is believed to be a specific possessed of universal virtues, and applicable to the majority of instances.

We have everything at hand to conduce to a successful reform. We have a most extensive literature, classic as well as modern, for alterations in the health of the skin were observed, described, and recorded by the Greeks, the Romans, and the Arabians, with remarkable precision and correctness; and diagnosis with them was in the majority of cases perfect and accurate. Dubious points, therefore, have enjoyed a long-continued criticism, whilst the organ concerned has been under the control of our touch and our sight; and since the foundation and superstructure of our knowledge have thus been well tested, and a mass of uncertainties removed, our ignorance of the subject is more than ever reprehensible.

We wish to see cutaneous medicine take a more decided position within the area of general medicine—to see it less cast aside, less depreciated, more studied; in fact, taken out of the narrow field of rank specialism, and properly cared for within the walls of our colleges and hospitals. One of the means by which this may be attained is a scientific recording of facts and of the views of those who are working together in the field of dermatology.

Several of the medical schools in America are fully alive to the importance of a more careful attention to dermatological studies; and at the Haward University of Boston, the Bellevue Medical College, New York, and the Berkshire Medical College at Pittsfield, in the western part of the State, special instruction in cutaneous diseases is systematically given by professors appointed for the purpose. Indeed, the Americans are puzzled to understand how England, with her nearness to European schools, is lagging so sadly behindhand as she does in the matter.

The 'Journal of Cutaneous Medicine,' the first number of which appeared on the 1st of April last, is edited by Mr. Erasmus Wilson; but it is generally understood that he has the assistance of Dr. Tilbury Fox. In the Introduction, we are

told "that much may be accomplished towards the perfection of the science and the management of those diseases (cutaneous) by the united labours of the many in one direction," and "that this purpose would be much favoured by the publication of a journal which should record the observations of fellow-workers in the same field; and in furtherance of this purpose, we invite the literary aid and assistance of all those who have already made or are willing to make the study of cutaneous medicine an object of interest." The Journal is a quarterly one, and intended to embrace lectures on cutaneous subjects; papers, essays, and original investigations; reports of cases; hospital reports; cutaneous hygiene; therapeutical memoranda; illustrations of the progress of science at home and abroad, and other matters; in fact, it is intended to become the "archives of every matter of interest appertaining to the cutaneous system." Among the contents of the first number are a lecture on the Anatomy and Physiology of the Skin, and a paper on Herpes and Zoster, by Mr. Wilson; papers on Lupus by Mr. Milton and Dr. M'Caul Anderson; on Tinea, or vegetable parasitic disease, by Dr. Tilbury Fox; on Eczema, by Dr. Fraser; on Chloasma, or pigment alteration, by Professor Hebra; on Psoriasis and Lupus, by Mr. Hutchinson: also reviews of the works of Wilson and Hebra; editorial remarks on Nomenclature, &c. There can be no doubt, from the mere mention of the above, that the Journal is calculated to be of great service to medicine, and to fulfil the object for which it is designed; and a perusal of its contents will show us that the disconnection between the work of cutaneous pathologists has led to no little confusion in the subjects of classification, nomenclature, pathology, etiology, and therapeutics. In fact, up to the present time, observers have worked quite independently of one another, and, having had little opportunity of comparing together their several interpretations of phenomena or individual observations, have often arrived at totally different conclusions on the same subject. We purpose, instead of analysing in detail the works placed at the head of this article, to enter into a suggestive general examination of the subject, and to indicate the special points that deserve attention at the hands of investigators.

Hebra's work, which includes a portion only of the diseases of the skin, has been very recently noticed in these pages and its contents will, therefore, be only referred to in their comparative aspects in relation to English pathology and practice; but it would be most unjust did we not here compliment Dr. Hilton Fagge on the excellency of his translation, and the amount of industry he has displayed in the work which he undertook for the Sydenham Society. It is only needful to add, that Hebra's

work should be studied by all who are interested in the subject of dermal pathology.

Now to our criticism. There is no question of greater pertinence touching the subject in hand than that which seeks to ascertain if there be any difference in character or aspect in diseases as they exist in different countries. It may be put in other words thus: The same disease existing in our own and other countries, does it possess any appreciable or contrasting peculiarities, either as a whole or in its individual components, in the two instances, by which it may be distinguished? *i. e.* have variations of climate and race any material influence?

Both Mr. Wilson and Dr. Tilbury Fox assert the affirmative; and the latter, basing his remarks upon an experience gained not only at home but abroad, instances the cases of lichen ruber, itch, eczema marginatum, favus, syphilis, pityriasis rubra, and other varieties of cutaneous mischief. This is an important matter for consideration at a time when the opinions of foreign dermatologists have been and are being freely introduced and freely used by us, as exemplifying the phenomena of disease in our own country. That there are differences between the same disease as it exists here and abroad, dependent upon variations of climate, mode of life, constitution, and other influences, can scarcely be doubted; and we must allow that there are certain peculiarities (markedly the existence of a sthenic type), consonant with the English mode of life and constitution, which should make us cautious in being wholly guided by the observations of foreign authorities in an estimate of the diseases of our own country. There is a nationality about diseases, as well as in *physique*, temperament, or modes of thought. Some writers adopt the German, and others the French views; but it is quite certain that he who, bearing in mind the important consideration just referred to, has seen and *compared* all varieties, has much advantage over one who has not; but if one inundates us with the inferences deducible from the facts observed abroad under circumstances different from those which obtain here, and attempts to make those inferences the explanation of the features presented by the diseases of our country, he must lose credit by his neglect of that which is not only *à priori* likely to the acute thinker, but also patent to the careful worker—that the same disease varies in different countries, often considerably so.

Scabies is a much more ugly affair in Hebra's wards than in this country. The acarus is at the bottom of the mischief; but the patient's constitution and nutrition are very unlike in the two cases, and hence the results differ. Nothing is more striking to the eye of the Englishman who goes his first round through the wards of an Austrian clinique, than the peculiar ulcerative aspect

of the various cases, and the cachectic look of most of the patients: indeed, many of the descriptions given by the Continental observers of ordinary cases represent exaggerated forms of disease as seen in England, and there is a corresponding distinction to be drawn in regard to therapeutics.

Dr. Jeffries, the accomplished lecturer on Diseases of the Skin at the Pittsfield College, a pupil of Hebra, tells us that though he has been able to carry out in general the treatment he learnt from the distinguished German professor, he finds that the people are much more "nervous" in the dry climate of his country; they react, so to speak, much more quickly to the external and internal exhibition of medicines: and he adds, in writing to us, "that Hebra's book, therefore, which is classic, and will remain so a long time, must be modified for this country."

The gist of these remarks is the necessity for the encouragement of an English school of cutaneous medicine—a point particularly insisted upon by recent writers. Having so far shown that we must have a care lest we fall into error in the application of foreign to home practice, we proceed to ask whether the diseases as they exist now-a-days are the same in character as those noticed by the ancients;—in other words, have any diseases died out to be replaced by new ones, and have original types been preserved? The question is suggested by the discussions that have lately arisen in regard to leprosy. Writers of great credibility assert that the lepra of the Hebrews was a form of malady peculiar to that people (this was Lorry's view). The modern race is exempt from any characteristic of the kind; and we are, therefore, driven to the conclusion that if such a disease did exist, it must have long since died out from amongst them, or that the Hebrew lepra must be identical either with leprosy or lepra vulgaris. There is reason to think that the term lepra (or Hebrew leprosy) as used by Moses had a wide signification, and included many different diseases, which were distinguished by their subsequent progress, and pronounced accordingly clean or unclean; their inclusion under one head was calculated in the best possible degree to detect not merely the earliest trace of mischief, and all instances of disease of a graver character, but to have under observation those of doubtful nature. The most recent researches clearly show—and this view is maintained by Mr. Wilson, Dr. Vandyke Carter, and other Englishmen—firstly, that the two forms of the berat of Moses, the morphæa alba and nigra, and the melas and leuke of Celsus, are identical, and we now know that they are early forms of leprosy: secondly, that the boak of Moses, the alphos of Celsus, and the lepra vulgaris of Willan, are the same; and it has been recently stated by Dr. Tilbury Fox that

he became acquainted in Syria with a form of malady called baras el Israily (Hebrew, baras), which on examination was found to be the lepra vulgaris. This gives us a clue to the belief that the disease *supposed* by modern writers to have been peculiar to the Jews may probably be the same that we now know to be lepra vulgaris: whether it exists in a different form at the present day is a matter of uncertainty; at any rate, the point to note is this—that we have good reason for believing that in regard to leprosy and lepra, no new diseases have sprung up, and none have died out. "To our mind," says Mr. Wilson, "the identity of elephantiasis Græcorum and the leprosy of the Hebrews—if, indeed, the latter be not, as imagined by certain divines, a mere symbol of moral disease or sin—is a fact which cannot admit of the slightest doubt." It is an instance of the discriminative learning and diagnostic powers of the early Arabian writers, that they should have seen the connection between the one baras and elephantiasis Græcorum, and the essential difference in nature of the baras el Israily; and no less interesting is it to note that in India it is the commonest thing to classify those things together by mistake, which Moses placed together purposely—for example, the lepra vulgaris or alphos of Celsus, and the form of morphæa (or berat)—a fact in itself of much proof in favour of the facts as stated above. We have referred to the case of leprosy as the best illustration of the point under notice. Another example is to be found in the case of animal and vegetable parasitic maladies. They are not of new origin, but were included in the descriptions of non-parasitic diseases: eczema included scabies, the kerion of Celsus is a form of tinea tonsurans, and favus was confounded with the impetiginous eruptions of the scalp. This must be recollected in appealing to the descriptions of the ancients to enlighten us upon dubious points in the teaching of our own day. There is an evident tendency at the present time to refer back and rely upon the earliest accounts of disease, in the absence of original research, which is likely to lead us into error, if it be true that, in consequence of a deficiency in pathological knowledge, different things were confounded together; but the case is otherwise if we ascertain that those diseases which we supposed to have existed and were improperly classified are of recent occurrence. However, as far as we are enabled to judge, it seems certain that modifications as to topography, type, and degree, have been common, but the development *de novó* of new maladies has yet to be proved. A very striking illustration of the springing-up of a well-known disease in a new country, which gives an appearance of new birth to a disease, is exhibited by the case of rubeola notha. It is said by Dr. Salisbury to be connected in some way

with the presence and action of the fungi on mouldy straw, and was certainly not known in the New World till the introduction thither of wheat. But this does not prove that rubeola notha is a new disease. It has been acclimatised, there is reason to think, in Egypt, the land of corn, from time immemorial. It is an instance of the migration of disease, and, inasmuch as circumstances (such as climate, diet, sanitary measures, intermarriage, and intermixture, &c.) which play upon the individuals of different countries have considerably altered their character in the progress of civilisation, certain changes must have been induced hereby, and it is a matter of no astonishment that recent research should have shown that some affections are dying out here, to be reproduced or replanted rather there, and with a somewhat different aspect. The case of leprosy, which in some countries is losing its tubercular and assuming an atrophic (anæsthetic) form, may again serve as an illustration, inasmuch as it is a subject to which our Government is giving apparently some attention. The appearance of leprosy after the Crusades, and its subsequent disappearance from England after the enforcement of the segregation of lepers and an improvement in the food of the people, are facts of great interest in this respect; and though the gigantic manifestations of the thing itself have disappeared, the lesser traces of its former presence are to be found in England, and the morphæa, the hide-bound disease sclerosis, and the area alopeciata of the scalp, await a very careful examination of their pathologies as bearing upon this point, whilst it would seem that traces of elephantiasis Græcorum have been observed in our own country in an increasing proportion. Migration of old and generation of new diseases must not be confounded.

This is not a mere question of historical interest, for a knowledge of those things which have conduced to *modified aspects* gives us a powerful preventive prophylaxis, and a most reliable clue to the nature of those attendant circumstances which we ought to annihilate in our endeavour to check disease. With regard to eczema, it may be asked—Is it passing through any special crisis? Is it losing somewhat its vesicular, and taking to itself an abortive or excessive action? or is the absence of vesicles dependent upon the fact that the early stage is rarely seen by the physician? or are our definitions shifting? These questions are prompted by the fact of so much change of opinion regarding skin diseases having occurred within a very recent time.

Dr. Fox has suggested another instance which bears upon the modification of disease by the operation of new influences to which have been given wider and wider play. He has described in full detail the features of a "contagious impetigo," known also under

the designation of porrigo (of Startin), which frequently assumes an epidemic form, and is said to exhibit transitional grades between ordinary impetigo and bastard vaccinia; and it was a question discussed by Dr. Fox whether the affection has not some connection with the vaccine or varioloïd poison. It can be reproduced as to its typical eruption by inoculation with its pus, and again and again by experiments with pus taken from a series of inoculations in the same subject; and the facts as yet known in reference to the disease seem to indicate the importance of instituting a careful examination as to the influence of the practice of vaccination upon the community at large. Sufficient time has perhaps scarcely elapsed as yet for us to have collected the materials of a reliable conclusion on the point. Still for years past we have been steadily introducing into the systems of our children a potent virus. With what result? Has the influence of the virus extended beyond the production of those immediate effects which we term vaccination, or does it conduce to the occurrence of secondary phenomena, that are not so immediate and direct? and has it any modifying influence upon other diseases? Vaccinia is an acute specific disease, and we acknowledge that acute specific diseases very materially modify the nutrition at large. Measles and rubeola are liable to be followed by the development of the tuberculous crasis. Has vaccinia in a similar way any effects appreciable after the period of desiccation?

A criticism of the changeableness of diseases and the influence of modifying agencies pre-supposes a good knowledge of the natural (normal) course of each disease of the skin; and of that we are quite ignorant, having little opportunity of watching it. This is Hebra's charge against us. As matters of simple clinical observation required of us may be mentioned the following:

1. The significance of coëxistences (or complications).
2. The normal duration of diseases.
3. The natural changes undergone by diseases when uninfluenced by medicines, affording us thereby an idea of the plan by which Nature effects a cure and guides to treatment.
4. The effect of modifying agencies as to climate, constitution, diet, age, sex, including the peculiarities of the same disease in different countries, and the special dermatology of each country.
5. The effect of local remedies upon the natural course, with and without constitutional measures.

We now turn for a moment to the matter of *nomenclature.* At page 107 of the Cutaneous Journal, we find a chapter headed "Dermatological Nomenclature," and commencing thus: "What

are dermatologists about?" The writer argues out the necessity for a universal nomenclature. The subject is one of great urgency, simply on account of the growing tendency on the part of different writers to use the same term in different senses. The Journal contains an article by Hebra on "Chloasma," which English readers will at once take to mean pityriasis versicolor: not so—it is applied to pigment stainings of the skin. This must inevitably puzzle the English student. In Dr. Belcher's paper on "Leucoderma," the difficulty of satisfactorily settling the meaning of vitiligo is seen. Hebra and Wilson are at variance as to the application of the terms psoriasis and lepra. Mr. Wilson suggests a new word again—*phytosis*—as the generic term for the vegetable parasitic disease; but in this case the word *tinea* is by far preferable, and it is now very generally accepted by modern writers. The coining of new names is a failing that deserves the severest condemnation, and, above all things, we would deprecate the use of old names in senses different altogether from their wont, by which well-established designations, which have passed current for years and become familiar as household words with most definite signification, are used in such a way as to lead to confusion. The word psoriasis had better at once be got rid of, if it is to be applied to chronic eczema. There is no objection to the use of the term alphos to signify lepra vulgaris—for lepra rightly belongs to elephantiasis. The elephantiasis arabum, or Barbadoes leg, is better known as bucnemia tropica, and the introduction of spargosis would only create confusion. The wide significance given to eczema so as to include lichen and prurigo is one that cannot as yet be accepted; it should be confined entirely to that disease as defined by Willan. Mr. Wilson has added to the new edition of his work a glossary of terms, which will be of essential service in helping to the correct understanding of the various terms in use at different times, and as pointing to their derivation and original significance; and it "will also, it is hoped, act as a restraint upon the terminological innovations of modern nomenclators." Mr. Wilson informs us that his endeavour is to "invest terms with the meanings which were attached to them by our ancestors;" and he has certainly acted freely upon it. We have given good reason already why we should be cautious in adopting the ideas of the ancients. The following alterations may very well be adopted:—Leprosy may absorb elephantiasis and lepra; alphos, describe lepra vulgaris; tinea, designate the genus of vegetable parasitic diseases; the Barbadoes leg, or elephantiasis arabum, be called bucnemia; vitiligo, be altogether discarded; and leucoderma, be used to indicate deficient pigmentation. On the other hand, to call chronic eczema psoriasis, to include truly

papular disease with eczema, and to appropriate the term chloasma for a non-parasitic disease, are not convenient nor conducive to simplicity. In many instances there is a commingling together of diseases. Mr. Hutchinson, in a paper in the Cutaneous Journal, on "Psoriasis and Lupus," has suggested the use in such cases of a combination of the names of the substantive diseases: ex. gr., eczema-lichen; prurigo-pemphigus. This is, perhaps, already met by the addition of an adjective to a substance, as in the case of pemphigus-pruriginosus: there is little difference between the two plans. In fact, it appears to be best in every way to use terms as much as possible in the manner in which Willan employed them, for the majority of them have now become significant enough by the lapse of time, and all we need may be readily secured by rejecting those which are really of dubious meaning, and substituting appropriate classical ones.

Classification.—There is no subject which has offered a wider range for the display of originality than that of the classification of diseases of the skin; and in Hebra's work and that of Mr. Wilson we have the best illustration of the absolute difference which may obtain between observers of equally good, varied, and large experience. The former makes twelve, the latter twenty-two classes. Indeed, no two writers agree with regard to the best mode of arranging cutaneous maladies; each lauds a scheme of his own making—Bazin, Hardy, Cazenave, Startin, Tilbury Fox, Buchanan, Gull, Simon, Hillier, and others. Now, we may classify in three different ways—anatomically, pathologically, or clinically. The tendency at the present time is to resign anatomical in favour of pathological considerations, but we are scarcely in a position to succeed in anything beyond an approximation to a complete system of the kind, for the simple reason that pathology is shifting very much in regard to skin diseases; hence much is uncertain and indistinct. The most common disease eczema is regarded by some as a catarrhal disease; by others, as a variety only of a disease, the whole of which includes many others now looked upon as separate existences; by some, again, as vesicular; by others, as non-vesicular. Hardy argues for it as a member of his class dartrous; Wilson and Hebra, as an inflammation which may be erythematous, papular, vesicular, pustular, or fissured, either *ab originé* or *in totó*. The tendency of all this difference of opinion is clearly to overthrow the importance which up to the present time is claimed by and has been accorded to Willan and Bateman's scheme. The latter supposed that certain eruptions, to which appertained certain special and recognisable characters, and which have been called "elementary lesions," were charac-

teristic of certain diseases; that, for example, lichen and prurigo were characterised by the occurrence of papules different in each case, and distinctive: but a gap of wide extent remained between the labours of Bateman and Willan, and those of modern dermatologists; and in the interim, as often happens, the first principles of the new science were not correctly estimated, from the want of immediate workers and followers.

We have now suddenly begun to Darwinise freely with Willan's labours, and believe that certain elementary lesions previously considered as distinct are but varieties of one species of change. The prevailing idea is, that all elementary lesions are but modifications of one form of inflammation. When pathology shall have answered satisfactorily the two questions, Are not diseases of the surface characterised by certain special elementary lesions? and may these not present similitudes, but yet be different in their totality? then we shall be able to form an acceptable classification. This much is certain, that in all those arrangements that are regarded as deserving of approval, the essence of the system of Willan is embodied, as in Hebra's class of exudative dermatoses. Willan and Bateman's system was the first step in the right direction, the rough outline of the truth—in detail undoubtedly erroneous, but in principle essentially correct; a plan manifestly true to life, and best suited for teaching purposes. Let us compare Hebra and Wilson. Hebra has certainly given the best pathological classification; he has groups named hyperæmias, anæmias, anomalies of glandular secretion, exudative (acute and chronic) hæmorrhages, hypertrophies, atrophies, neoplasmata, parasitic, pseudoplasmata, ulcerations, neuroses—twelve in all. In the details of this scheme, Hebra makes use of anatomical considerations, and recognises, as we have said, in his subdivisions, Willan's ideas and system. We ought to allow anatomical considerations to influence us somewhat, for by this means we arrive at the seat of the disease, and are then enabled to guess to some extent the character of the affection. Hebra's classification does not help us very much to the true cause of disease, whilst it lays stress upon one feature only—the local expression, so to speak, of each disease; and then it only gives one a general idea of that. We wish to possess some plan by which a more useful idea of disease could be conveyed, and in this respect another mode of arranging diseases of the surface has the advantage. It is that styled by Mr. Erasmus Wilson *Clinical*; and in his last publication it is given to the profession as the result of his long and matured experience. Mr. Wilson makes twenty-two groups—(1) eczematous; (2) erythematous; (3) bullous; (4) furuncular; (5) nervous; (6) vascular; (7) hæmodyscrasic; (8)

developmental and nutritive; (9) hypertrophies and atrophies; (10) zymotic; (11) alphous; (12) strumous; (13) syphilitic; (14) carcinomatous; (15) leprous; (16) affections of the hair and follicles; (17) sebiparous; (18) sebaceous; (19) cromatogenous; (20) diseases of nails; (21) traumatic; (22) phytodermic. Here it must be observed that the names of classes are given in some cases according to (*a*) *the nature of the cause;* in others, *anatomical, e. g.* the hair and nails; and in others, again, *pathological* considerations, *e. g.* carcinoma. So that it is curious to observe the leading authorities in England, in France, and in Germany adopt a different system of classification: the first, according to a mixed idea, so to speak; the second (if we take Hardy), according to causation; the third, chiefly according to the local pathological expression. Pray, let us ask, at what do we aim in making any classification? We desire to classify diseases, so that they may be arranged carefully in our own minds for purposes of reference, and particularly that the student may be able to learn dermatology more easily; and that he may do so the more readily, it is desirable that any system should possess at least a uniformity of plan—an arrangement including the broadest but safest amount of reliable knowledge. Well, we observe, (*a*) that the external appearances of cutaneous diseases are the evidence of certain altered states of blood or nutrition; (*b*) that these states differ in kind; the rule is that the naked-eye evidences of each perverted state of blood has its peculiar type—for example, acute specific diseases, cancer, syphilis, struma, parasitism, &c. That being the case, our first step is to arrange these diseases according to the different types of action; this Willan has nearly done, but we require something else—*we want a designation for each group that shall express as much of the total disease as possible, especially the cause, and, in addition, the kind of the elementary lesion—in fact, the pathological lesion*—as much as the word cancerous does, as designating a group of cutaneous affections. Now, what do we mean when we have a patient before us and affirm that he is affected by cancer? Nothing more or less than this—that we have seen and felt a certain enlargement which we class under the term "tubercula," which has certain characters peculiar to itself, and that it is the expression of a peculiar state of nutrition which we have called cancerous. The latter word conveys to us many things, especially the approximate cause, the pathological lesion, and the general character of the disease. The same thing is seen in the use of the word parasitic; it means a special state of nutrition which is favorable to the growth of fungi, producing characteristic effects upon the hair and epithelium. So with acute specific diseases—syphilitic,

strumous, neurotic, &c. Hardy, then, is right in principle, but wanting in detail. Hebra only gives us the local expression. The anatomical considerations in Mr. Wilson's scheme are so very inconsiderable, that his must be pronounced, in accordance with our wants expressed just now, as most worthy of our reception. We hasten to add, that the principle which we have here attempted to show to be worthy of adoption is merely a larger generalisation upon Willan's system. The latter contained the germ, the idea, the suggested principle of a more complete scheme.

We may then well receive the terms strumous, cancerous, parasitic, syphilitic, acute specific, nutritive, including atrophy, hypertrophy, and congenital diseases (such as ichthyosis and xeroderma), erythemata (the lesion being hyperæmia simply), eczematous (lesion a vesicle), lichenous (lesion a papule), bullous (lesion a bulla), alphous, leprous, chromatogenous diseases. Pyohæmic (furunculi and pustular diseases), hæmodyscrasic (purpura), neurotic—the diseases of the glands want a word. The diseases of the nails will be met with under the heads alphous, syphilitic, pyohæmic.

There really is simplicity here. The plan is uniform, and the definition of each term implies the recognition and description of the cause (crasis or diathesis), which we may not however accurately know as yet, and in addition the pathological lesion. In the case of the groups bullous, eczematous, lichenous, &c., we must do what has been done with such instances as cancerous and strumous—work out carefully the whole histories of the diseases. And if it should be shown that the lichenous and eczematous affections are one and the same in kind, it is easy to erase one name, or make any other change according to the teaching of pathology. The adoption of such a scheme as Mr. Wilson's makes every possible allowance for probable changes in pathology, which is no small proof of its truth.

The causation and pathology of diseases of the skin are subjects pregnant with difficulty and interest. It is important to cling fast to the idea that the pathology of the skin is identical in kind with that of the body generally, and for this reason analogical evidence is invaluable in the elucidation of dubious and uncertain points. If we turn to any text-book on diseases of the skin, we shall find that an infinite variety of circumstances trivial in themselves are asserted to be causes of very different diseases—predisposing and exciting causes are raised to the rank of *veræ causæ*, in the sense of unconditional sequence. We allow that certain agencies have a selective action for particular tissues, but it is with reference to those influences that act more generally upon the system that we now speak; such

as errors of diet, cold, excesses, fatigue, mental worry, functional disease, and the like. None of these are definite in their action as specially related to any particular form of disease; they induce depravation of the body and its tissues generally, but our language and treatment would seem to show that they are the essential *producers* of mischief. In speaking of classification, we use the terms strumous, cancerous, syphilitic, alphous, eczematous, and others, and they convey to us something very definite; for example—cancer implies that there is a form of nutrition in which the tendency is to the local development and aggregation of certain special cells possessed of the power of endogenous growth, undergoing peculiar changes subsequently. Now here you have a special form of life set going by the tissues themselves in relation to, it is thought, a suitable form of nutritive fluid. Could a blow produce this? Wilson and Hebra say that alphos (lepra vulgaris) is the result of a special blood-state *sui generis,* and discard all other considerations; but even in this disease the patient may be apparently healthy to our keenest appreciation, and while there is every appearance of a degenerate kind of growth developed locally in the derma. In fact, the important point to remember is this—that beyond and besides a special state of the *nutritive* fluid, the *tissues* of the diseased part have a reciprocal action upon the blood, and that disease probably often takes origin in a disorder of the function and growth of the tissues themselves.

Healthy nutrition means nothing more or less than that the tissues grow according to what we call a healthy standard when supplied by proper food; so diseased alterations are the result of an altered relation between the blood and the tissues, both of which participate in the production of the changes which we call disease, and it is this relation that we know comparatively little about. It is too diffuse a subject to enter into here very fully. We are not without significant indications in many diseases: in lupus we have a tendency to fatty change; in cancer, to vigorous proliferation; in leuce, to atrophy and decay; in fibroma molluscum, the fibrous tissue seems to appropriate a greater supply of pabulum, and the essence of the disease is clearly in the tissue action, just as it is in cancer. It is into the mysteries of cell-life that we must penetrate for a keener conception of diseased conditions of the skin. This doctrine is nowhere more clearly brought out than in the recent views which Virchow has promulgated and noticed in his recent lectures devoted to the pathology of the tumours of syphilis, lupus, lepra, and glanders; and it is dwelt upon especially by Dr. Tilbury Fox, in his pamphlet before us. He points out that one likely way in which peculiarities of cell-life may

be seen is by criticising hereditary affections: the longer a disease due to general causes lasts, the more do the local powers deviate from their natural function, until the diseased change becomes a habit, at the same time that the general state upon which the malady depends may be even lessening in degree. So in hereditary eruptions: for when the latter have existed in the parents, perhaps a long time, the begotten inherits not only the blood-state, which is now perhaps somewhat passive, but also a certain local peculiarity, a peculiar disposition to perpetuation of the local change. Here is an instance of a special peculiarity of cell-life preserved from one to another generation. This line of reasoning may be applied to the more common forms of disease. Now the participation of the tissues themselves in the production of disease is a very active one. The leading and fixed idea in most men's minds as regards the essence of the majority of skin affections is simply this—that the basis is inflammation (as a primary condition.) Now there appears to be no particular distinction of kind made; perhaps the not very clear views in existence about inflammation lead astray. The limit between inflammation and ordinary nutrition is not well marked. In one sense, the former is an over-expression of the functions of the ordinary agencies (circulation, nerve supply, and local power), hard at work to get over some difficulty. It is an eliminative and reparative process, well seen in the desquamation the result of a poison which spends itself upon the skin. In inflammation there needs be a good supply of blood to furnish material, and a great activity of the nerve government: hence congestion, heat, pain. Vesiculation signifies the introduction of another phase in which effusion helps, and in pustulation a different agency is at work, while the result depends upon the state of blood in relation to the behaviour of the tissues. There are many different desquamations, vesiculations, pustulations; each case of its kind exhibits, however, a similar type of action, but is not necessarily the same in total features. This is the case in papulation, as in lichen, strophulus, prurigo. Again, the greater degree of expression of inflammation involves the occurrence of the minor states, so that the idea of papulation includes more or less that of congestion, and the idea of pustulation that of the other two; but the potential characters in each instance are different. Now, we are apt to look upon all inflammatory changes in the skin as modifications of *one* inflammation; but it is more convenient that inflammation should be regarded not so much an entity as a *mode of operation*. It is a type of action common to many affections: some require the help of one, some that of more of its stages or degrees; in one case erythema suffices, in another pustulation is required.

The development of the fuller or higher stages necessarily is preceded by that of the earlier ones, though these may be only lightly expressed: the type, and hence the result, is modified according to the nature of the *cause;* the blood-state, in relation to the behaviour of the elements of the diseased structure. Now it follows that every erythema is not identical—every papulation is not exactly the modified erythema, or a pustule the modification of the papule, of common inflammation. Erythema may be the type of expression of disease produced by many different poisons, so with papulation and vesiculation; the type is modified in each case by the nature of the cause. For example, a lichen from the moment of onset has a tendency to the exudation of lymph of a peculiar kind, which forms a papular elevation; and to supply this material a certain degree of erythema is needed, but the latter is not well marked. Prurigo tends to the same end, but a modified result is obtained. An eczema tends from the moment of onset to produce elevation of the cuticle by a special fluid, and needs the assistance of erythema, causing also in its progress resemblance to papulation. A herpes observes the same type of action, but gives us a modified result. So is it with pustular disease; for example, ecthyma and impetigo. Now, a disease may fail in its full development only reaching an early stage: for instance, a vesicular disease may be arrested at the erythematous or the papulous stage. Hence the same disease—say eczema—may in one sense be erythematous, papulous, vesiculous, or even pustulous; but the true description would be certainly that which gives it a vesicular nature. This fact has led many moderns to assert that, inasmuch as they have observed an erythema become a papule, a papule a vesicle, a vesicle a pustule, and so on, that many diseases are identical. Eczema in its papular stage has been called a lichen; this cannot be; the papulation of a lichen, of an eczema, of a prurigo—the pustulation of an eczema, of an ecthyma, of a smallpox—the vesiculation of a herpes, of a pemphigus, of an eczema—the erythema of a pellagra, a rubeola, an erythema nodosum—all present in the several groups a resemblance, because they follow a certain type of action, due to the mutual action of tissues and blood-states, and nerves; they are all different in their totalities. Hebra in forming his division of eczema has in reality unwittingly accepted this view; but he does not seem to be aware of the law involved. He takes an ordinary irritant (croton oil), and applies it to the skin, and finds that there are certain stages which follow in order according to a certain type of action; and, by analogy, he divides eczema into corresponding stages, and yet he misses the true interpretation.

The syphilitic poison shows itself in its effects upon the skin according to the same *modus operandi*, but there are peculiarities in its manifestations which we well know and acknowledge. Smallpox passes through all the stages of and employs inflammation; but the results are peculiar, because the blood-state is peculiar, and therefore the tissues, though employing the same agency as other diseases, behave in a special manner towards it. Most recent writers altogether miss the appreciation of this difference of kind in conjunction with the same machinery or plan of operations, and no one has as yet drawn the distinction which it has been attempted here to describe in detail. Had they done so, they would not have mixed up under the term eczema, such dissimilars as lichen, prurigo, psoriasis, pityriasis, and rosacea, which, however, of necessity, as before observed, have relations, because they each employ the same process, viz. inflammation.

All this involves the doctrinal teachings of Willan and Bateman, which are now being assailed on all sides. True, they require some modification to harmonise with the more recent pathology of the day, but they cannot be set aside as untrue.

The whole thing turns upon our understanding that diseases of the skin are especially brought about by the altered relation between certain states of blood and ordinary tissue-life—by a disorder of the two together. If, instead of saying that lichen was a papular disease, Willan had said that it depended upon a peculiar state of blood, and expressed itself by the exudation into and appropriation by the skin of a lymph of special characters, which, moreover, tends to accumulate and form a little elevation, called a papule (its pathological lesion), having peculiarities from other papules, or that eczema in like way was due to a special state of system generally, and locally the exudation of a fluid *sui generis*, with accompanying congestion, often *quasi* papulation, and the tendency more especially at first to elevate the cuticle into little bladders called vesicles, he would probably not have been attacked. A great field of research is open to us, then, to determine what are the peculiarities of each species of elementary lesion, and upon what these depend.

A class of writers who are known by the title of neuro-pathologists have lately created for themselves an undoubted right to be heard in behalf of the influence of the nervous system in the production of disease. There can be no questioning the fact, that the nervous system and its ramifications determine the seat of disease and modify the supply of blood sent to it: that alterations of the nerve-trunks or centre directly conduce to disordered states of the surface, is every day more distinctly proved; but it appears to us that we must never lose

sight of the disordered tissue action, &c., where the nerves are involved as the apparent causes of disease. The observations which Mr. Hutchinson especially has made with so much scientific accuracy in neural pathology are highly important and suggestive. They deserve to be studied closely by every student of cutaneous medicine.

It is only right to conclude this part of the subject with a reference to mixed forms of disease. The coëxistence of two diseases is no proof of identity. In the 'Journal of Cutaneous Medicine,' Mr. Hutchinson draws attention to the occurrence in the same subject of lupus and psoriasis. This is a possible occurrence, and we have seen it clinically illustrated; at the same time, we know that lupus erythematodes is sometimes mistaken for a mixture of psoriasis and lupus. Dr. Anderson also in the same place records the case, illustrated by a photograph, of a black man who was affected at the same time with fibroma molluscum, hypertrophy of the scrotum, and kelis. The cases of prurigo and pemphigus, urticaria and lichen, and many other examples, teach us that dissimilar affections may coëxist, and suggest a caution to us not to generalise too freely upon the identity of cutaneous affections.

We must omit any notice of skin therapeutics, and reserve that for another occasion. We have already occupied much space, but our object has been to show, in the wide range over which we have travelled, that the high position which we have hitherto held as pioneers in the advancement of cutaneous medicine is being taken from us by the competition of Continental observers; that little is taught now-a-days in England touching diseases of the skin; that it behoves us to have a care in allowing ourselves to be wholly led by even the most trustworthy experiences of foreign authorities, for there are peculiarities in regard to the skin diseases of our country which necessitate a special therapeutic plan consonant with English habits of constitution; that whilst there is the widest difference of opinion amongst authorities upon many elementary matters, a more careful study of cutaneous medicine must yield results of highest interest and wide applicability in the field of cell pathology, and not until this has been more carefully explored shall we ever be able to free ourselves from the empiricism that reigns supreme over our modes of treatment; and, lastly, that cutaneous medicine is a subject which may fairly claim the profoundest attention of the most able amongst us.

REVIEW VI.

Undersögelser angaaende Brug og Nytte af vore stivelseholdige Næringsmidler. Af Professor Dr. FAYE. (Særskilt aftrykt af 'Vid.-Selskabets Forhandlinger for 1866.') Christiania. 8vo, pp. 45.

Investigations respecting the Use and Value of our Farinaceous Aliments. By Professor FAYE. (Reprinted from the 'Transactions of the Academy of Sciences for 1866.')

THE investigations referred to in the title of Professor Faye's pamphlet were intended to solve, on scientific principles, a question which had been the subject of controversy in certain popular periodicals—whether, namely, "a great part of the population, who live essentially on farinaceous food, prepare the same so imperfectly that great quantities are lost in consequence of the indigestibility of the food." The latter statement refers more particularly to a species of food national in both Norway and Sweden—"gröd," evidently corresponding to the "porridge" of Scotland, or "stirabout" of Ireland. Professor Faye, in starting, adopts Baron von Liebig's division of aliments into the plastic or histogenetic: albumen in its various forms, and the analogous substances, fibrin, gluten, and casein; and the respiratory: starch, dextrin, sugar (the so-called carbo-hydrates), and the different kinds of fats; a classification which he believes to be physiologically appropriate, and to give a very good idea of the value of aliments.

The author proceeds to inquire, what part starch plays as food? It is destined to serve principally as a combustible during respiration, and so to assist in the development of the heat of the body. What cannot be used in this way is present in excess, and is therefore useless, perhaps injurious; it will either pass away only partly digested, as excrement, or it will, after having been changed into glucose, and perhaps into fat, be excreted and deposited in other modes. This may take place without injury to the system, but it may also produce diabetes, or fatty degeneration of the heart and other muscles.

Professor Faye, having taken a rapid review of the food of different classes of animals, proceeds to describe that best suited to man under various circumstances. He shows that persons who from circumstances are compelled suddenly to change their mode of living, usually feel ill, and lose both in strength and condition, until the digestive organs have adapted themselves to the altered diet; a fact which it is important to bear in mind in

physiological experiments upon animals, and which diminishes the value of alternating experiments performed upon a single dog, for example, especially where the diet in such a (carnivorous) animal is suddenly changed from albuminous to farinaceous food. At the same time, it must be acknowledged that the system possesses a remarkable power of adapting itself to the food presented to it; and it appears that after some time the digestive powers become strikingly sharpened in a particular direction, and this even for matters rather difficult of digestion. In times of scarcity, too, the human organism is capable of discovering and utilising the nourishing constituents of apparently indigestible or very poor aliments. In reference to the relative quantities of the nitrogenous and carbonaceous food necessary to keep up the metamorphosis of matter, and to maintain the strength and condition of the body under moderate exercise, but without very great exertion, the author refers to the researches of Christison and Maclagan ('Monthly Journal of Medical Science,' May, 1862), pointing out the close coincidence of some of their results with those of Hildesheim ('Die Normaldiæt,' Berlin, 1856), and C. Majer ('Schmidt's Jahrbücher,' No. 10, 1866). Professor Faye quotes also Dr. Bonnar's paper in the 'Edinburgh Medical Journal,' October, 1865, and 'Schmidt's Jahrbücher,' i, 1866, and Dr. Playfair's 'Food of Man in relation to his Useful Work,' and shows that with the conclusions deduced in the English and Scotch works just named, those of a German investigator, Director Schneibler, closely agree. The average amount of daily food assigned by these writers as necessary for the above purpose is rather more than four ounces of nitrogenous to seventeen ounces of farinaceous and fatty aliment.

"If the population in general," observes the author, "would be content with an amount of nourishment at all corresponding to the above, or even a little exceeding it, large quantities of food would certainly be saved, and both the health of many individuals and the economical circumstances of the country would be real gainers. But in this frugal and salutary mode of life man finds no pleasure; his appetite will be satisfied in a quite different and more liberal manner, and it will then depend upon many conditions, among which the nature of his work, in connection with various partly acquired, partly hereditary, individual circumstances play an essential part, how far an abundant alimentation can, in the long run, be consistent with health, strength, and well-being."—P. 15.

For due nutrition with food consisting chiefly of meal or potatoes, it is necessary that the system should, in the first place, be supplied with sufficient tissue-forming matter. If the food used does not contain enough of the latter, it must either be

supplemented with other foods, or be given in greater quantity. It may thus happen that the amount of starch may exceed what is required. The author adds a table, from the German chemist above named, of the relative proportion of tissue-forming and respiratory constituents in the most important vegetable aliments :—

		Tissue-forming matter.	Respiratory matter, &c.
100 lb. of	Potatoes contain of	2 lb.	10 lb.
„	Wheaten bread	8 „	30 „
„	Meat (not fat ?)	20 „	16 „
„	Peas	27 „	50 „
„	Beans	20 „	40 „

Supposing a person to live chiefly on potatoes, he would require to consume daily 14·3 lbs. to obtain 4½ ounces of gluten or tissue-forming material necessary for the metamorphosis of matter. This quantity would contain more than double the amount of starch necessary, according to the Scotch and German investigators, and about the quantity indicated by Playfair in his third table, referring to a greater amount of work.

The author alludes to the modes of preparing our farinaceous food, and to the change of starch into "leiocome" or "dextrin" effected by a heat of about 410° F., especially when aided by the action of yeast, and effected also by very long-continued boiling. This change, by bringing the starch nearer to the state of sugar, renders it more easily digestible: nevertheless, Professor Faye seems to be of opinion that the "aërated" bread latterly prepared in England by the direct addition of carbonic acid, in which the change alluded to takes place probably in a much less degree than in bread baked in the ordinary method, is not the less economical on that account. The same remark applies to bread in which bicarbonate of soda is used as the aërating agent, by which the gluten is at the same time acted on; or where hydrochloric acid is employed, whereby the starch is rapidly changed into dextrin. It is not to be supposed that in the many different forms in which farinaceous food is ordinarily prepared, in several of which the meal is little better than uncooked, all should be lost whose starch is not fully hydrated, or has not undergone complete fermentation. In the so-called "aërated" bread, a portion of the meal, which in ordinary baking is lost by some of its starch being converted into alcohol, is saved by the addition of carbonic acid.

After some further preliminary remarks, the author proceeds to detail the results of his own experiments, undertaken with a view to ascertain the changes which our most ordinary farina-

ceous aliments undergo in their culinary preparation, and the utilisation by the organism of the foods when prepared.

The object of the first series of experiments was to determine, with the aid of Professor E. Winge, whether the starch contained in the flour prepared for use had passed into a form in which the cells had lost their peculiar appearance under the microscope, while, at the same time, the presence of the starch as a chemical body continued demonstrable by iodine.

		With Tincture of Iodine.
1. Fine sweet biscuit exhibited under the microscope	Indistinct starch granules.	Characteristic reaction.
2. Wheaten bread (French roll)	„ „	„
3. Two kinds of rye bread, of finely sifted meal	No distinct granules.	„
4. Unleavened bread of wheaten meal	Starch granules in a distinct form.	„
5. Coarse solid rye bread	Distinct unaltered granules.	„
6. Boiled potatoes	Many distinct granules.	„
7. „ turnips	Less distinct granules.	„
8. „ beans	As in potatoes.	„
9. „ peas	„ „	„
10. Well-boiled sago gruel	Indistinctly contoured granules.	„
11. „ thin barley groats	Many distinct granules.	„
12. Barley gruel	Outline of granules	„
13. Well-boiled barley stirabout	Distinct granules.	„
14. Boiled rice	„ „	„
15. Barley groats with lumps	Unaltered granules.	„
16. Thin pancakes	Form of the granules perceptible.	„

It is evident that the chemical nature of the starch is not altered by a boiling heat, as the intensely blue colour invariably shows itself on the addition of iodine in all kinds of farinaceous food [after they have cooled?], no matter how they may have been prepared.

The object of a second series of experiments was to ascertain whether raw starch cannot be so altered by the digestive fluids as to become physiologically available to the system. It is well known that the salivary and pancreatic secretions are the proper solvents of starch; though Professor Funke's researches have shown that even the juice secreted in the vermiform appendix of the cæcum can with ease, and without the aid of other fluids, change starch into sugar. Professor Faye's experiments were thus conducted: To a small quantity of the starchy matters to be tried, so much saliva was added in small flasks, that it could easily be shaken, and thus brought into intimate contact with the starch granules. The flasks were kept in a uniform temperature of from 104° F. to 122° F. for several days, more or less, as the nature of the material required. The results are given in a tabular form, and prove—

"That a sufficient quantity of saliva is capable also outside the body of changing starch into sugar, or, in other words, of causing it to undergo an incipient process of assimilation, without the granules having been previously in any way prepared by a higher temperature or by a boiling heat."—P. 25.

Experiments performed by the author, in consequence of a statement recently made that cellulose can by chemical means be converted into sugar, and undertaken with a view to ascertain whether the saliva has the power of producing this effect, afforded a negative result.

In order to discover whether the organic ferments are capable of altering starch so that it can fulfil its part in the process of nutrition without having been previously subjected to a temperature equal to that of boiling water, it would be necessary by direct experiments to ascertain whether starch is passed unchanged with the excrements; and if so, whether this can be considered to take place to a disproportionate amount by reason of the imperfect preparation of the meal, which often prevails among the lower orders, and partly also among the more refined classes of society. To clear up this point, the author instituted experiments upon two persons, of whom he was himself one, under a moderate daily diet consisting of both animal and vegetable food, the latter predominating, and containing potatoes, bread, and other farinaceous aliments. No starch could be discovered in the fæces.

The next experiment was intended to ascertain whether starch would be found under a diet containing unprepared meal in quantity equal to or even exceeding that in which meal is in many parts of the kingdom added to and stirred up with the porridge after it is taken from the fire; but neither in this case did the fæces exhibit the reaction of starch. Hence, and from a still more conclusive experiment upon himself, Professor Faye infers—

"Not only that raw starch can be changed during digestion, when the wants of the system are so urgent that it appropriates the altered starch as material for the current metamorphosis of matter, but that the digestive powers are sufficient to change it also in those cases where the organism is not in pressing want of an increased amount of carbo-hydrates. That a portion of the starch may, where the supply is abundant, pass away as excrementitious matter, may be considered certain, and it is probable that in excess a part is expelled unchanged; but that this does not necessarily happen, even if the meal be used in the raw state, I believe there can be no doubt."—P. 33.

The author explains the popular custom of adding raw meal to

porridge after it has been removed from the fire, by showing that food, if very easily digestible, soon leaves the stomach empty, and an empty stomach is usually accompanied with a feeling of languor. Hence experience led the peasant to prolong the process of digestion by the use of less perfectly prepared food. In some parts of the country the same object is attained in a different mode, where, observes Professor Faye,

"The cook is praised who can produce the porridge so hard that one, as it is said with something of hyperbole, may walk upon it; and in Sweden they are said to boil and use it so stiff, that it may be cut into slices or pieces, in which small fry of fish are often stuck, affording a mixed food of the same nutritive value as bread and fish."

These remarks bear so very strongly on some observations on the food of the labourer published by the late Sir Henry Marsh, of Dublin, precisely twenty years ago,[1] while his country was suffering from the dreadful famine of 1846-47, which so largely reduced its population, that we shall offer no apology for quoting largely from the essay of that distinguished physician:

"Secondly," writes Sir Henry, "I am anxious to say a few words on the importance of giving bulk and firmness to food prepared for the labourer. A soft, semi-liquid diet will maintain the life and health of children, and, in times of scarcity, will be sufficient for those adults whose occupations are sedentary, and is best suited to those who are reduced by and recovering from a wasting disease. Such persons do not stand in need of the more abundant and more substantial nutriment which is essential to those who are daily engaged in occupations exacting much muscular labour. In the preparation and distribution of food, this I believe to be an important point, and one which should be held steadily in view. For the labourer, the food should be in part solid, requiring mastication and insalivation, and not rapid of digestion: food, however nutritious, which is too quickly digested, is soon followed by a sensation of hunger and emptiness, and consequent sinking and debility. Food of this description is unsuited to the labourer—it will not maintain strength, nor will it maintain health; and if long persevered in, it will be followed by some one or other of the prevailing diseases, which result immediately from deficient, imperfect, and impoverished blood. Of this fact the labourer fed on the potato was practically, though not theoretically aware: his potato, when cooked to suit his taste and feeling, was never thoroughly boiled; he wished, to use an expression which I have often heard from the labourer himself, 'to leave a bone in it, that it might stick to him!' By this

[1] 'On the Preparation of the Food of the Labourer: in a Letter to Joshua Harvey, M.D., from Sir Henry Marsh, Bart.' Dublin, James M'Glashan, 1847, p. 6.

method of cooking, two valuable objects were attained: first, the food in the stomach required more time to undergo the process of digestion, and consequently the return of the sensation of hunger was postponed; and secondly, it required a more prolonged and more perfect mastication and insalivation. For these reasons, the addition of even non-nutritious, solid material, provided that it be not deleterious, is practically useful: it increases the bulk, imposes the necessity of more complete mastication and insalivation, protracts the digestive process, and prevents the food from passing too hastily from the stomach into the lower intestines."

This adoption of the same principle in the preparation of different kinds of food in different countries, and evidently as the result of similar personal, practical experience, apart from theory, is undoubtedly interesting; and it is gratifying to find eminent physicians, like Professor Faye and Sir Henry Marsh, in the true spirit of that philanthropy so characteristic of the medical profession, turning their attention from subjects which might be expected more directly to engross it, to questions whose main object is the welfare of the humbler classes of society. Professor Faye, after some valuable observations on luxurious living, remarks, towards the close of his essay, that,

"According to our general physiological knowledge and the result of the present investigation, it cannot with truth be asserted that it is the labouring class of society which is especially wasteful of food, or by whose inadequate system of alimentation thousands of national wealth are lost—although it certainly must be admitted that the mode of preparation might be better and more appropriate to a weak digestion,—but that this reproach may with greater reason be cast upon the luxurious and more prosperous ranks. Food and drink assist in producing in the system a superabundant state of nutrition, and it may without exaggeration be said, as has also lately been affirmed in other countries, that many stomachs might be satiated with the superfluity. A rational dietary would undoubtedly be a gain to all parties. Our ancestors had a dietetic rule, which in my youth I often heard quoted, that we should leave off with an appetite. The rule, judiciously applied, is good; but it has long since been laid aside as antiquated and unsuitable to our present system of physical education. The rule in other words would run thus: 'Little, but good;' but as such it is unfortunately seldom put in practice."—P. 40.

Review VII.

On Railway and other Injuries of the Nervous System. By John E. Erichsen, &c.

This book is but a small one; but its importance is, in our opinion, great, both in consequence of the author's position, and of the great interest which it possesses for all medical men who have to treat severe accidents, or who are called upon to give evidence about them in courts of justice. We shall, therefore, allot to Mr. Erichsen's book rather more than the amount of space which might seem proportionate to its size.

In considering Mr. Erichsen's subject, we shall approach it successively on the two aspects indicated above, viz., first as a medical, and next as a medico-legal subject. Let us, however, first define what Mr. Erichsen's subject is. From the lettering on the binding of the book, which runs thus—"Erichsen on Railway Injuries," the author has been misunderstood as intending to make a new speciality of railway injuries, and, by consequence, of "railway surgery." This misunderstanding is the more remarkable, and the less justifiable, as Mr. Erichsen has in the plainest possible terms guarded himself against such a misconstruction. To take one only out of several similar passages which occur in these lectures, the author says (on p. 9)—

"I will not confine my illustrations to cases drawn from railway accidents only, but will show you that precisely the same effects may result from other and more ordinary injuries of civil life. It must, however, be obvious to you all, that in no ordinary accident can the shock be so great as in those which occur on railways. The rapidity of the movement, the momentum of the person injured, the suddenness of its arrest, the helplessness of the sufferers, and the natural perturbation of mind that must disturb the bravest, are all circumstances that of a necessity greatly increase the severity of the resulting injury to the nervous system, and that justly cause these cases to be considered as somewhat exceptional from ordinary accidents. This has actually led some surgeons to designate that peculiar affection of the spine that is met with in these cases 'the railway spine.' But yet, though the intense shock to the system that results from these accidents naturally and necessarily gives to them a terrible interest and importance, do not for a moment suppose that these injuries are peculiar to and are solely occasioned by accidents that may occur on railways. There never was a greater error. . . . In the writings of Sir A. Cooper himself—in those of his predecessors and contemporaries, especially of Boyer, of Sir

C. Bell, and at a later period of Ollivier and Abercrombie, you will find many cases recorded that prove incontestably that precisely the same train of phenomena that of late years have led to the absurd appellation of the 'railway spine' had arisen from accidents, and had been described by surgeons of the first rank in this country and France, a quarter of a century or more before the first railway was opened."

In fact, Mr. Erichsen's subject treats of the immediate and remote effects of extensive injury to the nervous system, mainly to the spine, but incidentally and secondarily to the brain and the nerves. Such injuries have become out of all proportion more common, more extensive and perplexing, and of far greater public interest and importance, since the invention of travelling by railway. Previous to this, though it is true, as Mr. Erichsen has said, that they were occasionally seen, yet it was so rarely that they were rather looked upon as curiosities; and a medical man who did not profess a special surgical experience might easily afford to admit that he was not familiar with the subject, and saw no reason why he should be. Now, however, they have been brought literally to every man's door; and no one who practises surgery at all can tell but that to-morrow he may be called on to diagnose and treat such a case, with the attendant responsibility of explaining his views on its pathology and prognosis under the ordeal of cross-examination in a court of justice. Yet nobody would assert that either the pathology or the prognosis of such cases is at all clearly understood by practitioners in general. Hence we cannot but think that Mr. Erichsen is doing a great benefit to the profession in writing on the subject, provided he writes clearly and well.

Having said so much about the motive of the work, we will proceed to discuss its subject in the medical point of view. That subject, as we have shown above, is mainly concerned with the extensive and lasting injury to the contents of the spinal canal usually contemplated under the vague term "concussion of the spine," though it is not limited to such injury. Under this term Mr. Erichsen believes four distinct pathological conditions to be included, viz.:

"1. A jar or shake of the cord, disordering, to a greater or less degree, its functions, without any obvious lesion cognisable to the unaided eye; 2. Compression of the cord from extravasated blood; 3. Compression of the cord from inflammatory exudation within the spinal canal, whether of serum, lymph, or pus; and, 4. Chronic alterations of the structure of the cord itself, as the result of impairment of nutrition consequent on the occurrence of one or the other of the preceding pathological states, but chiefly of the third."

And the causes he divides into—slight and apparently trivial injuries inflicted upon the spine; injuries of distant parts of the body, or shocks of the system, unattended by any direct blow on the back; and, finally, wrenches and twists of the spine (pp. 18, 19). Examples of all these causes and of all these post-mortem conditions of the cord will be found detailed or referred to—some in Mr. Erichsen's own practice, and others from well-known writers; Mr. Erichsen having wisely avoided the silly affectation of only giving the results of his own observation, and thus magnifying his experience at the expense of leaving his subject incomplete.

This account of the causes and pathological changes in spinal concussion is followed by an unusually clear and interesting description of its symptoms and progress, which is certainly the most original and the most valuable part of Mr. Erichsen's work, and bears unmistakable evidence of being the production of a practitioner of large experience, not a compilation from the writings of others. It is hardly possible to present a useful abstract or abbreviation of this description; but we may, perhaps, advantageously notice some of the main points of it.

In the first place, Mr. Erichsen brings out prominently this fact—that in these cases of lesion of the spinal cord, the force has sometimes been considerable, and applied directly to the spinal region; in others, slight, and not apparently affecting the back; further, that the symptoms have sometimes come on instantaneously, at others shortly after the accident, and at others, again, not until after an interval of health: but that in all cases they have this feature in common—that the symptoms, when once established, although they may show remissions, never admit of any complete and perfect intermission; that there is never an interval of complete restoration to health (p. 111). The essence of the disease being, according to Mr. Erichsen, inflammatory, and depending on the development of chronic inflammation with its sequelæ, either in the medulla spinalis or its membranes, or both, Mr. Erichsen shows, by references to the works of Abercrombie and Ollivier, that the symptoms which he sketches from the history of cases of "railway concussion" under his own care are exactly the same in kind as those which they have described as accompanying chronic myelitis or spinal meningitis from other causes (pp. 118 *et seqq.*). Had Mr. Erichsen wished to answer by anticipation the captious objection that his book led to the "specialisation" of railway surgery, he could hardly have done so more completely than in these pages, which contain besides a most useful *résumé* of the lengthened account which precedes them, tracing clearly to their pathological origin—(1) The

cerebral symptoms, headache, confusion of thought, loss of memory, disturbance of the organs of sense, irritability of the eyes and ears, &c., which are referable to cerebral arachnitis, developed by continuity from the inflamed spinal membranes; (2) The pain at one or more points of the spine, greatly increased on pressure, and on movements of any kind, so as to occasion extreme rigidity of the vertebral column, and usually accompanied by muscular rigidity, which Mr. Erichsen seems disposed to regard, with Ollivier, as indicative rather of inflammation of the membranes of the cord than of the medulla itself; and (3) The painful sensations along the course of the nerves, followed by more or less numbness, tingling, and creeping—some loss of power, affecting one or more of the limbs, and giving rise to peculiarity and unsteadiness of gait, but unaccompanied by paralysis of the sphincters, which prove, according to their degree, that the membranes and more or less of the substance of the cord is undergoing degeneration.

"If," says Mr. Erichsen, "we take any one symptom that enters into the composition of these various groups, we shall find that it is more or less common to various forms of disease of the nervous system. But if we compare the groups of symptoms that have just been detailed, their progressive development and indefinite continuance, with those which are described by Ollivier and other writers of acknowledged authority on diseases of the nervous system as characteristic of spinal meningitis and myelitis, we shall find that they closely correspond with one another in every particular—so closely, indeed, as to leave no doubt that the whole train of nervous phenomena arising from shakes and jars of or blows on the body, and described at pp. 96 to 110 as characteristic of so-called 'concussion of the spine,' are in reality due to chronic inflammation of the spinal membranes and cord."—P. 122.

It is to be regretted that Mr. Erichsen's opportunities have not allowed him to bring forward more ample proof of his views of the pathology of this affection from recent and full post-mortem examinations. With the exception of a short, and we must say an imperfect, reference to a post-mortem examination in a case of concussion of the spine by railway accident, published by Mr. L. Clarke in the Pathological Society's 'Transactions,' there is not, as far as we can see, any account of the minute anatomy of the cord in these cases. This is the more to be regretted, as there can be little doubt that the term "concussion" of the spine, as of the brain, is really a veil for ignorance—that there is no such thing as a simple shaking-up of the nervous matter, but that mechanical injury followed by mechanical alteration of structure is at the root of the symptoms

in both cases. With respect to the treatment also, Mr. Erichsen's volume is a little disappointing. He recommends absolute rest both of the spine and of the brain, the prone position, counter-irritation, and the administration of the bichloride of mercury during the early period of the disease. Later on, the preparations of nux vomica or strychnia and of iron will, he says, be advisable in appropriate cases; and he tries to distinguish those in which strychnia may be expected to be of service. But it is obvious that he does not expect much permanent benefit from treatment in cases where the symptoms are at all severe and confirmed.

The questions of diagnosis and prognosis are so intimately associated with the medico-legal aspect of these cases, that we have left them for the second branch of the subject, which refers to the conduct of medical men in courts of justice in railway and other such cases. It would be idle to deny that much scandal has arisen, and much blame has been cast upon surgeons, from the discrepancy of their opinions in such cases. Mr. Erichsen attempts to get us out of this trouble by a defence, which will be found on pp. 4 *et seqq.*, and which amounts to this—that these cases differ from those of obvious external injury, such as fracture, which are questions of fact, and partake more or less of the nature of matters of opinion—that "the symptoms come on slowly and gradually, and may possibly be referable to other constitutional states, quite irrespective of the alleged injury—that the result is necessarily more doubtful, being dependent on many modifying circumstances," and that, therefore, discrepancies of professional opinion must necessarily exist. He then goes on to instance, and to urge in extenuation of the differences of the doctors, the discrepancy of opinion which is found in all other professions—among lawyers, divines, engineers, chemists, &c. But, with all submission to Mr. Erichsen, we cannot help thinking that he has mixed up two different matters in the above passage—the one referring to diagnosis, the other to prognosis. Questions of prognosis must always be doubtful, and no reasonable man would consider it worthy of surprise or blame to hear two men of equal professional ability and experience expressing different views as to the probability of future events; but this does not apply to matters of diagnosis. Whether the patient is suffering from concussion (so-called) of the spine, and whether there is any probable cause for the affection in the patient's history (detailed in court) beyond the injury which he is sworn to have suffered, are questions on which we cannot help thinking that constant and irreconcilable difference of opinion is discreditable to the members of our profession. Similar discrepancy in matters relating

to other professions involves, in our view, similar discredit to them, but does not relieve us from any of ours. We trust the publication of Mr. Erichsen's work will go, at any rate, some way to obviate this state of things in future. In forming a judgment on this subject, the question divides itself naturally into the two topics just stated, viz., Is the patient suffering from spinal concussion, and is that state the consequence of the alleged injury? To the former question Mr. Erichsen supplies, we think, very full and satisfactory means of reply. The diagnosis is to be made, according to Mr. Erichsen, from cerebral concussion, from rheumatism, and from hysteria. But ought it not to be added, from imposture? We pass over the first, because it is of little moment in an action for damages whether the head or the back is believed to be the part most injured, so long as it is agreed that the plaintiff is suffering from injury inflicted by the defendant. From rheumatism and from hysteria such cases may be distinguished, the diagnosis from the former being made by "the slow but gradually progressive character of the symptoms of spinal concussion, the absence of all fixed pain except at one or more points in the back, the cerebral complication—the gradual occurrence of loss of sensibility, of tinglings and formication—the slow supervention of impairment or loss of motor power in certain sets of muscles," and by observing that "in spinal concussion there is never any concomitant articular inflammation, and that although the urine may continue acid, it does not usually present evidence of a superabundance of lithates" (p. 125). As to hysteria, it appears extraordinary, as Mr. Erichsen observes, that any confusion can be made between a disease so fitful and irregular as hysteria and one so unvarying as spinal concussion; but the real fact, as it seems to us, is, that the surgeons who use the word "hysteria" in these cases really mean "imposture." At any rate, the question which so often occupies the court, and which medical men are so often divided about, is whether the plaintiff has really suffered a grave and probably life-long injury in the accident in question, or is making a pretext of the accident to obtain compensation for injuries which have no real existence. Mr. Erichsen ought, we think, to have discussed this question more explicitly than he has done, though we are ready to allow that his book contains by implication all that is necessary for its decision in his description of the symptoms of real concussion. It seems, then, from Mr. Erichsen's description, that not only ought the patient who is to be pronounced as suffering from real spinal mischief to exhibit a change from his former active habits to an alleged incapacity for business, and from the ordinary mental condition to one which presents more or less analogy to that of hysteria;

not only ought he to describe to his medical attendants various subjective phenomena, referable to the special senses, and allege certain deteriorations in the power of motion, or of bearing fatigue, or of sensation, or of the sexual appetite—for all of which the medical man must trust to his unsupported assertion —but there ought also to be certain objective symptoms caused by the paralysis of separate groups of muscles, and by the affection of the parts within the spine—perceptible stiffness of the back, unequal response of the different muscles to the galvanic stimulus, loss of bulk of the affected muscles, loss of weight of the whole body. Above all, these symptoms should be continuous and progressive. We are convinced that a careful study of Mr. Erichsen's volume would go far to preserve medical witnesses from the discredit of swearing to their belief in the permanent crippling of plaintiffs who, in a few days after pocketing their damages, are found running foot-races or scrambling over dizzy cliff-walks. As to prognosis, we have less to say; Mr. Erichsen gives several valuable hints about it, but our limits have been already exceeded, and we must not indulge in further quotations. Prognosis is a matter which must always remain doubtful, and on which, as we said before, a difference of opinion will always be regarded as natural.

The other question, which is also one of diagnosis, is as to the connection between the injury and the symptoms. On this point Mr. Erichsen might perhaps have been more explicit. We should have been glad to know, especially, what is the longest period of health which he has ever known to intervene, in an undoubted case of spinal affection from injury, between the receipt of the injury and the first occurrence of symptoms which attracted attention, and whether even during this period careful inquiry could not elicit some slighter symptoms that might serve as a connecting link.

But we must draw this notice to a conclusion. We hope Mr. Erichsen's book will reach a second edition, and that he may then be in a condition to give us still more ample information on this very interesting and important subject. The future progress of the cases, after the trial is over, when no further motive exists for imposture or exaggeration, is a point of great importance, on which more extended experience might throw light; and this, with the other desiderata which we have specified, may be added in a new edition. But the book as it stands seems to us of great value, and its careful perusal would be highly advisable to any gentleman who finds himself engaged in any case of the kind treated of by the author. Clear ideas of diagnosis and careful investigation of symptoms would save us from many, though perhaps not all, of the exhibitions of

divergent opinion in court, which, for all that Mr. Erichsen can say, will be looked upon by the public as disgraceful to the medical profession, and, in our opinion, are justly so regarded.

Review VIII.

Clinical Memoirs on the Diseases of Women. By M. Gustave Bernutz, and M. Ernest Goupil. In Two Volumes. Translated and Edited by Alfred Meadows, M.D.

The Sydenham Society has now added to its translations of the works of foreign authors an abridgment, in two volumes, of the 'Clinical Memoirs on the Diseases of Women' published by MM. Bernutz and Goupil.

The original work has hitherto been but cursorily noticed by English systematic writers on these subjects: it may, therefore, be worth while to examine more closely the opinions put forward therein. On the present occasion we propose to notice the first volume only, leaving our review of the second one until our October number.

The first volume contains separate clinical essays on Menstrual Retention, Peri-uterine Hæmatocele, Intra-pelvic Hæmorrhages occurring in extra-uterine pregnancies. The second volume contains essays on Pelvi-peritonitis and Uterine Deviations.

The authors declare themselves separately responsible for their own essays. Nos. I and II in the first volume, and No. I in the second, are written by M. Bernutz, and the others by M. Goupil.

The same plan is adopted by both. A case is related in full detail; the past history, present symptoms, and the appearances at the autopsy are carefully noted, and conclusions as to pathology and therapeutics drawn from a comparison of the ante- and post-mortem observations.

This mode may seem somewhat prolix, and to involve repetition; but it has the immense advantage of putting facts before the student in the place of the dogmatic assertion of the text-book.

In the original, the cases are most elaborately taken, and will serve as models; and we regret that the translator, who has very ably performed his task of abridging and condensing the diffuse style of the text, especially that of M. Bernutz, has cut down the cases within a somewhat small compass.

M. Bernutz commences his essay on "Menstrual Retention"

with some general observations on dysmenorrhœa; he considers that "the state of congestion is secondary to deficient excretion, and ought to be attributed to the action of the retained fluid and the reaction of the organs which expel it," and that this same cause originates "diseases which have usually been regarded as idiopathic, such as metritis, phlegmons of the broad ligaments, ovaritis peritonitis."

He takes for granted that blood is effused into the uterine cavity, and refers all the symptoms to its non-escape *per vias naturales*. The obstacle may be impermeability of the vulva or vagina, an abnormal condition of the cervix or body of the uterus or of the tubes, showing an apparent analogy between menstrual and urinary retention.

The causes of menstrual retention M. Bernutz divides into eight classes:

1. Congenital or acquired imperforation of the vulvo-uterine canal before puberty.
2. Cicatrices, rendering the passage impermeable after the age of puberty.
3. Congenital contractions or cicatrices of the vagina or cervix.
4. Increased volume of that organ, whatever may have been the cause.
5. Temporary retention by the interposition of adventitious deposits either in the cavity or in the body of the uterus; polypi, pseudo-membranous dysmenorrhœa, &c.
6. Uterine deviations, especially flexions.
7. Spasmodic contraction of the cervix.
8. An abnormal condition of the Fallopian tubes.

In the illustrating cases, M. Bernutz shows that one train of events is common to all, viz., distension of the uterine cavity and tubes by the blood, and its actual or impending effusion into the pelvis.

His remarks on the first class, as to the fatality of the apparently simple operation of incising the hymen, coincide with the general experience. One successful case is detailed, and four in which peritonitis followed. The operation is a necessity, but a small gradual evacuation is to be preferred, as "when the uterus is emptied too quickly, we are apt to get a simultaneous contraction of the Fallopian tubes."

When the vagina is congenitally obliterated, Amussat's plan of making a way through the compact tissue by a tearing rather than a cutting operation, using the finger and tents rather than the knife, is illustrated by Case IV, where the bladder was opened by the knife, by Case XLIX, where congenital absence of vagina existed, and symptoms of menstrual reten-

tion and gradual enlargement of abdomen began at the age of seventeen, a genito-rectal fistula then formed, and blood passed for six years: on the stoppage of this canal, Amussat's operation was performed, and for two years blood passed per vaginam, but then re-appeared per anum: nothing further was done. In his second class Bernutz gives a case, No. XI, of occlusion of the "os tincæ" after cauterization; a case, No. XII, of amputation of the cervix, which cicatrized rapidly, but in three years' time cicatricial contraction obliterated the canal, and caused menstrual retention, peritonitis, and death; a case, No. XIII, of cervical obliteration following labour, when the head remained three days in the cervix uteri; here menstrual retention occurred for eight months: on examination, a cicatrix, instead of the os, was found; this was successfully punctured with a trocar.

In Case XIV, gangrene of the vagina took place in typhoid fever; death resulted from peritonitis following a puncture made through the vaginal wall into the uterus to relieve menstrual retention, the cervix being obliterated. Blood was not effused into the cavity of the abdomen, but passed through the artificial opening.

M. Bernutz thinks that cicatrices of the vagina rarely, if ever, follow venereal disease; but obliteration may result from dartrous ulceration, and especially from chronic eczema. "Congenital narrowness of the cervical canal does not, according to M. Bernutz's experience, of itself interfere with menstrual excretion. To disturb this function, some morbid phenomenon must be added to the atresia." Two cases, Nos. XVII and XVIII, where Dr. Oldham performed division of the cervix, in one of which pregnancy followed, and in the other only partial relief, are detailed; while four patients are mentioned in whom the cervical canal was so extremely narrow as only to admit a fine stylet, the excretion, however, was easy; in two pain was felt at the menstrual period as the result of peri-uterine inflammation: in one there was no pain, in the other the pain was accompanied by the expulsion of clots. "I," says M. Bernutz, "have never had occasion to operate for it, but I should prefer division by the hysterotome to dilatation with bougies." To establish his fourth class, Cases XIX, XX, XXI are detailed, in which dysmenorrhœa with fulness in the iliac region and dilatation of the uterus apparently resulted from hypertrophy of the cervix. In the first two cases, relief followed on escape of blood and clots from the uterus, rest in bed, with the application of poultices and leeches to the abdomen, and baths. In the third case, the cervix was considered to be affected with cancer. M. Bernutz thinks that the greater number of cases of cervical enlargement are the

expression of a diathetic condition, and all treatment should be subordinate to this end. We must carefully trace out the relation existing between the antecedent history and actual engorgement of the cervix. Amongst young women scrofula is the more common cause. Local means should be resorted to to relieve the uterine catarrh; general antiphlogistic treatment with leeches, &c., is recommended during the actual dysmenorrhœa.

Our English text-books and teachers hold somewhat different opinions. Dr. West[1] speaks of three forms of dysmenorrhœa—neuralgic, congestive, mechanical. The purely mechanical, requiring division or dilatation of the cervix, he regards as excessively rare:

"At the same time, just as when stricture of the urethra exists, the bladder, ureters, and kidneys become irritated and disturbed in the performance of their functions, so it is quite conceivable that a state of the cervix uteri may exert the same influence on the function of that organ, and render the menstrual flux scanty in quantity and morbid in character, as the consequence of the difficulty in its discharge. The natural constriction of the internal uterine orifice is probably often heightened by functional disorder, and thus becomes the occasion of a great increase of the patient's sufferings."

Dr. Churchill[2] holds somewhat similar views with Dr. West, but lays more stress on the congestive form "which approaches inflammation." He says,

"That Drs. Simpson, Protheroe Smith, Mr. Whitehead, Dr. Oldham, regard a narrow state of the cervical orifice and canal as a cause of dysmenorrhœa. That it is so in some cases cannot, I think, be denied; but I cannot agree with those who regard the occurrence as the most frequent, or as being in all cases, when present, the sole cause of the dysmenorrhœa."

Dr. Barnes[3] read a paper before the Obstetrical Society of London, June 7th, 1865,[4] on "Dysmenorrhœa dependent on a peculiar formation of the cervix uteri, and the treatment by dilatation or division." He says—

"If a woman has suffered long from painful menstruation and metrorrhagia, and has been married some years without pregnancy, it may be predicated with great confidence that the neck of the uterus projects in an abnormal degree into the vagina, that its form is

[1] 'On the Diseases of Women,' p. 75, &c., by Dr. West, 3rd ed., 1864.

[2] 'On the Diseases of Women,' pp. 208, 218, 5th ed., 1864; Fleetwood Churchill, M.D., Prof. of Midwifery, &c., King and Queen's College of Physicians, Ireland.

[3] Robert Barnes, M.D., Lecturer on Midwifery, &c., St. Thomas's Hospital; Examiner in Midwifery, Royal College of Physicians, &c. &c.

[4] 'Transactions of the Obstetrical Society of London,' vol. vii, 1866.

conical; that the os *in*ternum (? os *ex*ternum) is a small round orifice barely admitting the uterine sound; and that probably there is deviation of the cervix backwards or forwards, or to one side. The seat of the obstruction I believe to be almost invariably at the os uteri externum. Menstrual retention is the consequence; and following this may be metrorrhagia or menorrhagia, and hæmatocele in the vagina, while sterility, endometritis, &c., follow marriage."

The treatment is division of the cervix. Dr. Greenhalgh,[1] in commenting upon the paper, agreed as to the frequent necessity of surgical interference, but placed the seat of obstruction at the os internum.

Dr. Graily Hewitt[2] believed that in bad cases of dysmenorrhœa the condition present was frequently retention of the fluid in the uterus, and that this retention caused the pain: he thought, however, that in most cases relief could be given without resorting to mechanical treatment of the os uteri.

M. Bernutz considers the exciting cause of dysmenorrhœa to be a morbid state of the uterine and peri-uterine tissues—an unhealthy diathesis frequently co-existing, and the immediate cause a swollen condition of the mucous membrane of the uterus and cervix, often exaggerated by hypertrophy of the latter, by which egress of the menstrual fluid is prevented; the mechanical pressure causes reaction on the genital organs, and so congestion, pain, and other accidents. Dr. West lays the chief stress on an ill-defined condition under the name of neuralgia, and places the congestion of the cervix and uterus prior to the diseased menstruation. Dr. Churchill puts congestion and inflammation in the first place, and the remaining symptoms as consequences; while the surgical school looks upon the strictured state of the cervix, found only on examination, and after the symptoms of pain and other incidents have been long established, as the *fons et origo mali*.

Uterine polypi and pseudo-membranous dysmenorrhœa may be causes of menstrual retention. Case XXIII is detailed, where dysmenorrhœa and leucorrhœa existed, and for which a fibrous polypus was detached by dissection, the uterus being drawn down and the cervix divided: peritonitis followed the operation. Cases XXIV, XXV, XXVI, and XXVII are given as instances of pseudo-membranous dysmenorrhœa. Case XXVII was published by Dr. Oldham in the 'London Medical Gazette,' 1846, vol. ii, p. 970. M. Bernutz differs altogether from Dr. Oldham as to the pathology.

"I cannot regard the *ovarian influence* as constituting a disease, nor the *detached* membrane as the cause of the dysmenorrhœa,

[1] Lecturer on Diseases of Women, St. Bartholomew's Hospital.
[2] Lecturer on Diseases of Women, St. Mary's Hospital.

The real obstruction is to be found in the morbid condition of the *attached* uterine mucous membrane, especially the cervico-uterine portion of it; for the microscopical characters of the tissue do not exclude the possibility of inflammatory action, and we may fairly compare this affection with that of retention of urine from inflammation of the neck of the bladder. It is not, therefore, a specific affection."

Spasmodic contraction of the cervical canal gives no trouble, unless another morbid condition arises in consequence, or the spasm is associated with hypertrophy or catarrh. Cases XXVIII, XXIX, XXX, are detailed, where menstruation was stopped either by cold lavation or mental emotion, but the dysmenorrhœal symptoms did not come on until after a lapse of time, when, the uterus failing to get quit of the menstrual fluid, repletion of the organ produced congestion, inflammation, &c.

The eighth class—menstrual retention from abnormal conditions of the Fallopian tubes—is hardly recognisable during life. Three cases illustrate it. The autopsies revealed closure of the uterine ends of the Fallopian tubes, XXXI congenital, XXXII by a fibrous tumour, XXXIII by inflammation. Blood-cysts formed. In the first case, death was caused by gangrene of the cyst, resulting from puncture with a trocar; in the second, by internal hæmorrhage from a ruptured Fallopian tube; the third patient died of pleuro-pneumonia, and both the Fallopian tubes were found dilated to the diameter of an inch, and contained a brown, viscid, inodorous fluid.

We see, then, that an obstacle to menstrual elimination in any part of the canal of the tube or uterus leads first to dilatation, then to distension of every part of the canal posterior to the seat of obstruction, and, when the repletion is varied to an extreme point, to the effusion of the retained blood into the abdominal cavity.

Hysterical dysmenorrhœa is not a local disease; the functions of the genitalia are merely disordered, like other functions, and can only be set right by the cure of the general disease.

In the second chapter, on 'Symptomatology,' M. Burnutz discusses retro-uterine hæmotocele consequent on menstrual retention. He traces the steps to the intra-peritoneal effusion, and the inflammation set up around the blood with the resolution. The constitutional symptoms, which are slight in these effusions, serve as a point of diagnosis from the intra-peritoneal affections of a hæmorrhagic diathesis when anæmia, &c., is marked. The prognosis is favorable. The characteristics of this form of hæmatocele are—1. The instantaneousness of its development, coincident with a scanty catamenial period. 2. The independence of the uterus which it displaces by its

immediate contiguity. 3. The difference felt in its consistence at different epochs. 4. The periodical aggravation of the symptoms and bulk of the tumour.

The physical signs must be traced by bimanual examination. A tumour rises into the abdomen, pressing the uterus against the pubes, and often assumes a trefoil shape. The development of the middle lobe much beyond the lateral ones is peculiar to hæmorrhagic hæmatoceles. A sulcus divides the posterior wall of the uterus from the tumour, which feels as though moulded by the bony walls. The tumour may open spontaneously either into the rectum or vagina, and rapid improvement generally follows: sometimes long-continued suppuration will induce hectic. M. Bernutz repudiates purely expectant treatment, equally with the puncturing of hæmatoceles.

The third chapter is devoted to the diagnosis between menstrual retention without resulting intra-pelvic effusion, and pure pelvi-peritonitis, between menstrual retention, pregnancy, uterine retroversion, and swelling of the broad ligaments. Retention has been mistaken for cancer, fibrous tumour, ovarian dropsy, purulent and hydatid pelvic cysts.

The first point in treatment is to look to the constitutional condition of the patient. Operations for atresia should be performed as early as possible, and when the generative organs are the least active. A small trocar and very gradual evacuation of the retained fluid should be practised in imperforate hymen. Emmenagogues must not be used when the state of the cervix is an obstacle to the free egress of the menstrual flux. Cervical enlargement is often strumous. If peritonitis occurs, there should be no delay in the treatment. Over-depletion does harm. Vomiting must be controlled by abstinence from drink—by the use of iced beverages, belladonna, opium, strychnia.

The objection to puncturing hæmatoceles is twofold: 1st, because of the serious results of the admission of air; and, 2nd, because the effusion is intra-peritoneal. But when suppuration has set up, we should puncture and maintain the fistulous opening.

M. Bernutz commences his essay on peri-uterine hæmatocele by considering the utero-rectal peritoneal sac as homologous with the male tunica vaginalis, and then limits the term to effusions therein from the generative track. Hæmatoceles may be ranged in three classes: 1. From rupture of a blood-vessel, or of one of the internal genital organs; 2. From defect in the excretion of the menses; 3. From a morbidly profuse exhalation of blood from the genital organs. Of hæmatocele symptomatic of rupture of utero-tubar varices, M. Bernutz can only find two recorded cases in which the autopsies revealed this lesion in non-pregnant women. A patient came under his own

notice who recovered, but he considered his diagnosis justified by the collateral signs; these were the characteristic signs of a post-uterine blood-tumour, regular but excessive menstruation, a general varicose condition of the lower extremities and vagina, a varicose ulcer of the cervix uteri. In all three cases the patients had been mothers, and the rupture resulted from fatigue. [Of hæmatocele from hæmorrhagic pelvi-peritonitis, M. Bernutz knows of no proved case, but does not deny their existence, as blood exhalations take place from analogous serous membranes.] Hæmatoceles symptomatic of rupture of the ovary or of the Fallopian tubes are rare, but some autopsies are reported. They result from some chronic error of nutrition, the rupture taking place under the influence of a rush of blood. The diagnosis is obscure. The symptoms are those of internal hæmorrhage and of peritonitis, while it is differentiated from metrorrhagic hæmatocele by the absence of hæmorrhage per vaginam, from that of menstrual retention by the absence of dysmenorrhœa, from rupture of varix, or of the cyst in extra-uterine pregnancy by the correlative signs. The treatment is to stop the hæmorrhage by iced compress to the abdomen, to support life. Later on we can attend to the peritonitis, but bleeding is here absolutely prohibited, and the tonic treatment must be persevered in. The theory that the hæmorrhage is due to rupture of a Graafian vesicle, M. Bernutz considers insufficient to account for the loss of blood, but suggests that the aberrant ovule which escapes into the abdominal cavity may become a small serous cyst, such as are often dependent from the pavilion of the Fallopian tube; but he seems to have mistaken for these the so-called hydatids constructed out of the remains of the Wolffian body and end of Müller's duct.[1] Hæmatocele from menstrual retention has been already discussed in the first essay. M. Bernutz urges the necessity for an accurate inquiry into the patient's antecedents, if we wish to diagnose any affection of the female genital organs, and thinks that this evidence is of as much or more importance than that furnished by physical examination.

Metrorrhagic hæmatoceles are the result of a morbid discharge from all or part of the genital organs in the unimpregnated state, some of the fluid being effused accidentally into the peritoneal cavity, while the rest escapes as metrorrhagia from the vulva. They may be divided into four varieties—pyrexial, puerperal, inflammatory, cachectic. The first occurs in fever or febrile conditions, as purpura, &c., when there is a hæmorrhagic diathesis, and, though more apt to happen at a menstrual period, yet is independent of it. The third kind cannot be diagnosed from pelvi-peritonitis, but two cases—Nos. XIII and XIV—are

[1] 'Cyclop. of Anat. and Phys.,' art. "Uterus," vol. v, p. 597.

detailed where blood escaped on puncture of the retro-uterine tumour. In the first of these, the symptoms followed soon after abortion; so that it belongs rather to the class "puerperal."

The cachectic cause of the fourth group may be losses of blood or defective hygiene, chlorosis, &c. The accompanying peritonitis is usually very slight and subacute in its character; so slight, indeed, that its existence has been denied by M. Trousseau: but without the false membrane formed by the inflammation there is no encystment, and, therefore, no tumour can be felt. The diagnosis is often very difficult. The anæmia may be due to external hæmorrhage, or to the effect of simple peritonitis. The deposit may be confounded with fibrous tumours. The treatment should be a rational one: no bleeding; opiates, poultices, injections, tonics, and absolute rest. Puncturing à la Nélaton is condemned.

Hæmorrhage within the pelvis may occur in extra-uterine pregnancy either with or without rupture of the fœtal cyst. M. Goupil has classified forty-two cases:—Hæmorrhage caused by rupture—1, of dilated utero-ovarian veins; 2, of the ovary; 3, of the Fallopian tube; 4, of the fœtal cyst; 5, hæmorrhage into the fœtal cyst, but not into the pelvis. The general symptoms are the ordinary signs of pregnancy, especially the suppression of the menses; some metrorrhagia, enlargement of the uterus, a peri-uterine tumour, the signs of internal hæmorrhage. The suspicion of pregnancy is often held by the patient even in opposition to the physician; a painless discharge of blood is sometimes the first symptom that calls attention to the state of things, and is, therefore, of great importance. The enlargement of the uterus, though frequently spoken to by all authors, yet in the cases here brought forward is by no means constant. In Case 1 it is said to be only a little enlarged, the walls pale, no trace of a decidua, but red-coloured mucus filled the cavity. In Cases 2 and 13, the uterus was normal. In Cases 3, 5, 7, the condition is not mentioned; Cases 6, 14, the uterus was larger than normal; Case 8, it was much thickened, and had increased *pari passu* with the fœtus; Cases 10, 11, the interior of the uterus had all the appearance of the gravid state. We see, therefore, that in only three cases—8, 10, 11—did the uterus present the appearance of gestation. In all the others, except No. 4, when a fœtus had been developed within the uterus as well as without it, the uterus was either normal or only somewhat larger; and in three cases its state is not mentioned. The rupture of the Fallopian tube may be caused by the development of the fœtal cyst, or hæmorrhage within the tube may distend it to rupture. The death is due to the hæmorrhage, which often seems quite disproportioned to the amount of lesion.

The rupture of the fœtal cyst is far more frequent in tubal pregnancies than in the other kinds, the difference being due to the amount of distensibility of the tissues.

M. Goupil thinks that intestinal tubal pregnancies terminate by rupture very early; but Dr. Farre[1] supposes that the thickness of the surrounded tissues is a protection, and that they are likely to allow of the fœtus to develop to a much greater extent than in other kinds of extra-uterine pregnancies. Professor Schultze, of Jena, relates a case[2] where the fœtus was thirty weeks old, no rupture took place, but fistulæ formed by ulceration into the rectum and bladder; the abdominal wall also opened by abscess, through which the fœtal bones were removed. The patient recovered her health perfectly. There are two symptoms premonitory of the rupture—pain and syncope; the latter is often repeated.

The prognosis, if hæmorrhage takes place, is invariably fatal; but if the fœtus die before this occurs, many extra-uterine pregnancies terminate happily. M. Goupil thinks that surgical intervention is hardly warrantable; but Frerichs has published a case in 'Virchow's Archiv,' xxix, 3 and 4, where the growth of the fœtus was arrested by injections of morphia through the vaginal wall. Dr. Greenhalgh, in the 'Lancet,' April, 1867, reports a case where he passed a hair trocar into the cyst and drew off the liquor amnii: the patient did well after the operation.

Galvanism is not unlikely to be of service, and gastrotomy may be sometimes legitimate.

When intra-peritoneal hæmorrhage has taken place without death speedily following, we must keep the patient at rest, and try to ward off death by food, stimulants, and ice. In other respects, an expectant treatment is the only course left us.

As before said, we purpose noticing the second volume of Bernutz and Goupil's work in our following number.

Review IX.

Official Report of the Committee appointed to inquire into the Origin and Spread of the Yellow Fever in Bermuda in 1864. Ordered by the House of Commons to be printed. Folio, pp. 112. 1866.

All such reports as the one now before us are pretty sure to be more or less instructive as well as interesting; for whatever may be the ability with which they have been prepared, or the

[1] 'Cyclop. Anat. and Phys.,' v, p. 621.
[2] 'Zeitschrift f. Medicin,' etc., t. iii, 1864.

soundness of all the conclusions arrived at, a number of authentic data relating to the epidemic in question are made public; and these data cannot fail, when carefully investigated, to throw some light on the history of the disease. The present is not the first record of the sort which has, of recent years, been published in respect of visitations of this pestilential scourge in the Bermudas, as we shall have in the sequel occasion to note.

This numerous group of small islands, clustered from east to west, is in mid ocean, intermediate between the Bahamas and Nova Scotia, and nearly 600 miles distant from the American continent. Their latitude, about 32°, is the same as that of Charlestown, the capital of South Carolina. The most easterly island of the group is St. George; it is the chief resort of the mercantile shipping, and there too the principal military forts are situated, and a large portion of the troops are situated. Ireland Island, the most westerly, is the seat of the dockyard and naval establishment. Hamilton, the chief town, where the governor resides, is on the central or main island. There is no highland in any of the islands; their general elevation above the sea-level is seldom greater than between 20 and 30 feet; the highest point in the whole group does not exceed 300 feet. The prevailing rock consists mainly of a calcareous sandstone, very porous, and generally so soft as to be easily cut with a handsaw. The soil is very scanty on the hills; but in the hollows it is sometimes two or three feet deep, and very fertile. There are marshes of considerable extent on the main island; the water in them is always more or less salt; it rises and falls with the tide, showing its connection with the sea. There are no fresh-water springs anywhere; consequently, rain-water is universally made use of for drinking and culinary purposes. It is collected from the roofs of the houses, and stored in underground cisterns, which are often in dangerous proximity to cesspits. The greatest rainfall is in July, August, and September; the average fall for the year is about 60 inches. The climate resembles in many points that of the Northern Bahamas; the heat of the summer is tropical, and not refreshed by regular land and sea-breezes as is the case in most of the West India islands. The frequency of storms maintains the character given to the group of being the "still-vexed Bermoothes."

Nothing unusual in respect either of the weather, or of the ordinary prevalent diseases, had been observed by the medical men prior to the appearance of yellow fever in 1864. The only remark on this head worthy of notice, relating to the antecedent atmospheric condition, was made by Dr. Hunter, a resident civil physician at St. George's where the earliest cases occurred, and who had witnessed the two previous epidemics of

yellow fever, to the effect that, for some time immediately prior to the occurrence, "the atmosphere had been in a very dense and oppressive condition; there were evidences of the presence of great moisture, and in general it appeared to be in so unwholesome a state as to induce me to mention to Dr. Franklyn (the principal medical officer of the troops), Dr. Burland, the health-officer, and Dr. Everett of the 39th Regiment, my conviction that fever would speedily develope itself—an anticipation which in a few days was verified."[1]

There was, however, no want of predisposing local agencies at the time to give force and wings to any zymotic poison which might find its way among the inhabitants of St. George's; a seaport town, close and confined, abounding in filth and nastiness everywhere, underground as well as on surface, and without any appliances whatever to get rid of the abominations that meet one in every direction; with many houses, at all times unwholesome and ill-ventilated, then crowded to overflowing with a reckless and dissolute floating population from the numerous blockade-runners and other vessels (which during the American civil war constantly plied between Wilmington in South Carolina, the Bahamas, and Bermuda), and also with a large influx of laboring people to provide for their wants, and to assist in coaling the steamers. Crowds of drunken seamen often spent night after night sleeping in the open air. The demands for provisions of all kinds increased enormously; meat, in consequence of the high rate of wages, being freely and generally used. The slaughter-houses, all of them in the town, were in constant requisition; and as they, in common with privies and other foul outlets, drained into the harbour, in which there is very little current, there was a pretty constant presence of floating blood, offal, and other offensive matter, mixed with seaweed, floating lazily past the wharves. And all this under the fiery heat of a nearly tropical summer. The description of the place reminds the reader very much of the account given of Gibraltar in the early years of the present century, when there was a large congregation of troops and of crews of ships during the war, and the "Rock" was reeking with festering impurities in every direction. Yellow fever, it will be remembered, raged with destructive fury on several occasions among the garrison and civil population.

As to the origin and primary source of the pestilence in 1864,

[1] Prior to the appearance of the epidemic in 1853—and the same had been the case in that of 1843—there was an unusual prevalence of severe bowel complaints, several of the cases assuming all the characters of malignant cholera, and proving rapidly fatal. In 1853 more especially, the expectation of some impending great sickness seems to have been very general, for some time before the first cases of yellow fever occurred that season.

there was, as usual, great discrepancy of opinion; some maintaining that it sprang up, no one could say how; while others, the Bermudians themselves especially, held it was no product or growth of their country, and therefore that it *must* have been imported from abroad. That yellow fever had existed at Nassau in the Bahamas, and in several places in the Mexican Gulf, for some time before it appeared in Bermuda, is beyond doubt: the fact, however, was not, it seems, officially known to the Bermudian government. Most probably, the disease was present in Wilmington also, as it certainly was in some other seaport towns of the Southern Confederacy, during the year. From a list of thirty vessels "entered at St. George's" between the 4th of June and 30th of July, nineteen were from Wilmington, seven from Nassau, two from St. Thomas', one from Jamaica, and one from Porto Rico.[1] The prevailing belief among the Bermudians was that the pestilence was brought to them by the ship Fannie, that arrived on June 13th from Nassau, which place she had left four days previously. As she had entered the port without hoisting the yellow flag, the quarantine doctor did not deem it necessary to visit her at the time; and it was not till several hours after her arrival, when, in consequence of the governor informing him of a rumour that yellow fever existed at Nassau, he went on board to learn all the particulars. He was "quite satisfied that there was then no trace of infectious disease on board, and reported accordingly;" but by this time the crew and passengers had already landed, and were dispersed about the town in different directions! The only persons who had had any ailment during the voyage were one of the passengers, a paralytic, and another man, a carpenter, Alton by name, who had been suffering for some time from intermittent fever. This man had taken up his quarters in one of the lodging-houses on shore; "its sanitary condition would be considered bad." On the 18th, he consulted Dr. Hunter, who recognised his illness as tertian ague, and prescribed the usual remedies, which speedily made him feel better. From the day of his arrival on to the 23rd, Alton continued to "walk about and enjoy himself." On the latter day, feeling very unwell, "he again applied to me," says Dr. Hunter, "with suspicious symptoms, which I treated as for yellow fever, and attended him up to the time of his death on the 30th." There seems to be

[1] In none of these vessels had any cases of yellow fever, as far as was known, occurred during the voyage, or existed on arrival. The first vessel quarantined for yellow fever during the voyage was the Apus, from Nassau, which reached Bermuda on the 29th June. She, with all on board, was kept in quarantine for upwards of a week, and then cleaned and fumigated. No fresh case occurred. The Fannie left Bermuda on the 14th, the day after her arrival, for Greenock, which she reached on the 27th, without having had any sickness during the voyage.

no doubt that this was certainly the first *death* from the pestilence in St. George's; but whether it was the first *case* seems somewhat uncertain, for one of the resident medical men maintained that he had treated two, at least, of the native labourers who had been engaged on the wharves (one of the men, it is stated, had been at work on board the Fannie), for what he considered to be attacks of the fever, a few days previously to the 23rd, the date of Alton's seizure. As both men recovered, there may be a doubt as to the exact nature of their illness; but the fact is nevertheless noteworthy, as it serves to show that it was certainly about the end of the third, or the beginning of the fourth, week in June that the earliest manifestations of the impending sickness were recognised in St. George's.

To complete the history of Alton's case, it must be added that a coroner's inquest was held (for what good purpose it is not easy to understand), when a verdict was returned that "he died by the visitation of God, but whether of yellow fever or not the jury could not determine." The lodging-house was ordered to be forthwith emptied, and the inmates were placed, by the advice of the health officer, under canvas, "and guards posted over them for eighteen days to prevent communication." The house was then thoroughly cleansed and fumigated, and no case of fever appeared there again until late in August, when it was raging all over St. George's.

So much for the *first* unequivocal and fatal case. The *second* case also occurred in a stranger, who, after living entirely on board a vessel (which arrived at Bermuda from Wilmington and Nassau on the 10th of June) in the harbour until the 24th, went on shore to a lodging-house, where he continued, it would appear, in health till July 10th, when he was attacked and died with black vomit on the 17th. He had had no communication with Alton, or with the house where he died.

On the 18th July, a sailor, who had been found drunk in the streets on the previous day, died with black vomit. "Nothing was known about the history of this man." The disease had by this time appeared among the troops at St. George's. A serjeant of the 39th Regiment was attacked on the 16th, and died on the 22nd. The regiment left for England five days later, and no other case occurred subsequently in the corps. The 2nd Regiment, which replaced it at Bermuda, and which had arrived from Gibraltar only on the 16th, was, as might be expected, soon infected. Two men were attacked on the 23rd and 24th; and soon after this date, the disease became general among both civilians and the military in the town, and its neighbourhood. By this time also, the disease had begun to appear in the other islands of the group; most of them seem to have

been attacked between the 22rd of July and the end of the first week in August. The disease thus appears to have commenced in St. George's about the 23rd of June, and a month to have passed away before it gained any force. It then travelled somewhat quickly over the islands, proving most malignant in the towns and dockyard, wherever there were the most overcrowded and susceptible populations.

The earliest case observed in the town of Hamilton occurred at the end of July; the patient had come from St. George's; while the first case among the civilians in Ireland Island was on August 16th, in a boy who had also come from the same infected locality. It is to be remembered that there had not, from the very first appearance of sickness at St. George's, been any interruption of the ordinary intercourse between that and the other islands: indeed, in consequence of the unusual demand for labour at St. George's, the intercommunication had been much greater than usual. The epidemic seems to have reached its acme of intensity from about the end of August to the middle or third week of September. After the end of September, there was a marked abatement in the prevalence and severity of the pestilence: cases, nevertheless, continued to occur in different islands down to the end of the year. During the early part of November, the temperature was lower than in ordinary seasons, and the fever seemed to have disappeared altogether; but, after the middle of the month, southerly winds prevailed, and the air became sultry and oppressive. On the 20th November, there was a sudden reappearance of fever at St. George's, at Hamilton, Mount Langton, and at Ireland Island, and several other places simultaneously. Dr. Hilditch, R.N. remarked a similar circumstance in the epidemic of 1853. The cool weather of December destroyed the vitality of the disease. The last case among the troops appeared on the 12th of that month; but, in the town, a few cases were seen as late as the 5th of January, 1865.

As may be gathered from the preceding details, the town and neighbourhood of St. George's were unquestionably the original and principal seat of the pestilence; and there its chief ravages occurred. The total mortality among the civil population of Bermuda—estimated in 1861 at 11,450—was set down at 237 deaths out of no fewer than 3148 persons attacked.[1] This amount of mortality is nearly double what occurred in the severe epidemic of 1853, and three times as great as that in the milder visitation of 1856. The great influx of merchant-sailors and

[1] Less than three per thousand of the coloured classes died of the fever, although the cases were very numerous among them. Of the white civil population, forty-nine per thousand died. The estimated number of the coloured population was 6826; that of the white was 4624.

other strangers in 1864, consequent on the irregular trade then carried on, was most probably the chief cause of this difference.

We must now consider some of the principal points relating to the troops in connection with the epidemic.

The 2nd battalion of the 2nd Regiment, from Gibraltar, landed at Bermuda on the 16th of July, or about three weeks only after the earliest appearance of the disease. Seven companies were at once encamped on Boaz Island (which is immediately adjoining to Ireland Island), and the three other companies were camped out at a spot on the coast of St. George's, called the "Navy Tanks," about half a mile or so north of the town. Seven days after the arrival, one of the companies from Boaz Island joined the head-quarters at the "Navy Tanks;" and, a week later, the head-quarters were moved to Ferry Point, about six miles to the westward, where the 39th Regiment had been encamped previous to their sailing from Bermuda on the 26th July. A detachment of only fifty-six men was left at the "Navy Tanks" for fatigue duties, which unhappily exposed them to frequent intercourse with the town. Bell-tents of double canvas were issued to the men at all the camps. Three men lived in each tent; each man had therefore 171 cubic feet of space, estimating the cubic contents of a tent at 513 feet. Every man had an iron bedstead, and a horsehair pillow and mattress. The first case of the fever in the 2nd Regiment was on the 23rd of July; and, on the following day, a case among the Royal Artillery stationed at St. George's occurred. It would seem that the disease manifested itself among the troops in Boaz and Ireland Islands about the same time; but the type at first was mild, and no deaths occurred till the 18th of August. The chief mortality at St. George's took place between August 5th and the end of September; at Boaz and Ireland Islands, between the beginning of September and the end of the third week in October. As, however, on September 9th, 209 officers and men of the 2nd Regiment were sent off to Halifax, and as, on the 24th, an additional 189 men of that corps and of the Ordnance also left for the same place, it is impossible to determine the exact period of the acme of the fever. Among the troops sent off, four cases only occurred either on the voyage or at Halifax, and three of these proved fatal. These four cases were all among the second batch. There was no tendency to any extension of the fever after leaving Bermuda; it seemed to become extinct, upon leaving the focus of infection. After the removal of the 398 men, the rest of the troops on St. George's were concentrated in the camp at Ferry Point; and there was very little sickness afterwards at this station, although the fever continued to prevail for a month later at Ireland

Island, and still longer in the town of St. George's. Nothing can show more emphatically the terribly pestiferous condition of the town and suburbs at the beginning of September, and the virulence of the morbific poison there, than the painful fact that of six medical officers, who were brought from Halifax to attend on the sick in the hospitals, all were within five days after their arrival struck down with the fever, and that four fell victims to the scourge.

As to the effect of camping-out the troops on suitable ground, the Report states that it "was very good, and would have been still more satisfactory but for the necessity of sending the men back in small detachments to do duty in the most dangerous localities, until a sufficient supply of coloured people was obtained to relieve them of that labour. It would appear that every man, who died at Ferry Point Camp, contracted the fever while on duty at St. George's.[1] The bandsmen, who were confined to the camp, did not lose one of their number." Colonel Rose, in command of the battalion, remarks that "the removal of the head-quarters to Ferry Point was beneficial in the highest degree. The site would appear, from all circumstances, to be well suited for a sanitary camp."

In a therapeutic, as well as in a protective or preventive, point of view, the importance of withdrawal from an infected locality and its immediate neighbourhood is to be kept in mind: the chances of recovery from an attack are always sensibly greater in a wholesome atmosphere. Of 291 cases of the fever treated in the hospital buildings near St. George's, 107 died; whereas of 61 cases treated, at the same time, in marquees at Ferry Point, only 17 were fatal. As to the high mortality among the sick in the military hospital at St. George's, no one can wonder at it if, as Dr. Grant states, "at the time of the greatest crowding, the highest number in hospital was 103, and the cubic space per man was 349 feet. During the height of the epidemic, the pressure was extreme, many of the men having to lie on the floor and in the verandah. During such overcrowding, the ventilation was, of course, imperfect." This is very sad and very shameful.

From the general return of the statistics of the epidemic among the troops, it appears that of 74 officers, 40 were attacked, and 14 died; and that of 1147 men,[2] 511 were attacked, and 173 died. Among the 228 women and children, there were 79 attacks and 15 deaths.[3]

[1] Of 143 men of the 2nd, who were left in frequent communication with the town or immediate neighbourhood of St. George's when the head-quarters were moved to Ferry Point, 103 were attacked and 55 died.

[2] Eighteen men were admitted twice into hospital with relapses: the number of men actually treated was therefore 493.

[3] It would seem from the Army Report, 1866, that at the commencement

The high rate of mortality among the military, as compared with that among the white civil population (139 and 49 per 1000), was doubtless largely owing to the influence of non-acclimatisation among the former. Newly arrived persons, under such circumstances, invariably suffer far more than others.

But little information is given in the Report as to the results of the epidemic in the naval department at Bermuda. Of 98 sailors (Ireland Island), 46 were attacked, and 15 died; while, among the same number of marines there, only 12 were attacked, and 3 died. This small loss among the latter was owing to their early removal to Halifax. Of 335 white artisans and labourers in the dockyard at Ireland Island, 146 were attacked, and 10 died: whereas, among the 95 coloured men engaged in the dockyard, only 16 of them sickened, and without a fatal result in any of the cases.

The following example well deserves the attention of nautical men:

"The value of a proper application of sanitary laws was made very evident at Hamilton in September, at which period the fever was raging with great virulence. The Melita, a steamer of 850 tons, with fifty-five people on board, arrived at St. George's on August 23rd, and afterwards proceeded on to Hamilton (the chief town in the central island of the group) on September 12th, where she was hauled alongside a wharf, where she lay till October 3rd, without a case of fever appearing in the ship. The precautions used were these:—Total abstinence on the part of the captain and crew, and no liberty allowed on shore. The vessel was fully awned; decks washed twice a day; cabins and forecastle cleaned daily, the latter whitewashed twice a week; and the bedding aired fore and aft. The holds, fore and aft, and the engine-room, were several times whitewashed; the bilge-water was constantly cleared out; chloride of lime was sprinkled daily over the whole ship. The crew had fresh provisions, and an arrangement was made for them to have salt-water baths on board."

After relating the above particulars, the Board of Health of Hamilton very sensibly remark that, considering the immunity of the Melita, "while the fever was raging violently in the town, and on board of vessels near and around her, it is reasonable to conclude that, on similar regulations being made by householders and others, and particularly by masters of vessels coming here during the prevalence of a yellow fever, like benefits would result from them—at least, in whole or in part, as the regulations may be put into practice."

How necessary such advice is, in respect of shipping, will

of the epidemic, there were 85 women and 134 children present with the troops.

appear from the fact mentioned by the health-officer that during the prevalence of the epidemic, "almost all the blockade-running steamers plying from Bermuda and Nassau were infected, having carried the disease from this port or Nassau."

It is also worthy of notice that, out of seventeen attacks which occurred among the inmates (the total number is not stated) of the town-prison of St. George's, not one terminated fatally. This result is attributed by Dr. Hunter to the regularity of the diet, the impossibility of obtaining spirituous liquors, and the non-exposure to night-air. Ten of the cases were in Europeans, and seven in natives.

That the virulence, as well as the diffusion, of the disease was fearfully aggravated among the troops, especially among those who were stationed near the town of St. George's, by the insanitary arrangements and very unwholesome condition of the quarters of the men, is clear beyond a doubt. In many respects nothing could be worse—badly situated, close and ill-ventilated, and inevitably polluted at all times with privy effluvia from foul cess-pits. All this was forcibly pointed out in the official report on the epidemic of 1853, when between three and four hundred soldiers were swept off within less than three months. Yet little seems to have been done in the interval to correct this state of things, which ever-recurring experience shows as infallibly feeds a pestilence as old timber and tar-barrels will feed a conflagration. Colonel Hawley, R.E., the lieutenant-governor of the colony, in transmitting the report to the Colonial Office, fairly objects to this being called "the *cause* of the pestilence, because they (the buildings, &c.) remain from year to year much in the same condition, and, if they generate and emit a poisonous influence, that influence must operate constantly, though not necessarily, to the extent of producing every year yellow fever. There is, however, no evidence of such constant operation, for, except in epidemic years, the population is singularly healthy." We much question the accuracy of this latter statement: there is no registration of deaths among the civil population; and with respect to the health of the troops in ordinary years, this has often been anything but satisfactory. The abounding sanitary evils in a place may not directly or proximately be the *cause*, but they may be the *occasion*, of a pestilential visitation; they may not *per se* generate or beget, but they may serve to breed or engender, a disease which, without their concurrent action, might never have become fully developed. The genesis of many diseases seems truly to be the product of a binary, not of a single, agency; and Colonel Hawley is too intelligent not to recognise practically this medical truth; for he goes on to remark—"With regard to the spread of the

epidemic, a different conclusion seems warranted. After it had once begun, there is every reason to believe that it was fostered and propagated through the absence of sanitary precautions." Now, when we find that even the officers' quarters are declared to be so unwholesome that it is "most dangerous for any one to continue living in them, particularly during the summer months," what, it may be expected, will be the condition of the barracks of the men? It has been already seen how severely the officers suffered. One instance will suffice as to the chief cause of their disastrous sickness and mortality: — "Of eight officers occupying these quarters (to the east of the Royal Barracks), not one escaped, and three died. Two officers' wives and two children were also living in the quarters; and of these only one, an infant at the breast, escaped an attack."

With such surroundings, and in so unprepared a state as the military were evidently in to encounter the emergency of an epidemic visitation, the prompt removal of as many of the troops as possible to Halifax when the disease had already proved very destructive, and threatened further ravages, was doubtless a wise and salutary act, adopted in compliance with the urgent representation of Dr. Barrow, who had become principal medical officer on the death of Dr. Franklyn. "One measure," remarks Colonel Hawley, "not a profound or scientific one, was resorted to with good effect—namely, flight. It is, to the best of my belief, the only reliable mode of escape. It was undoubtedly the means of saving many lives, and I entirely concur in the opinion of the commissioners that the means of transport should be at hand every summer." The Report declares that, in the event of future epidemics, the prompt removal of the troops from the colony "is the only effectual and certain remedy; for it must be remembered that there are no mountainous districts in Bermuda, where land can be found at such an elevation above the sea as to render immunity from the disease probable." Dr. Barrow recommends that, on the occurrence of the first case of yellow fever in any town or barrack in the colony, the whole of the troops in the vicinity should be instantly placed under canvas, at the distance of two or three miles, if possible; and that, should a second case anywhere appear, steps should immediately be taken to remove all the white troops to Halifax. It may be presumed that this advice is conditional upon the continuance of the existing insanitary state of the military quarters, and the absence of available arrangements for the prompt and safe encampment of the men, on the first suspicion of impending sickness in any season. The climate and configuration of the islands, notwithstanding the absence of high elevation, are not such as inevitably to render them everywhere

unsafe in yellow-fever years. So, at least, thinks one of the non-medical commissioners, the attorney-general of the colony, who takes exception to the unqualified recommendation in the report, on the ground that

"no sufficient evidence is produced to show that these islands do not contain within themselves abundant sanitary resources for all practical purposes, if the same be promptly and fairly employed. Isolation and dispersion of troops are perfectly obtainable in Bermuda; and if the various camping-grounds were habitually kept in a state fit for use in emergency, and proper regulations with regard to the troops to save them from exposure, fatigue (intemperance, also), and contact with infected districts were at hand, to be immediately enforced on the first indication of epidemic disease, and were promptly and firmly carried out, the results would probably be little, if at all, less satisfactory than those devised in 1864 from sending the troops to sea. . . . At least, it seems to be worth while, before resolving on denuding the colony of its garrison, to try the effect of a more complete dispersion and prompt separation of sick from well than has yet been attempted."

The whole subject obviously demands careful consideration by the War Office; it affects sanitary regime, not in Bermuda only, but in many other colonies where our troops are stationed. Nor is the Navy less concerned in the matter than the Army; for the question of "flight" from a station of our ships of war, on all occasions, upon the appearance of any cases of yellow fever among the crew, as the only measure of safety that can be had recourse to, is at present exciting a good deal of discussion in the naval service.

If it be really the case that all sanitary and hygienic precautions on board a vessel are powerless, even with the concurrent assistance of well-appointed hospitals on shore, against the spread and ravages of yellow fever in tropical climates, the prospects of our mercantile shipping, which of course cannot at once start for a northern latitude when danger is apprehended, will indeed be very dismal. But this has not been shown; and we are certainly not without a strong hope that much may be done by judicious arrangements and appliances, systematically carried out *before* as well as during the occurrence of the disease, whereby the risk may be very greatly diminished. The berthing of the men is always a point of prime importance in relation to health; in epidemic seasons, it becomes a question of life or death. As far as possible, the sick should be treated under canvas on the upper deck, so that there may be at all times free circulation of pure air around them.

A comparison of the epidemic in 1864 with the visitation in 1853 and that in 1856 brings out some noteworthy results.

The mortality among the civil population, including the mercantile marine, in these three years was respectively 237, 138, and 88.[1] The large excess in 1864 has already been accounted for. Among the military, the deaths were in the several visitations 206, 361, and 4; the strength of the garrison, including women and children, being respectively 1460, 1517, and 1326. Regard is, of course, to be had to the removal of a considerable number of the troops on the last occasion, before the subsidence of the epidemic. As to the very small mortality in 1856, this was doubtless due, in a great measure, to the circumstance that the stress of the epidemic, which was altogether less virulent and more local throughout its continuance, was chiefly felt over the western islands of the group; St. George's, where most of the troops are stationed, being comparatively but slightly affected. That, however, the early camping-out of the troops had a good deal to do with the fortunate result, as indicated by Dr. Milroy in a memoir addressed by him to the Colonial Office soon after the visitation, may be reasonably inferred from what took place among the garrison stationed on Ireland Island, which was then one of the principal seats of the disease. "On the very first announcement of the yellow fever, a large number of the troops were put under canvas at night; and immediately that a case appeared in barracks, all the men were encamped; and as soon as a case showed itself among them there, the camp was shifted. By these means the infantry, 296 in strength, presented only 28 attacks, and no deaths."[2]

Whatever uncertainty may still exist as to the primary origin of yellow fever in Bermuda—whether it be of indigenous development, or be due to introduction from abroad—there can be none as to the local conditions which foster its growth, and which give it force to spread and become fatally destructive. The history of the latest as of the former epidemics shows, moreover, the futility of quarantine measures, as hitherto practised, to keep out the pestilence. The system pursued in Bermuda, and in too many other places, is obviously a mockery and delusion, and looks very much like a mere official sham to impose upon the public credulity. If, indeed, (as one of the resident medical men, Dr. Lough, suggests,) a thorough inspection of every vessel arriving from the West Indies, especially during the summer months,

[1] In 1853 and 1856, there was a large body of convicts at Bermuda; they were mostly kept in hulks moored off Boaz and Ireland islands. In the former year, out of 1778 there were 161 deaths from yellow fever; and in the latter year, out of 1110, there were 134 attacked, and 27 died.

[2] On the "Yellow Fever Epidemics of Bermuda," by W. R. E. Smart, M.D., Deputy-Medical Inspector of the Royal Naval Hospital, Bermuda, in the 'Transactions of the Epidemiological Society,' vol. ii, p. 24. This valuable paper will well repay an attentive perusal by all military and naval medical officers.

and a hygienic examination of all on board, were made by a competent health-officer, and this gentleman had ample discretionary powers to enforce all requisite sanitary precautions before free pratique was granted, much good might reasonably be hoped for, in respect alike of the vessels themselves and of the community on shore.

In 1864, the fever had existed in and about the town of St. George's for some time before any quarantine upon foreign arrivals was had recourse to; nor can any one read attentively the history of the earliest fatal instance in the man Alton without believing that he contracted the dangerous fever on shore, as is extremely apt to be the case with new-comers into an infected locality. Neither in 1853, nor in 1856, could the commissioners, who reported upon them, trace the disease to foreign importation; most of the medical men upon both occasions declared their belief that it was of indigenous development. Unfortunately, the profession are, on the whole, so much at sea —without a compass, too—upon the subject of Quarantine in relation to yellow fever, and there is so little accord of opinion as to the line of procedure which should be adopted in different cases, that there seems to be little prospect yet of any rational system being pursued in Bermuda for the future, notwithstanding all the lessons of past experience. The Report before us shirks the difficulty by expressing no opinion on the matter. Dr. Barrow has evidently no faith in quarantine as a defence against future visitations; and he justly remarks that reliance upon it as a safeguard will infallibly serve to delay, and to render less thorough, the correction of the abounding sanitary evils which have hitherto played so pernicious a part in all the epidemics. The opinion, hazarded in the recently published report on the health of the Navy, that "had quarantine been as rigidly enforced at Bermuda in 1864 as it was at New Orleans in 1862, there can be little doubt that the islands would have escaped the disastrous visitation which it sustained," will be questioned by many, and probably by none so much as by the medical officers, of both services, who have had experience of any of the recent epidemics there.

It would seem that, during the height of the epidemic, a Nuisances' Removal Act was hurriedly passed by the Bermuda legislature; and this is, oddly enough, entitled "An Act further to amend an Act to establish certain Regulations for the performance of Quarantine." Surely it is high time that the Colonial Office of this country should have their attention directed to the sanitary condition and requirements of the dependencies under its control, with the view of counselling and guiding them in respect of the measures required both for their internal im-

Potter: the muscles of the back, thigh, and leg. The epilogue or conclusion by Dr. Archibald Pitcairn."

Another remarkable circumstance is the rapidity with which this long-neglected study advanced, mainly owing to the ability of the distinguished men by whom it was taught—these being Monro, primus and secundus; the first appointed Professor of Anatomy in 1720, with a yearly salary of £15, the second, as his father's successor and colleague in 1754; who in his turn was succeeded by his son, Monro tertius, in 1798.

Dr. Struthers, in his account of the Edinburgh Anatomical School, collegiate and extra-collegiate, perhaps attaches too much importance to this branch of medical education as regards the celebrity which Edinburgh acquired as a school of medicine. In fact, during the whole of the time that the third Monro filled the anatomical chair in the university, viz., from 1798 to 1846, anatomical study was at its lowest ebb, and yet it was during that period that the university was most resorted to, its classes were largest, and its reputation highest. This may be attributed to many circumstances, and chiefly, first, to the excellent system of clinical teaching there carefully followed out; next, to the eminent men who filled some of the more important chairs; and further, to a society, the Royal Medical Society, instituted and governed entirely by students, and possessed of an excellent library—a society truly of emulation, where alone honours were attainable, and where, in all the presumption of youth, as shown in their debates, its members considered themselves often in advance of the professors. Moreover, during a considerable part of this period, it should be kept in mind that the Edinburgh Medical School had scarcely any rival, there being then no provincial schools, and the system of instruction followed by those of London attached to the great hospitals being, with the exception of anatomy, of the most elementary kind.

The biographical notices, which occupy a large proportion of the pages of Dr. Struthers' work, are very unequal; they are amplest concerning the extramural teachers, some of whom must be familiar to our readers, others, perhaps, unknown. The notes are ample, and in them will be found the details of most interest as regards the rise and progress of the alma mater of so many of our profession.

ART. II.—*On Insanity, and the Criminal Responsibility of the Insane.* By THOMAS MORE MADDEN, M.R.I.A., &c. *Dublin,* 1866, pp. 23.

WE have read with much satisfaction this ably written paper,

in which its author points out the inhumane manner of the judicial treatment of the insane under trial for criminal acts, on the plea that, provided the accused can distinguish between right and wrong, they are to be held responsible. And he adduces a considerable number of individuals decidedly insane who have been condemned to death under that plea, and of whom many have been executed.

What specially called Dr. Madden's attention to the consideration of the subject was a remarkable case of much interest, which he describes; that of a youth, ill educated, his mind vitiated by the reading of the low sensational novels of the London press, who attempted the murder of his mother, followed by an attempt at suicide, after an attack of fever; and who, when brought before a police magistrate, charged with these acts, would have been sent for trial, had not a physician shown that it was a case, not for a jury, but for a lunatic asylum, where he was accordingly sent, and where, under proper medical treatment, he was restored to health, and capable of following his former occupation.

To meet what our author holds to be the defects in the judicial proceedings in cases of the kind, he suggests two measures, short and simple, which he thinks would be effectual, viz.—

1st. That the 64th article of the French code should be adopted by our law in its entirety.

2nd. That the question of madness should be decided by a tribunal of experts.

The article of the French code is the following:

"Il n'y ni crime ni délit, lorsque le prévenu etait en etât de démence au temps de l'action." And he quotes a similar law which has been enacted in the state of New York.

Reasoning on the propriety of employing medical assessors in lunacy, who could advise the court on all professional points, as nautical assessors do in marine cases, he well remarks—

"No rational man would be likely to take the opinion of a jury of grocers, publicans, or tallow chandlers, as to whether his lungs were sound or his watch in order, in preference to that of a physician or watchmaker. And yet in a question far nicer than either of those the question of a man's healthy or diseased physiological condition—of all scientific investigations the most delicate and difficult—is, by the laws of England, left to the decision of such persons, assisted by a judge whose knowledge, however extensive it may be in legal matters, is hardly if at all greater than theirs in any department of medical science."

It is right to remind our readers that there are two acts by which at present, in cases of insane persons condemned for criminal acts, execution may be arrested and humanity not outraged.

One, that of the 3rd and 4th of Victoria, cap. 54, that, even after sentence has been pronounced, "it shall be lawful for any two justices of the peace of the county or place where any person under sentence of death is confined to inquire, with the aid of the physicians or surgeons, as to the sanity of such person; and if it shall be certified by such physicians and surgeons that such person is insane, it shall be lawful for one of Her Majesty's principal Secretaries of State to direct, by warrant, that such person be removed to a proper receptacle for insane persons." The other, that of the 27th and 28th of Victoria, cap. 29, by which the power of medical inquiry into the state of mind of persons under sentence of death is confined to physicians and surgeons specially appointed for this purpose by one of Her Majesty's principal Secretaries of State.

We heartily concur in the hope expressed by the author in his conclusion—"That a wiser and more humane spirit may before long pervade the laws of this country relating to the so-called criminal insanity, and that the most horrible punishment that man can inflict shall no longer be awarded to the most unfortunate of all victims of disease."

ART. III.—*Report on the Health of Liverpool.* By W. S. TRENCH, M.D., Medical Officer of Health for the Borough. 8vo, pp. 32, &c.

WE purposed noticing this pamphlet before now, but though it refers to the public health of nearly two years ago, yet as it is concerned with subjects of general interest and operation, we think that it is right even now to draw public attention to it. It appears that the year 1865 was very sickly and fatal in this great commercial port, with its half million of population. Rather more than 36 in every 1000 of its inhabitants were cut off; the deathrate in different districts varying from between 24 and 25, in some districts, up to 49 per 1000 in others. This high mortality is considerably above the corrected averages of the previous ten years. The excess was mainly due to a marked increase of fever in Liverpool ever since the end of 1862. This home pestilence is never absent from the town; but for the last three years it has prevailed in the epidemic form. In 1863, the deaths from fever (chiefly pure typhus) were 1304; in 1864, the number rose to 1774; and in 1865, they were no fewer than 2338. Dr. Trench gives a projected diagram, which shows the weekly mortality from the disease during these three years; and also a mortality map of typhus in 1865, the worst districts being coloured yellow, and the number of deaths in each street indi-

cated by red dots. The topographical distribution of the pest over the town is thus patent at a glance. Charts of disease are always extremely instructive; the example of our provincial brethren should be followed by the associated officers of health of the metropolis, the medical topography of which has yet to be investigated with the care which the importance of the subject demands. Sanitary experience in Liverpool confirms, in the most striking manner, the results of observation elsewhere in respect of the principal causes of the origin and spread of typhus, viz., overcrowding in unwholesome, ill-ventilated rooms, especially when this is associated with destitution, and filthiness of person and abode. There is certainly no form of pestilence whose ætiology (and consequently whose prophylaxis and subjugation) has been more satisfactorily made out on the whole, than that of typhus. We cannot, indeed, as yet explain why it is so much more epidemic in some years or series of years than in others; but as to the endemic or local provocatives and adjuvants of the morbific poison, we know with certainty what will on the one hand inevitably foster and intensify, and on the other hand what will dissipate and prevent its deadly operation. Nowhere is it more needful that this most important social question be rightly understood by municipal and other public bodies which have the charge of the public health than in Liverpool, and we are therefore glad to find that it has not long ago attracted the attention of a Health Committee, appointed to inquire into and report as to the causes of the excessive mortality of the borough, and as to the means to be adopted for mitigating the same. From the minutes of evidence taken before this committee, and which consists almost entirely of the evidence given by Dr. Trench, we select the following passage, which very clearly puts before us certain peculiarities, social and economic, in the condition of the labouring population of Liverpool, which require to be attended to in discussing the question of the generation and prevalence of typhus in their midst.

"In a manufacturing town the men work in large bodies, and thus they are known to each other, and there is among them a solidarity of interest. They may not always be occupied full time and obtain full wages; but at the worst there is only a lessening, never a total cessation of income, and this diminished time, though on the verge of 'clamming,' never reaches to absolute want. Of course I am talking of ordinary times, and not of years so exceptional as that of the cotton famine. Now, *per contra*, the unskilled labourers of our port have no fixed place or combined unity of work; there is much of chance and little of certainty in their employment; a man goes from Toxteth Park to the extreme northern docks, and *vice*

versâ, in search of work; a westerly breeze,[1] or a spurt of commercial speculation may for a few weeks give him full wages; but then comes the period of total stoppage, for which no provision has been made. The transition is almost immediate from abundance, or at least sufficiency, to absolute pauperism. This uncertainty, like the rapid changes of gambling, tends to demoralise the man, and almost to preclude any steady household economy by the wives; hence we find among them an amount of reckless imprudence, drunkenness, and squalid wretchedness greater than among the same class in manufacturing towns. I have stated that the indigent unemployed flock to the towns; unfortunately many of such immigrants bring with them not only the burden of poverty, but a capacity of submitting to privations, which acts as a pernicious example to other labourers, and hence we find in this town of commercial prosperity and wealth large sections of the people in the lowest scale of social, physical, and moral degradation.

"There can thus be no difficulty in understanding why—1, typhus fever is never absent from Liverpool; 2, why, among a population so reduced by overcrowding and privations below the normal standard of health and strength, the slighest increase of public distress quickly converts sporadic typhus into the gravity of a fatal epidemic; and 3, why seaports like Liverpool and Glasgow, the resort of unskilled labourers, and especially of poor Irish, are so unfavorably placed on the lists of the Registrar-General returns of annual mortality."

Dr. Trench, like all other experienced officers of health, is convinced that by far the most important preventive and prophylactic measure for the terrible health-evils which they have to grapple with in Liverpool, is the providing of better dwellings for the poor. He points to what has been or is being done in London, Glasgow, Newcastle-on-Tyne, and other towns, in the way of erecting model lodging-houses and cottages, and expresses his regret that "Liverpool does not take the lead in a matter in which Liverpool is more interested than any other town in the kingdom." It is fortunate that the corporation have so able and zealous a professional counsellor at their side as Dr. Trench, and we cordially trust that his representations will not have been made in vain. The matter in question is at every time of vital moment to the welfare of the town.

[1] Whenever there is any long continuance of easterly wind, and consequent delay in the arrival of homeward-bound vessels in the Mersey, there is inevitably a large and sudden increase among the claimants of parochial relief. "This increase of pauperism is always accompanied by an increase of typhus; and thus the records of a meteorological phenomenon, of an economic effect, and of a contagious disease are all imprinted on one and the same line of the death register."

ART. IV.—*On the Electrolytic Treatment of Tumours and other Surgical Diseases.* By JULIUS ALTHAUS, M.D., M.R.C.P. London, 1867, pp. 31.

THIS small treatise is the substance of a paper recently read before the Medical Society of London. After allusion to the physical, chemical, and physiological effects of a galvanic current on living bodies, and explaining "*what takes place in a galvanic battery which has been properly charged,*" the author proceeds to speak of certain microscopical observations on the changes undergone by animal structures under the influence of the continuous galvanic current, which led him to adopt the electrolytic treatment. He observed that no animal tissue whatever could withstand the disintegrating effects of the negative pole, and that the rapidity and force with which this disintegration is brought about are proportioned to the electro-motive force employed, and to the softness and vascularity of the structures acted upon. This electrolytic action of the negative pole is mainly composed of the mechanical action of the nascent hydrogen and of the chemical action of the free alkali developed along with the hydrogen. Experiments on living animals showed that considerable changes in the nutrition of the parts were observed as proximate and remote sequelæ of the operations.

"It was then observed that a needle connected with the negative pole of the battery could be inserted into and removed from the body without causing any loss of blood; that the current used did not appear to give any pain to the animal beyond what was due to the introduction of the needle through the skin; and that the parts operated upon shrank sensibly after the operation, but that there was neither inflammation, suppuration, nor sloughing. If the negative pole was made to act upon blood-vessels, it was found that they were first filled with a foreign body due to disintegration of the blood, and round which afterwards a slow separation of cancellated fibrine took place. They were thus charged into solid strings wherever the current had been made to act."

This being the case, the author thought that the current could safely and successfully be applied to parts of the body where shrinking and disintegration of tissue and obliteration of blood-vessels might be required for surgical purposes.

After describing the battery and the apparatus required for the electrolytic treatment, Dr. Althaus proceeds to enumerate the various pathological conditions in which the treatment has been or may be expected to prove serviceable, and these are tumours, certain diseases of the blood-vessels, especially

aneurisms, serous effusions, strictures, wounds, and ulcers. Cases are adduced illustrating the treatment in some of these affections.

ART. V.—*A Manual of the Operations of Surgery, for the use of Senior Students, House-Surgeons, and Junior Practitioners.* Illustrated. By JOSEPH BELL, F.R.C.S. Edin., &c. *Edinburgh,* Maclachlan and Stewart; *London,* Hardwicke, 1866, 16mo, pp. 267.

THIS is a modest and, we think, a useful little volume, which will compare favorably with its numerous competitors by Maunder, T. Smith, and others, inasmuch as it embraces several methods of performing various operations which have been invented since the publication of those works, is illustrated by woodcuts, though perhaps hardly sufficiently, and the woodcuts, although rough, are yet not so unpleasing as those given in Mr. Maunder's volume. The author very properly disclaims any originality for a work of this nature, and he also most properly gives his authorities for almost every part of his compilation; yet he writes in the style of one who has had some experience of his own on the living body; and the proceedings which he describes are, for the most part at least, feasible in practice. From this statement a few exceptions may, doubtless, be made; but hardly any of these manuals of operations on the dead subject are free from some descriptions which most people would find it impossible to realise on the living. To our minds the following must be classed in this category. In the operations for "perineal section," *i. e.* for opening the urethra and dividing a stricture when impassable to instruments, Mr. Bell gives the following directions:—"A full-sized catheter must be passed as far as possible up to the stricture, and held firmly in the middle line . . . . The urethra should then be opened over the catheter, the edges of the mucous membrane held to each side by silk threads passed through them, and the surgeon must endeavour to pass a fine probe into the opening of the stricture. If this can be done, it is comparatively easy to slit the stricture up," &c. (P. 248.) We have performed or assisted at numerous such operations, which we hardly think Mr. Bell can have done, or else his cases must have been very different to ours. The depth of the parts and the free bleeding renders it absolutely impossible to recognise the edges of the mucous membrane—at the apex of a conical wound, perhaps, two or three inches in depth; and to see anything like the opening of a stricture at such a depth, a man must have sharper eyes

than surgeons on this side of the border can boast of. Another statement of Mr. Bell's on the subject of urinary operations would be read with much amazement in France:—"Internal division of stricture may be merely noticed here as of historic interest, but is now very rarely practised, and has justly fallen into disrepute, as being both dangerous and (in severe cases) impossible" (p. 245). But, notwithstanding a few such statements as these, which we regard as either erroneous or at least incomplete, we have no hesitation in speaking in terms of much praise of Mr. Bell's book, as much exceeding most works of the kind in clearness of description, combined with brevity; and we would recommend it as peculiarly adapted to the use of surgeons in the army and in distant colonies, for whom a portable and at the same time a tolerably complete manual, brought down nearly to the present day, is certainly a desideratum.

ART. VI.—*Leprosy.* By C. MACNAMARA, Surgeon to the Calcutta Ophthalmic Hospital. Calcutta, 1866, pp. 59.

THIS pamphlet, a reprint from the 'Indian Medical Gazette,' contains a succinct and ably drawn-up analysis of the one hundred and seven reports of medical officers constituting the contents of the volume recently published by the Indian Government in reply to the interrogatories of the Royal College of Physicians relative to leprosy.

The conclusions which Mr. Macnamara considers himself justified in drawing from these documents are the following: and so far as the data are concerned, they appear to us to be fully warranted.

1. That leprosy is a specific disease and confined to two forms, often mingled, the tuberculated and the anæsthetic.

2. That it is more or less generally diffused in the Bengal Presidency, in the Straits in Borneo, but is unknown amongst the native population of Burmah.

3. That it is decidedly hereditary.

4. That it is contagious, "*but it is necessary for the propagation of the disease by this means that the discharge from a leprous sore should enter the blood of the healthy person*; and, further, that the disease even then (unless under peculiar circumstances) may remain undeveloped in the system for years." In an interesting letter appended, facts are stated by Dr. Hillebrand, derived from his experience in Honolulu of the Sandwich Islands, greatly in favour of this conclusion. In that island the disease, it would appear, has been known only for a few

years; and that since its first appearance in 1852, it has spread so much that about 4 per cent. of the inhabitants are now the victims of it.

5. That neither climate, kinds of food, or filthy habits *per se* are capable of generating it.

6. That when fully formed it is incurable; but may be mitigated, at least, if not arrested in its early stage, more especially the premonitory. Mr. Macnamara states that from no application has he witnessed better effect than from the external use of Chaul Moogra oil. This reminds us that when in the Mauritius many years ago we heard of the temporary cure of the disease in cases of it removed to Tortoise Island, there employed in making cocoa-nut oil with which their bodies were smeared, their food principally turtle. Dr. Hillebrand remarks that he found nitrate of silver more beneficial in checking the malady than any other medicine of which he made trial; he began with one tenth of a grain, three times daily, gradually increased to a quarter and even half a grain, discontinuing it about the end of the third month. He states also that he had some good effects from copious draughts of cold water, six to eight tumblers daily.

Mr. Macnamara in a note to his paper, after adverting to the total deficiency of suitable hospitals and asylums for lepers in India, makes a strong appeal to the Indian government in behalf of the neglected state of the natives, so many of the vast population destitute, without poor-laws, without workhouses, or any charities from which their orphan children can receive relief. He urgently asks for a more liberal system, and apart from humanity asks for it on political grounds. He points out how much might be done towards the acquiring a more exact knowledge of the epidemics which are the scourge of the country, were the highly educated medical officers of the establishment directed to investigate them, and encouraged so to do by the publication of the results of their researches; and, in proof, he adduces the instance of what has been accomplished by these gentlemen in the short space of a few months as shown in their reports on leprosy.

ART. VII.—*Pavilion Asylums (with a ground plan.)* By C. L. ROBERTSON, M.D. Cantab., Medical Superintendent of the Sussex Lunatic Asylum, &c. London, 1867, pp. 12.

DR. ROBERTSON, in this paper, which was read at the Annual Meeting of the Medico-Psychological Association in Edinburgh

on the 31st July, 1866, advocates pavilion asylums, on account of what he believes, and, so far as we can judge, justly, to be their superior advantages, viz., economy of construction, facility of enlargement, separation of the several departments, and efficiency as regards communication, ventilation, and quietude.

With much propriety he makes provision in his plan for baths of different kinds, of which in all our hospitals, compared with the continental, there is so great a deficiency; and we are glad to see that in adverting to this he does not omit the hot-air or Roman bath, of the therapeutic agency of which he has witnessed good effects in cases under his own care at Hayward's Heath.

ART. VIII. — *Medicinskt Archiv, utgifvet af Lärarne vid Carolinska Institutet i Stockholm.* Redigeradt af E. A. KEY, Professor i Pathol. Anatomi; C. J. ROSSANDER, E. O., Professor i Chirurgi; A. KJELLBERG, Adjunct i Pædiatrik. Tredje Bandet, Andra Häftet. Stockholm. Samson and Wallin, 1867.

Archives of Medicine, published by the Teachers in the Carolinean Institute in Stockholm. Edited by E. A. KEY, Professor of Pathological Anatomy; C. J. ROSSANDER, Professor Extraordinary of Surgery; and A. KJELLBERG, Adjunct in Pædiatrics. Third volume, second part.

THE present number of the 'Swedish Archives of Medicine' contains four papers: the first, of eighteen pages, by Professor E. Axel Key, on the diffusion of trichinæ in Sweden, to be followed in the next number by one in answer to the question: "Whence does the pig derive trichinæ?" the second, by K. Kjerner, of thirty-eight pages, on vaccinal syphilis; the third, by A. Wimmerstedt, of twenty-eight pages, a chemical examination of the mineral waters of Medevi, the oldest medicinal springs in Sweden; and the fourth, by E. W. Wretlind, of 156 pages, investigations respecting the mortality of Stockholm. In addition, part of a report on the instruction in the Carolinean Medico-Chirurgical Institute is given in an appendix.

From M. Wretlind's paper it would appear that the mortality in Stockholm is very great, varying during the last fifty years between 3·42 and 4·54 per cent., while in the other towns in the kingdom it has ranged from 2·27 to 2·98 per cent., and in the country between 1·89 and 2·27 per cent.; "Stockholm has thus had a mortality about double that of the country,

and one third greater than that of the other towns of Sweden." Of the fourteen years, 1851-1864, only in 1859, 1860, 1861, 1863, and 1864, did the births in Stockholm exceed the deaths. Compared with other *capitals*, too, Stockholm appears to nearly as great disadvantage. Of twenty enumerated by the author, "the majority of which are much larger than Stockholm, we find only Florence, whence the report, moreover, refers to only one year, having a higher per-centage mortality than our metropolis." The author concludes his very valuable and elaborate essay in the following terms:—

"The investigations now carried out respecting the causes of disease in Stockholm seem to justify the following statement:

"That the *climatic conditions* cannot be regarded as in any essential degree contributing to the great mortality, although the *spring weather* is unfavorable, and assists in producing or aggravating catarrhal diseases, especially in the lungs;

"That among the *local conditions* a very injurious influence must be ascribed to the *soil* being in many places, through defective drainage, highly *marshy*, causing damp in the houses, and thus contributing to produce catarrhs, rheumatism, tubercles, scrofula, and rickets; that the *products of putrefaction*, which partly in the gaseous form are developed from this soil, and partly enter in solution into the *spring water*, have an especially pernicious effect by giving rise to or aggravating nervous fevers, diarrhœa, and similar diseases;

"That it is, however, in the *social conditions* that the principal causes of the great mortality of Stockholm must be sought, among which, in men, vice, exhibiting itself in *drunkenness* and *unchastity*; among women, *insufficient remuneration for work* and consequent privations, especially with respect to food; and *among children*, particularly the illegitimate, *insufficient care or injudicious management*, must be placed in the first rank; that the *poverty* produced partly by the above-named vices, and partly by other causes, in connection with *miserable and unhealthy dwellings*, must also have a very great effect; and that, moreover, *intemperance* and a *dissipated tone of society* amongst the more wealthy, as well as *insufficient and defective care of the sick and of children*, depending on the corporation, must be looked upon as important co-operative causes."

ART. IX.—*Lectures on Animal Chemistry, delivered at the Royal College of Physicians.* By W. ODLING, M.B., F.R.S., &c., &c. Pp. viii, 165. London: Longmans, Green, & Co. 1866.

THIS little work is a commendable effort to point out the explanations which modern chemistry offers as to the functions

and processes of animal life. Professor Odling, commencing with the types on which compounds are constructed, shows how various organic bodies are mutually related, and how the recognition of the replacement, in certain apparently complex substances, of an element by a compound group, simplifies our view of their constitution. This simplification becomes still more striking where, as in so many instances, we can actually artificially build up from the elements bodies before known only as organic products. This process is generally performed in accordance with the inferences drawn from our insight into the internal groupings and structures of the bodies in question. Very happy are Dr. Odling's illustrations of the internal structure of hippuric acid, salicine, urea, and other important animal and vegetable products. He clearly shows that these and other complex organic bodies are built up of what we may call the "residues" of comparatively simple molecules, and that they may be referred to definite positions in such related series or groups of substances as those of the aromatic and fatty acids, of the alcohols or aldehydes, &c. The first two lectures of our author are occupied with these and kindred subjects; in his third lecture the relations of animal to vegetable functions are more particularly discussed, the general oxidation attendant upon the former being contrasted with the deoxidation effected by the latter. The spendthrift character of animal nutrition, in which the complex products elaborated by plants from very simple mineral bodies, are again more or less broken up, is dwelt upon with still greater detail in Dr. Odling's fourth lecture. Here we are told of the circulation of matter from the mineral to the vegetable, and through the vegetable to the animal; the animal again, in its turn, restoring during its life part of its oxidized products, namely, carbonic acid and water, to the plant; and finally after its death giving back to the earth other compounds, as phosphates, nitrates, and sulphates, just in the very form in which the elements they contain are best adapted to supply the necessities of vegetable nutrition. We get a clear notion of how the solar force is rendered latent and stored up in the separated oxygen, and in the carbonaceous tissues of plants, and how it is again liberated by the combustion of vegetable matters in air, whether in ordinary recognised processes of burning or in the processes of animal life. Thus, we see that the solar force, which is accumulated by vegetable organisms, is dissipated by those of animals. So far we follow our author in general and pretty close agreement, but when he proceeds to affirm that the hypothesis of (that which for want of another term we call the) vital force is baseless we cannot help demurring to his doctrine. We do

this, not because of any novelty in the doctrine, nor because it presents itself to us in a disagreeable light, but simply because it is only partly supported by evidence. It is all true enough to talk about the artificial and synthetical formation of hundreds of compounds once supposed to be incapable of construction save in the organisms of plants and animals. But what we conceive to be the most distinctive feature of organic processes is never, we believe, alluded to by Professor Odling. We mean that specific directive and determinative force (shall we call it?) which makes one plant store up its nitrogen in the form of caseine, and another in that of fibrine only; which makes one plant produce the quinine group of alkaloids, another the morphine group; which causes, in fact, the endless chemical differences between different plants. For another purpose also we are compelled to admit the existence of a power which we cannot comprehend—that specific originative power of growth and increase which we discern in all animal and vegetable forms. No one will now deny that plants and animals work in strict obedience to the laws of chemistry and physics; but it is the starting of specific processes and action, and the formation of specific products in obedience to these laws, which remain at present without explanation, and seem to elude our grasp. We have spoken, indeed, of these actions as due to a specific directive force, a force which *we* cannot at present wield at will, but at the same time we freely confess that this expression is only a convenient way of speaking of something which we do not wholly understand, but which, at all events, we see cannot be as yet explained by any combination of the known laws of chemistry and physics.

It will be impossible to review in so brief a notice as the present, the elaborate reasoning of Professor Odling concerning the sources of heat and energy in the body. Much valuable information is condensed in Chapter V, where the nature and results of muscular metamorphosis are discussed. Our author's data and his arguments are always worthy of attentive study, even where, as in the matter of the relative importance of nitrogenous and carbonaceous food, his conclusions are not in accordance with the most recent researches.

The concluding chapter of the work is partly occupied by expansions of certain general statements previously made, and partly by practical applications of animal chemistry.

ART. X.—*A Treatise on Emotional Disorders of the Sympathetic System of Nerves.* By WM. MURRAY, M.D., &c. London, 1866, pp. 118.

WE hardly know how to express our opinion of this little work. The subject is obscure, as is its title, and also its treatment. Judging from the style in which it is written, we must presume that it is intended by its author rather for the general reader than for the professional. Were it designed for the latter, surely Dr. Murray would not have thought it necessary to point out the situation of the solar plexus and ganglia, with the addition that *he* had ascertained it, as expressed in the following passage:—"I have ascertained that the solar plexus and its ganglia lie chiefly beneath the tip of the ensiform cartilage." And what confirms us in our opinion is that, in his preface, adverting to the impositions of the charlatan, to whom "emotional disorders" open so large a field for action, he expresses the hope, in which we can hardly join, that his treatise will be the means of protecting the sufferers from these ailments from imposition, and will "direct them to a rational source of relief."

Further criticism, we think, would be superfluous.

ART. XI.—*Training, in Theory and Practice.* By ARCHIBALD MACLAREN. London, 1866, pp. 202.

THIS is a remarkably well got up book; and with its frontispiece, an excellent photograph of the "Oxford University Eight" in harmonious action and full sweep, can hardly be otherwise than attractive.

Such was our first impression on opening the volume. After reading it we can say more in its commendation. We have no hesitation in expressing our opinion that it is a book which any one may read with advantage—good sense, sound judgment, and much knowledge being its distinguishing features. Nor do we think we are praising it too highly in adding that the style in which it is written, so clear and simple, is worthy of its matter.

Addressing our medical readers, who can hardly expect to find much information new to them in the author's pages, we would especially call their attention to the foot-notes, in which Mr. Maclaren gives some of the results of his own experience. For the benefit of those of our *confrères* who may not have

an opportunity of meeting with the work, we quote the following.

Treating of "true exercise," and of its effects, the author remarks—

"In a long pedestrian tour, extending over nearly four months, in which the average per day on foot exceeded nine hours, and usually with a knapsack averaging twelve pounds, I found the law of development in relation to employment strongly demonstrated. . . . Thus, the chest fell from forty-one to thirty-nine and a half inches, the upper arm from fourteen and a half to thirteen and three-quarter inches, the lower arm remaining unchanged at twelve and a half inches; the lower limbs, on the contrary, were vastly increased, the calf of the leg passing from sixteen to seventeen and a quarter inches, and the thigh from twenty-three and a half to twenty-five inches."

The next relates to loss of fluid from exercise. He says—

"In one hour's energetic fencing I found the loss by perspiration and respiration, taking the average of six consecutive days, to be about three pounds, or, accurately, forty ounces, with a varying range of eight ounces; of this, about thirty-six ounces remained in the clothing, the remainder having probably been lost in respiration, transpiration from the face and hands or evaporation from the clothing."

The next to the allaying of thirst:

"In cases of extreme thirst the best plan is first to cleanse the mouth and throat, either by rinsing it with a mouthful of water as a gargle or by chewing a morsel of bread or biscuit and spitting it out when lubricated, and then to swallow slowly a few mouthfuls of the liquid; again to repeat the rinsing and the chewing, for, the mouth and throat having been the place where the thirst was most severely felt, the salivary excretions at this time are unfit to be conveyed into the stomach; again to repeat the mouthful-by-mouthful mode of swallowing until the thirst has become a little allayed. By this means the water will be conveyed into the blood almost instantly, leaving no quantity to remain in the stomach. After a little space the luxury of a *hearty* drink may be freely taken. I have myself followed this rule under the fullest extremes of heat and thirst, . . . not only during special exercises which I was laboriously practising, but when traversing on foot in midsummer some of the hottest parts of Europe, when, as the natives complimentarily have it, 'everything was asleep or in the shade but dogs and Englishmen.'"

Though the title of the work implies that it is on training, without restriction as to kind, yet we soon learn that it is composed expressly for rowing—that exercise, which, above all others, whether bodily or mentally at the present time excites most public interest and individual emulation and ambition in

our seats of learning. Whilst, on the whole, Mr. Maclaren is deprecatory of the influence of this exercise, on the ground that it is incapable of fully and equally developing the bodily powers, and is inferior to other kinds in relation to wholesomeness, we are glad to find that he confidently holds that much good has incidentally resulted from the training which it requires, and that it has more than anything else, more than all the corrective and preventive measures of local authorities, whether of proctors or police-force, effected a beneficial change in the habits of our university men.

We may add that not the least valuable portions of the book are those in which the author exposes and controverts certain commonly received dogmas of trainers, especially those respecting "fat" and "sweating."

To those specially interested in the subject of training and boat-racing, the value of the work is enhanced by an appendix containing plans of boats, dietary, tables, &c.

ART. XII.—*Lecture Notes for Chemical Students, embracing Mineral and Organic Chemistry.* By E. FRANKLAND, F.R.S. London, J. Vanvoorst, 1866, pp. xx, 422.

THIS is no common book. Coming, as it does, from a master in the science of chemistry, it demands careful criticism. It neither is, nor pretends to be, a manual in the usual acceptation of that word; it does not describe in detail the physical and chemical properties of the more important elements and their compounds; processes and modes of preparation are given in barest outline; yet, in certain directions, this volume extends further than any other moderate-sized text-book. For Dr. Frankland displays, by several ingenious mechanical contrivances, what he conceives to be the constitution of hundreds of chemical compounds. And he does this in very many cases in such a manner as to connect related bodies together very plainly and evidently; especially is this the case in the domain of organic chemistry. There is so much that is new, or at least put in a new light, in the volume before us, that we must enter into some detail about the plan which the author devised, and the mode in which he has carried it out. When, in journeying through the course of instruction given by Dr. Frankland, we catch a glimpse of anything novel (shall we say startling?), we will stop and examine it.

Passing by the preliminary definitions in Chapter I, we come in the succeeding chapter to the difficult question of chemical nomenclature. On the whole, we are inclined to think that the

names adopted by Dr. Frankland are the best at present available; we prefer stannous chloride and stannic cloride for the old "protochloride" and "bichloride of tin" respectively to any other verbal expressions for the formulæ $SnCl_2$ and $SnCl_4$. The restriction of the term "acid" to certain hydrogen-containing bodies is, no doubt, a sound one, but we are not sure that our author's definition of an acid as "a compound containing one or more atoms of hydrogen, which become displaced by a metal when the latter is presented to the compound in the form of a hydrate," is altogether satisfactory. We doubt whether this condition is not occasionally fulfilled in the case of some aldehydes and alcohols.

Passing on to the consideration of another point, we find that the bracket is used in this volume with an unusual signification. It is intended to show that the elements placed perpendicularly with regard to each other, and next to the bracket, are bonded together. The following are a few illustrative examples of this function of the bracket:

$\left\{\begin{matrix}AsMe_2\\AsMe_2\end{matrix}\right.$ Kakodyle. $\qquad$ $\left\{\begin{matrix}CH_3\\CH_2Ho\end{matrix}\right.$ Alcohol. $\qquad$ $\left\{\begin{matrix}CH_2Ho\\CH\ Ho\\CH_2Ho\end{matrix}\right.$ Glycerine.

The first of these formulæ is intended to show that in kakodyle one of the bonds of one of the arsenic atoms is united to one bond of the other arsenic atom, while the two remaining bonds of each arsenic atom are united to two methyle atoms. So likewise with the formulæ for alcohol and glycerine, the carbon atoms are linked partly to each other, partly to certain atoms of hydrogen and hydroxyle (HO or Ho, as our author writes it). The one most characteristic feature of Dr. Frankland's treatise is, in point of fact, the very great importance which is everywhere attached to this matter of "bonds." Throughout the book hundreds of very complex and beautifully drawn figures occur, illustrating the number and probable connections of the several atoms of a compound. In all these diagrams the several constituents are linked together in accordance with the number of the active bonds in each constituent. Dr. Frankland calls elements having one bond "monads;" those having two, "dyads;" and so on. He speaks of the "atomicity" of an element in the same sense, though the word "quantivalence" is more precise, and though terms still more exact, such as "vinculance" might be coined to express this function of atoms. We cannot give any fair and full illustration of our author's special methods of viewing compound bodies, save by the use of complex diagrams; but by quoting his account of one of the

elements we may convey a notion of his general treatment of this part of his subject (page 60):

"NITROGEN, *Azote*, N_2.

"*Atomic weight* = 14. *Molecular weight* = 28. *Molecular volume* □□. 1 *litre weighs* 14 *criths. Atomicity* , *which, by the mutual saturation of pairs of bonds, becomes reduced to* ‴ *and* ′. *Evidence of atomicity:*

Nitrous oxide . . .	ON_2'.
Ammonia . . .	N'''.
Ammonic chloride . .	N^vH_4Cl.

"*Occurrence.*—In the free state in the atmosphere. In nebulæ? In combination in animal and vegetable bodies.

"*Preparation.*—1. By burning phosphorus in the air, whereby oxygen is removed from the latter.

"2. By passing air over ignited copper, when the oxygen unites with the copper.

"3. By heating ammonic nitrite . . . :—

$$N'''O(N^vH_4O) = N_2 + 2OH_2.$$

"4. By passing chlorine through an excess of solution of ammonia:

$$8NH_3 + 3Cl_2 = 6NH_4Cl + N_2."$$

While referring those of our readers who are interested in philosophical chemistry to the volume itself, we may sum up our opinions of its merits and demerits in a very few words.

The reaction given of processes of formation are particularly exact; the descriptions of substances are always accurate, but often meagre; the aspect under which compounds are viewed is generally one-sided, and so, while presenting an appearance of consistency, often ignores the well-ascertained relation of allied bodies. In the sulphates of zinc, for example, Dr. Frankland puts out of sight the existence of the old radical or transferable group, SO_4, and, introducing a new radical, ZnO_2, formulates the normal and basic zinc sulphates thus:

Zincic sulphate . . .	SO_2Zno''.
Tetrabasic zincic sulphate .	$SOZno_2''$.
Hexabasic zincic sulphate .	$SZnO_3''$.

It seems inconsistent to retain the name sulphate for three salts in which there is no common term which that name can

represent. As long as this name is used a corresponding formula must contain some such group as SO_3 or SO_4:

ZnO, SO_3 or $ZnSO_4$.
$2ZnO, SO_3$ or $ZnSO_4, ZnO$.
$3ZnO, SO_3$ or $ZnSO_4, 2ZnO$.

On the whole, we are inclined to regard this book of 'Lecture Notes' as a very valuable addition to our text-books; but at the same time we are far from agreeing with all its theories. While it throws much light upon the relationship of minerals and the metamorphosis of organic bodies, it often seems to generalise too hastily concerning the functions of the atoms in such bodies, and to affirm too rashly a definite structure for them. Young students will, unless cautioned, be misled by these bonded elements, worked, as they are, into such attractive combinations (called "glyptic" formulæ), especially now that these formulæ are no longer confined to paper diagrams. Sets of coloured balls with diverse links representing various elements univinculant, bivinculant, and so on, are now used in lecture illustrations. It is easy to think that these balls mean more than they are intended to teach—that they purport to represent the actual position of the constituent atoms. Yet these glyptic and graphic representations have their uses, though they must be employed with due caution. So fascinating are these glyptic formulæ in their tangible representations in the form of brightly coloured and silvered balls joined by brass rods and elastic cords, that "a very young philosopher might excusably convert them to purposes of purely recreative science."

We may add that the india-rubber connections have sometimes to be stretched to their fullest extent to express Dr. Frankland's very elastic bonds.

Art. XIII.—*Studii Clinici sulle Malattie Mentali del dottore Cesare Lombroso*, di Pavia. 1865.

Clinical Studies on Mental Diseases. By Dr. C. Lombroso, of the University of Pavia.

La Medicina Legale delle Alienazioni Mentali studiata col metodo esperimentale. Saggio del Dottore Cesare Lombroso. 1865.

The Medico-Legal Aspects of Insanity. By the same Author.

The first of these pamphlets contains five cases carefully, but not tediously, detailed, and of great clinical interest. The first one is a case of aphasia in an epileptic woman; the second is styled acute mania from extreme ossification of the sutures;

the third, aphonia with congenital idiocy from atrophy of the anterior lobes; the fourth, mania pellagrosa; and the fifth is a case of general paralysis.

The following is a brief account of the first case, viz., aphasia in an epileptic.

A woman, of sound parents and good constitution, lived till she was twenty-five in good health, when she had an epileptic attack while menstruating. She had no return of it for seven years, but her mind was rather dull. When she was thirty-three the epilepsy re-appeared, the attacks occurring every fifteen days, and being aggravated during menstruation. In one of these attacks she fell into the fire and burned her right hand. A curious fact connected with them was that, during the period of excitement, the urine diminished in quantity nearly half, its specific gravity rose ten degrees, it was loaded with urates and phosphates, and twice albumen was also found. Her mind was much impaired, and she had almost total loss of memory and of the use of words. She could not answer a question, sometimes would pronounce such words as bed, bread, &c., but had generally only one or two phrases which she would repeat.

At the autopsy the bones of the cranium were found much thicker than usual, and in the frontal region there was on the inner surface a remarkable deposit of calcareous substance. The skull measured only 18·8 inches in circumference, 7·2 in its longitudinal and 5·2 in its transverse diameter.

The weight of the brain, with the cerebellum and medulla, was 36 oz. avoirdupois, *minus* 16 grains. The author thinks that the small size of the head resulted from the unusual ossification of the sutures, and that this interfered with the nutrition of the brain. He also considers that this case is opposed to Broca's views as to the localisation in the brain of the faculty of speech, as both lobes of the brain were atrophied, and the right the most, while he mentions several writers who have related instances of disease in the brain with aphasia, far away from M. Broca's convolution. For instance, E. Cruveilhier found aphasia with disease of the left corpus striatum, Bennet found the same with atrophy of the convolutions of the right hemisphere, and Barthe had a case due to hydatids in the left ventricle.

The next case, the third, is also one of interest, and is abridged as follows:—M. B—, a male, æt. 38 at time of death, with this history:—His parents were healthy, but from birth he showed a singular want of intellect, took the breast with difficulty and could not utter a cry. He could only succeed in learning to walk imperfectly. No change occurred at puberty; he never spoke, but made noises when it rained, walked with very tottering

steps, and later only ate when forced, showing no dislike to the most nauseous substances. His hearing seemed pretty good, but not so his sight.

Autopsy.—The measurements of the cranium were in circumference 19·4 inches, ant.-post. diameter 7·3, transverse ditto 5·6, and vertical height 4·6.

On the anterior surface of the petrous portion of the right temporal bone was a small pedunculated tumour, the left petrous bone was smaller than the right, and without the elevations corresponding to the semicircular canals, while the upper semicircular canal on this side was narrow and atrophied.

The left hypoglossal, the olfactory, and optic nerves (as well as the eighth, ninth, tenth, and eleventh, according to Italian anatomists) were atrophied and like threads of silk. The left meatus auditorius was quite filled with wax.

The weight of the cerebrum was 39¼ oz. avoird., that of the cerebellum 5¼. The cerebral substance was denser than usual and of a dull yellowish colour. But what was of the most importance the convolutions of both anterior lobes were smaller than the posterior, and the sulci hardly apparent. The microscope showed a deposit of pigment in the multipolar cells of the brain and medulla oblongata.

The author thinks that the primary disease was atrophy of the anterior lobes, and that the nerves were affected subsequently. The small volume and atrophied appearance of the anterior lobes are very remarkable in the engraving which illustrates the case.

We are glad to see such good fruits of the clinique on mental diseases as these cases afford, and can wish that a similar course were adopted in this country, where, though we possess far greater advantages in every way in the treatment of the insane, the student is debarred from any opportunity of watching the phenomena of diseases of the mind.

Dr. Lombroso, who has charge of the *clinique* for mental diseases at the famous University of Pavia, has evidently worked hard at his cases, and investigated them most industriously. The weight of his patients, the condition of their hair, teeth, and urine, the measurements of their skulls, their motor and sensory affections, intellectual state, and the etiology of their disease, all come under his notice in succession, and he has given the results of his labours to the public under the above title.

Work of this kind is very valuable for its own sake, though it is not sufficiently practical to be generally appreciated. We must be content to give an outline of the conclusions arrived at by the Italian professor, after his elaborate investigations.

The weight, he says, of an insane man is less than that of a sane one of the same stature and condition. Dementia, pellagra, and mania, diminish the weight of the body, and dementia the most. Patients in acute mania lose weight independently of the conditions of respiration and alimentation.

The hair of the insane often undergoes changes of colour, and they become prematurely grey or else bald. In an epileptic woman it was noticed that the hair stood erect on her head during paroxysms of fury,

The teeth are generally irregular or deficient, and more often so with the demented.

The attachment of the ear to the head and the form of the helix are often irregular in chronic mania and dementia.

The globe is often observed to have a continuous lateral movement.

The specific gravity of the urine is lower than usual in those suffering from pellagra and in melancholics; it is normal in cases of mania, and in the demented it rises to a remarkable degree, especially at the onset of attacks of excitement. In these cases the urine diminishes much in quantity, and the proportion of urea, phosphoric and sulphuric acids increases, while in some cases albumen is present. The urine is acid in most cases of mania, and very acid in those who are furious, but this acidity soon gives way to an alkaline condition. The most interesting of these observations, however, are the cranial measurements made in a considerable number of cases. From these the author concludes that the healthy individuals of each Italian province have certain special cranial characters which distinguish them from the inhabitants of the neighbouring provinces. These characters consist in certain proportions of the longitudinal as compared with the transverse diameters, which vary very slightly with the stature, and education of the individual, whilst they are subject to considerable differences under the influence of insanity. The demented and idiotic have a tendency more especially to the "ultra-brachiocephalic" type, the male sufferers from mania to the "dolico-cephalic," while monomaniacs approach nearly to the moderate brachiocephalic. The capacity of the cranium varies in the several provinces, but more still according to individuals or intellectual culture. It is absolutely less in all the subjects of mania, more markedly still in the demented and idiotic, and especially in demented epileptics. These facts are amply illustrated by an elaborate table showing the relative measurements of the various diameters of the skull, and giving also the height of the individual and his place of birth, and these observations have been made on a series of sane as well as insane people for purposes

of comparison. Thus, for instance, a certain diameter is found to measure 771 millemètres in a native of the Abruzzi, while it is 767 in a Neapolitan, and 814 in a Milanese. There are many obvious objections to attaching much importance to such differences, but a series of careful observations conducted honestly and on a scientific basis are entitled to attention, and the more so when they are put forward simply on their own merits and not to support any favorite theory. There are other tables in this little work, dealing with statistics as to hereditary influence, causation, &c., and a chapter on criminal lunacy; but the points dwelt on have often been brought under notice in this country before.

ART. XIV.—*Reports and Papers of the Metropolitan Poor-law Medical Officers' Association.* 1866 and 1867.
Reports and Papers of the Association for the Improvement of London Workhouse Infirmaries. 1866 and 1867.

IT is our intention in our next number to consider at length the measures taken and contemplated for improving the management of the medical relief of the poor in London. The two associations, the published papers of which are before us, have done good service in the agitation which has happily terminated in the passing of the "Metropolitan Poor Act" of the present session. These associations, indeed, were not the first in the field. Much had been already effected by the inquiries of D. Stallard, and of the 'Lancet' Sanitary Commission, consisting of Mr. Ernest Hart, Dr. Anstie, and Dr. Carr. It will be our endeavour when discussing the history of this successful agitation to give their due share of praise to all concerned in it. The two associations above mentioned differed in this, that the one is a professional and the other essentially a non-professional organization. Both, however, dealt with similar facts and grievances, although, as respects the former, other objects, secondary to the common object of both, were necessarily held in view.

ART. XV.—*The Restoration of Health; or, the Application of the Laws of Hygiene to the Recovery of Health: Forming a Manual for the Invalid, and a Guide in the Sick-room.* By WILLIAM STRANGE, M.D., &c. London, 1865, pp. 434.

THIS work contains a good deal that is useful, and, as it

seems to us, not a little that is superfluous. The portion which is physiological and, we are disposed to add, that which is pathological, might, we think, have been omitted with advantage. A very superficial knowledge of each of these difficult and abstruse subjects is more likely to bewilder the mind of the general reader than to enlighten it. It is surely but a vain attempt to indoctrinate in the higher branches of medical science those, such as the general public, who have not had the preliminary education requisite for the understanding of the merest elements of them, and, if ignorant of anatomy—as the public must be presumed to be—of understanding even the greater number of the terms employed. As regards the junior members of the medical profession, to whom the author thinks his work may be useful, we are also not without our doubts. To answer the purpose designed in their case the information afforded is hardly sufficiently minute or exact; and defective indeed must be the condition, and limited the reading, of those practitioners who can from this volume derive much instruction.

The 2nd and 3rd parts, having for their titles, "Nature, Causes, and Prevention of Diseases," "Management of the Sick-room," and "Convalescence and Restoration of Health," these comprising nearly three fourths of the entire contents, have, we think, better claims to attention, and are likely to be of more service, especially to nurses, whether professional or non-professional, and to such invalids and convalescents as are in no danger of becoming hypochondriacal, and of being too observant of their feelings. The majority of the rules laid down are judicious, and display an accurate knowledge of the ordinary wants of the sick, and of the desiderata in nursing, such as might be expected from an experienced hospital-physician.

The style adapted by the author is easy, flowing, and popular; perhaps occasionally too figurative, and also prolix, and too little idiomatic. Here is an example of the former. Adverting to the sympathy, which he calls universal, between the stomach and all other organs, he remarks:

"It both feels with them in distress, and it takes care to make them participate in its *own* troubles. It is not at all inclined to mourn in silence, and meekly put up with the 'wrongs' inflicted upon it; but it makes its lamentations heard by its neighbours far and near, and draws them all to participate in its quarrels, thus putting force upon the offending owner to compel him to alter his conduct."

Now, apart from the mode of expression, we might object to

this on the ground of exaggeration of statements. It is an undoubted truth that the stomach is a sympathetic organ; but it is equally true that it is an organ possessed of little sensibility, is capable of enduring much and with little derangement of the general health. Instances exist of sudden death in a person, well fed, and in previous robust health, from the bursting of an ulcer in the stomach, the existence of which had not been suspected, followed by the instant escape of the contents of the organ into the abdominal cavity. Even in the act of vomiting, as is now well ascertained, the stomach is passive, excepting so far as being the source of that reflex action on the parts adjoining which are principally concerned in the action.

In treating of consumption, the author indulges in the same style, and, as it appears to us, inaccuracy of statement. Speaking of the tubercular or consumptive tendency, he remarks:

"We have the disease *before* there is any disease, so to speak. We see mischief impending—functions deranged, organs excited or debilitated, the general nutrition of the body failing—before we can detect any actual disease, as marked by change of structure, or even of function in the lungs. But we know that the slayer is creeping steathily on; we see the toils gradually encircling his victim closer and closer; and before he has touched him with his full arm we know that his fate is sealed."

Now, is it not true that, in a large number of cases, the organic elements of the diseased tubercles form with little derangement of the general health, and that until the disease is advanced there is little emaciation? As in the sheep, so in man, tubercles, previous to the softening and suppurative process, do not seem incompatible with a certain degree of fatness.

Should the work reach a second edition, we take the liberty of expressing the hope that it will undergo a careful revision. We have marked passages which bear signs of carelessness; we may notice some of them. Under the head of clothing the author recommends that "woollen under-clothing should not be worn longer than three or four days without its being at least well dried before a hot fire to draw out the animal matter." Surely he must be aware that the animal matter, if it means anything besides moisture from prespiration, cannot be so expelled.

When noticing food for the sick, he says—

"I am not desirous of fostering the fashionable slang of pseudo-science, which professes to see important differences in articles of

diet which to all intents and purposes are the same, only varying in their acceptability to individual persons. Thus, there is absolutely no choice to be made amongst such things as rice, sago, tapioca, semolina, American corn-flour, &c., except in so far as the patient may fancy one of them more than another."

Now, though all the starches are very similar, from whatever vegetable obtained, the same cannot be said of flours. American corn-flour, the flour of maize, is, as is well-known, very compounded, starch being only one of its nutritive ingredients, and the same remark applies to rice and semolina.

We have marked other passages which seem to us to require the author's reconsideration. One more we will give for the sake of comment. He says—

"The most typical specific diseases are smallpox, chicken-pox, measles, scarlet-fever, whooping-cough, ague, yellow-fever, typhus, Asiatic cholera, and some diseases of the skin. Every one of these diseases is propagated by germs or atoms of matter which arise, or have arisen, under various conditions of the human body or of the soil or the atmosphere. Each disease is caused only by the application of *its own* peculiar germ, and in most instances these germs are multiplied in the body in the progress of the disease, and passed on from one individual to another by means of infection."

Ague we see included in these specific diseases, as if it were infectious, and capable of being propagated from one person to another by germs! The bringing together, as of one class, diseases so different as those named, and these obeying laws seemingly so diverse, appears to us, at least, inconsiderate, and, to say the least, a mark of carelessness. At the present time it is particularly advisable that there should be correct information diffused respecting diseases often giving rise to panic—cholera, for instance, and yellow-fever—and to the enactment under the influence of undue alarm of contagion of quarantine regulations, injurious to commerce and harassing to passengers and travellers.

The task which the author has set himself in a work so comprehensive as that before us, it must be acknowledged, is one of no small difficulty, and requiring for its completion no ordinary qualifications. What it most needs for its success, from a mere utilitarian view of the subject, is a quality least easy of attainment—the power of enunciating principles or rules for guidance, analagous, if we may use the expression, to sailing directions for the navigator, of unquestionable accuracy, axioms in brief of science, and expressed in the simplest and clearest language, unobscured by theoretical explanations. It is only after this manner, we think, that the

denunciation of popular medicine being an abomination—the saying of an earnest American writer—can be avoided.

ART. XVI.—*A Winter in Paris; being a few experiences and observations of French Medical and Sanitary Matters gained during the Season of* 1865-6. By FREDERICK SIMMS, M.B. Lond. London, 1866, pp. 151.

THIS little book may be of some use to those medical students who, acquainted with the French language, are desirous of availing themselves of the advantages which the Parisian hospitals and the great medical school of the capital afford. The volume is freely and pleasantly written, but also somewhat carelessly, indicating the want of a careful revision. Had it received that attention, we should hardly have met with the following. Adverting to the inferiority of French surgery, as compared with English, Dr. Simms says—"I could not refrain from asking myself what Fergusson would have said had he seen a pupil of his remove a simple fatty tumour from the thigh by ablution (*sic*), skin and all, instead of by a dissection out of the disease itself." Or where, in another place, he records how "A coachman, who had been eight hours wet through, showed us that the cutaneous inhibition (*sic*) of large quantities of water can produce a pemphigus!" The note of admiration is ours—not so much for the word, as the sentence. The greater part of the volume is occupied with a description of the hospitals; these, including the *special* (but not comprising the *hospices*), twenty-two in number, interspersed with medical remarks, most of which, we think, might have been omitted.

His accounts of these establishments is, on the whole, tolerably favorable. The manner in which they are conducted and provided for on the centralisation plan, independent of subscriptions, seems to be their best feature compared with our great hospitals. Another favorable feature which they have is the ample open spaces and ornamented grounds by which they are surrounded. Their greatest defects seem to be their crowded state and imperfect ventilation.

If one portion of our author's pages is more interesting than another, it is that in which a sketch is given of the history of the Hôtel Dieu, the largest and oldest of all the hospitals of the city, and, indeed, one of the earliest, if not the very earliest, of European hospitals. Before the French Revolution, judging from the reports on it by the Commission, of whom the illustrious and unfortunate Lavoisier was one, it was a scene of horrors—of so

many, various, and intense, as hardly to be credited, were they not so well authenticated. Its reform began in 1791, and has been continued ever since, an event that may be ranked amongst the compensating circumstances of that period of terrorism. Besides a description in some detail of the hospitals, the author has given an account of the "School of Medicine and of Medical Education," and also a brief "Notice of the Sanitary Arrangements of Paris." Each subject occupies sixteen pages out of the entire 151. Neither, we hardly remark, is exhaustive. Each may be consulted with profit by those interested in the subjects, and who have not larger sources of information to refer to.

ART. XVII.—*Degli Innesti Animali e della Produzione Artificiale delle Cellule: ricerche sperimentali.* Del Cavaliere PAOLO MANTEGAZZA, Professore dell' Universita di Pavia, &c. &c., con disegni dal Sign. G. BIZZOZERO, Assistente Incaricato. Milano, 1865.

On the Transference of Animal Tissues and Artificial Cell-production, in a series of experiments. By the Chevalier PAOLO MANTEGAZZA, Professor of the University of Pavia, &c. &c., with figures by Sign. G. BIZZOZERO. Milan, 1865, pp. 80.

IT is impossible by a brief abstract to do anything like justice to the admirable investigations of this well-known experimenter. We cannot too strongly recommend them for appreciation and study, as set forth in these brief pages. They include 287 experiments, of which no less than 242 are devoted to transplantation of various organs in animals of the same or other species; and the remainder to insertions of blood and fibrine within the textures, in relation to the question of the spontaneous growth of cells or their production from tissue and plasma. Of the larger group no less than 208 experiments refer to transplantation of the testicle, chiefly under the skin or into the abdominal cavity, and these are performed for the most part on batrachians. It will be seen that one unfortunate frog had forty testicles engrafted on him, with no accession to his constitutional strength, for somewhat more than in the others the muscles on which they rested became subject to the process of fatty degeneration. The best time for this class of experiment with frogs is in the winter season; they are far less successful after the period of fecundation is gone by. When included beneath the integuments in this species, male and female alike,

the testicle seems at first to live by endosmosis; it may then be pushed about from side to side, but soon it contracts firm adhesions with formation of vessels; sometimes a perfect pouch is formed, with a minute outlet, but the experimenter was never so fortunate as to perceive the exit of zoosperms from such an orifice. Transplanted into the abdominal cavity, the testicles form adhesions to the peritoneum, in which the new vessels are so considerable as to be sometimes discernible by the naked eye; in other cases their existence was placed beyond a doubt by filling them with coloured injection from the heart. The process of formation of these vessels and of the connective tissue in this as well as in other experiments of the kind constitutes a very advantageous field of study for the histologist. It is remarkable how very seldom putrefaction and septicæmia complicate these experiments. On the other hand, fatty degeneration is prone to occur, not only in the implanted viscus, but also, though in less degree, in the muscle on which it may chance to rest. In the mammiferæ the result of identical experiments is far different, the same proceeding with them leading to suppurative acts. In one more fortunate case than the rest, of transferred testicle in a guinea pig, fatty degeneration with loss of volume was the result, and very probably in course of time all trace of the organ would have vanished.

Not less interesting in character are those experiments which Signor Mantegazza has performed on the stomach and intestines of frogs. He discovered that the whole gastro-enteric tube can live for a month and a half under the shelter of another organism, drawing life and sustenance from it through the medium, as it would appear, of vessels and connective tissue. It is interesting to find the stomach completely severed from its attachments and included in the belly, yet preserving for at least twenty-seven days its full integrity and the faculty of secreting a digestive fluid. An independence of nervous influence is so far proved as concerns this class of creatures. In fact, the stomach was generally found to be vastly distended with mucus, so much so as in some cases to detach the confining ligatures at their extremities, or to seem at the point of bursting. The digestive power of the gastric secretion was always found to be perfect.

Next in interest to the experiments of transplanted testicle seems to us those which Signor Mantegazzi has performed with the spleen. In the character of autonomy this organ seems to stand nearest to the testicle. Not only does it live under the artificial conditions which we are describing, but it gains in weight, presenting unimpaired after many months the exact identical anatomical structure it possessed when separated from its

original attachments; similarly with the testicle, in its novel site it contracts adhesions of a fibrous or vascular character. Such, at least, is the case in batrachians; in the mammiferæ it was different only in an exceptional case, where vascular adhesions formed; in this instance the animal died from septic poisoning; in some other cases, after a few weeks' term, the spleen had wholly disappeared, absorbed, no doubt, after the occurrence of fatty degeneration in its structure, for fat is generally found upon the spot. This tendency not only occurs in animals of the same kind, for it equally happened when a rabbit was engrafted with the spleen of a guinea-pig; in this case it disappeared completely with the complication of a little pus at the very first. So far, however, is this from being of necessity, the professor thinks it not improbable that a spleen might remain indefinitely without change, fixed and adopted into the organism where it had been implanted. The transplantations of brain substance made by Professor Mantegazza in the frog are chiefly remarkable for the quantity of pigment, either under the form of round or oval cells or in that of the more common stellate and irregular shapes; such a condition is a frequent result; it might, indeed, be called a pigmentous degeneration. Amylaceous degeneration, we can only say, stands in need of further proof than the instance given; that of fat is common enough, and a certain amount of connective tissue often characterises the spot of transplantation. The spinal marrow of a rabbit, inserted under the skin of its fellow, doubled in weight in nine days' time, and contracted such firm adhesions as were hard to separate, proving a strong attraction at least of the nutritive elements of life to that spot. The muscles, when transplanted in frogs, are found to preserve their contractility for months. They gain in weight, the fibrine diminishes with increase of fatty matter; indeed, they mostly undergo fatty degeneration; the same occurs to a less extent in the part on which they rest. Virchow observed much the same thing in cysts of the extra-uterine fœtus left for many years in the body of the mother; the fat was seen to be principally in the cyst and in the tissues most closely in contact with the vessels of the parent. The tongue, when transplanted, is subject to the same law as other muscles; its cilia were once observed in movement by Sr. Bizzozero after eleven days' incarceration. More than any other muscle the heart resists and defends itself from this process in transplantation, especially with the higher class of animals and in widely divergent species, yet a small heart transported into an animal of disproportionate size and weight would probably end by being absorbed. Ollier has so far exhausted the subject of transplanted periosteum that nothing on that

head need be said. The most important fact in the implantation of bone is the excessive rapidity with which the periosteum becomes surrounded with a capsule of connective tissue adhering to the surrounding textures, and forming a bed for numerous new vessels.

The conclusions at which Professor Mantegazza arrives are, that there is a reciprocal action, a vital catalysis, if such an expression may be allowed. The graft receding more and more from its individual bent, becomes adopted into a new circle of existence, as the organism which is its receptacle is brought under its influence. The tissues operate mutually by contact; the host and guest arrange an understanding for their common benefit, neither of them being subjugated and neither wholly passive. The death of the engrafted part by putrefaction is rare, but a perfection of tolerance, it must be observed, exists at the expense of nobility in species, or rather the degree of tolerance is in inverse proportion to superiority of rank and capacity in the zoological series. The tissues and organs which display most of autonomia in the batrachians are the periosteum, the spleen, and testicle, and the spur in the cock; those which display least of it are the liver, brain, spinal marrow, and nerves; the last increase in weight, but soon lose their striated and transparent appearance, undergoing the fatty metamorphosis. The lymphatic vessels play a very active part in the histological changes which occur in transplanted tissues and organs, in further proof of which the neighbouring glands often attain an extraordinary size. It is common for transplanted tissues to increase in weight before being absorbed. Putrefaction is favoured by a high rate of temperature and by weakness of the animal on whom the organ is engrafted, and, above all, by divergence of species in the subjects of the dual experiment.

Professor Mantegazza has devoted a section of his treatise to impugning the well-known Berlin doctrine *omnis cellula à cellutâ*, which in an especial manner he revolts from. The experiments undertaken in this direction are forty-five in number. They consist in occlusion within the animal organism of blood and artificially pure fibrine, and in one case also of albumen. The fibrine, he says, in fourteen to twenty hours' time, breaks up into granules which become nuclei, organisation always taking place from without inwards in the transplanted mass. Some of the cells grow from a large nucleus from which loosens out a thin wall, while others are formed by one or more nuclei round which a membrane gathers; these centres of attraction rapidly increase the production of material in the fibrine mass, and, finally, the structure of the tumours becomes so rich in vessels as to be easily injected. In an experiment we will

notice, No. 273, injected blood had been put aside by the professor for one day before insertion; he squeezes some fluid from the wound two days after the experiment, and finds very large cells, 0·011 to 0·020 millimètres, which contain one, two, three, or even seven nuclei. These nuclei have a diameter of 0·0041 to 0·0072, and some of them are evidently formed out of blood-globules included in the cell, a fact of which he is perfectly assured. In some experiments of tying the jugulars of rabbits he observed the copious formation of nuclei and cells in the fibrine within the vein, with disappearance of blood-globules. In the great difference which exists between the behaviour of blood included between ligatures in a vein or artery, he was enabled to make important observations confirmatory of his idea and subversive of those of Virchow. In the vein the more rapid coagulation of the blood, and formation of enormous granular cells, with the limitation of the pus to the point of ligature, are points on which he insists, while in an experiment where he removes and subsequently replaces the carotid of a dog the animal gets well without production of inflammation or formation of pus, and he asserts that in such a case the blood is converted into connective tissue without formation of any kind of cells or nuclei. The disappearance of the blood-globules and their diminution in size, the early granulation of the fibrine and loss of fibrilization, are points which he brings most prominently into notice. The nuclei and granulations he attributes strictly to the fibrine.

ART. XVIII.—1. *On the Rational Employment of Mercury in the Treatment of Syphilis.* By Dr. COLOMIATI MEREDYTH. London, 1866, pp. 41.

2. *On the Mercurial and Non-Mercurial Treatments of Syphilis.* By R. WILLIAM DUNN, Surgeon to the Farringdon Dispensary, &c. London, 1866, pp. 48.

As regards original research and the advance of medical science, after a perusal of each of these publications, we do not see that either of them is required, inasmuch as in neither of them are there any new facts brought forward, or even speculative views having any pretensions to novelty.

Dr. Colomiati Meredyth is a special pleader in favour of the rational employment of mercury in the treatment of syphilis. His main argument is that it hastens the removal of the disease. His reasoning on the subject is not always, as it appears to us,

rational or logical; nor is his style of composition of the purest, being too often stilted and grandiloquent, and not always correct.

Mr. Dunn's production is of a more modest and temperate kind; and as regards composition has none of the defects of its more ambitious rival, if we may so designate it. In comparing the two modes of treatment (the mercurial and the non-mercurial), he trusts chiefly to statistics, and on them founds his conclusion in favour of the non-mercurial, as the safest, though not always the most expeditious.

No subject, we need hardly remark, has during the last half century given rise to greater difference of opinion, and excited more controversy than the disease in question, as regards its nature and treatment, or, it is a comfort to think, with greater advantage. Many among us can remember the horrid effects of mercury indiscriminately given, and have witnessed the cessation of them gradually, in proportion as a milder and non-mercurial mode of treatment was adopted, and this with greater security in relation to consequences, such as too often followed the administration and abuse of mercury.

ART. XIX.—*Medicinskt Archiv, utgifvet af Lärarne vid Carolinska Institutet i Stockholm.* Redigeradt af E. A. KEY, Professor i Pathol. Anatomi; C. J. ROSSANDER, E. o. Professor i Chirurgi; A. KJELLBERG, Adjunct i Pædiatrik. Tredje Bandet, Första Häftet. Stockholm, Samson and Wallin, 1866.

Archives of Medicine, published by the Teachers in the Carolinean Institute in Stockholm. Edited by E. A. KEY, Professor of Pathological Anatomy; C. J. ROSSANDER, Professor Extraordinary of Surgery; and A. KJELLBERG, Adjunct in Pædiatrics. Third Volume, First Part. 8vo, pp. 194, with two Lithographs.

WE have in former volumes noticed the previous numbers of the above periodical, and mentioned the scope and design of the work. The present fasciculus contains three lectures, extending to 105 pages, by J. V. Broberg, upon animal magnetism and the mysticism of the eighteenth century; a paper of fourteen pages, by Sven Sköldberg, upon beef-tea as an article of diet; one of fifty-four pages on malformations in the urogenital apparatus by fissure of the inferior abdominal parietes, illustrated with two

plates, by Dr. Georg Asp; one of ten pages, on the pathological anatomy of the ear, by M. V. Odenius; and a report, extending to eleven pages, on the instruction given in the Carolinean Medico-Chirurgical Institute during the scholastic year 1864-65.

The care usually taken in the preparation of papers for this journal is evidenced also in the present number; the plates, too, are well executed.

The number of students in medicine attending at the institute during the year was 140.

Art. XX.—*Vaccine et Variole: nouvelle étude expérimentale sur l'identité des deux affections.* Par Messrs. Chauveau, A. Viennois, et P. Mignet. 8vo, Lyons and Paris, 1865, pp. 104.

Vaccinia and Variola: an experimental inquiry into the identity of the two Affections. By Messrs. Chauveau, A. Viennois, and P. Mignet. 8vo, Lyons and Paris.

De la Spontanéité et de la Specificitié dans les Maladies. Par P. Em. Chauffard. 12mo, Paris, 1867, pp. 229.

On Spontaneity and Specificness in Diseases. By P. Em. Chauffard. 12mo, Paris, 1867.

We have been induced to group these two works together from the opposition we find in the tendency of the doctrines they convey. A complete system of ideas might be gathered from them which is but imperfectly taught in the pages of each. After study of the Lyons work one is inclined to indulge the idea that contagious diseases are everlasting; from the other, the idea is overdone that they come and go like ephemera. M. Chauffard is inclined to believe that the most afflictive of this class are precisely those of most recent evolution. As it seems to us, there is room for both shades of opinion. We read of hydrophobia in Homer, and we see contagious ophthalmia grow up from a sirocco wind in Malta or in a close hot barrack-room in England.

The former of the works before us rests on a vast body of experiment; the other bears the reader along with a considerable weight of argument. It is not difficult to say which of the two will be most approved, when deduction from experiment is the reigning and most accepted mode. In fact, the ideas we have derived from Jenner and Ceely as to the identity of variola and vaccinia

have received a strong shock in the experiments made under the Lyons commission, and nothing can be more profitable in instruction than their careful study in these pages. They show how a contagious principle can pass without changing its essential nature through the frame of animals different in species, and how imperfectly during these circuits the aspect and amount of cognizable phenomena are to be trusted in characterising them. It is one of those rare instances in which attention to antecedents serves us better than the employment of our senses.

ART. XXI.—*On a New Process for Preparing Meat for Weak Stomachs.* By W. MARCET, M.D., F.R.S. Pamphlet. 1867.

THE difficulty experienced by the stomach in various forms of disease in digesting meats, especially in cases where milk does not agree, suggested to Dr. Marcet the advisability of submitting cooked meat to some process similar to that which it undergoes in the stomach. After sundry apt and instructive remarks on the phenomena of digestion, our author shows that, by cooking, our meat is subjected to a process which raw food has to undergo in the stomachs of carnivorous animals. Alluding to the fact of hydrochloric acid being the acid of gastric juice, he proceeds to describe the process which he has to recommend, and which consists of digesting cooked meat in some degree by means of this acid, pepsine and carbonate of soda, previous to its being eaten; thus extending the preliminary digestion of meat one stage further than is done by cooking (*i. e.* softening and dissolving it). We must refer the reader to the details of the process as given in the author's pamphlet. By it a fluid having a strong and peculiarly pleasant flavour of roast meat and holding in suspension, a light, pulpy, and savoury substance is produced, to which water, pepper, and salt may be added according to taste. Strips of calves' stomach may be used instead of pepsine. We can have no doubt that so rational a plan of preliminary digestion of food will prove in many cases most worthy of trial.

PART THIRD.

Original Communications.

Art. I.

The Etiology of Insanity.[1] By J. Crichton Browne, M.D. (Edin.), Medical Director of the West York County Lunatic Asylum, late Medical Superintendent of the Newcastle-on-Tyne Borough Lunatic Asylum, and Lecturer on Medical Psychology to the Newcastle College of Medicine.

Insanity is in all cases essentially dependent upon dynamical or physical changes in the vesicular neurine of the encephalic centres. In the first case, it is a modification of vital activity connected with physical changes which are too minute and delicate to be appreciated by our senses, even aided by scientific instruments; and in the second, it is also a modification of vital activity, but dependent upon an appreciable and therefore a far more serious lesion. In either case it may arise from agencies operating directly upon the nervous centres themselves, such as an abuse of functional energy; from general systemic conditions, such as anæmia or tuberculosis; or from derangements of remote organs or parts, such as the ovaries or extremities. It must not be supposed that insanity is anything more than a modification of healthy processes. It is no new principle instilled into the being affected—it is no foreign growth grafted upon the mind, but only an altered arrangement, relation, or degree of the ordinary phenomena of consciousness. The stupor of dementia finds its analogue in sleep; the pangs of melancholia are the very sentiments which most of us daily experience, heightened and separated from their natural relations. We must look upon insanity, then, as a decomposition of thought, an eremecausis of the sum-total of perceptions and representations that occupy our minds. It is an unusual analysis and synthesis of the elements which form the compound body—conscious experience—without any consciousness of the process itself; just as in the decay of organic matter there is a separation of component elements, and a recombination of these under new forms. He who first conceived a mermaid or a centaur, analysed and synthetically combined his ideas: but he did so by a

[1] The substance of two Lectures, introductory to a course of Medical Psychology delivered in the Newcastle College of Medicine, Summer Session, 1866.

voluntary effort, and was aware that the product of his thinking was no reality; whereas the lunatic who effects similar results does so involuntarily, and believes in the truthfulness of his new ideas. Bearing in mind these statements as to the nature of insanity, recollecting that there is no structural degeneration upon which it especially depends, and that there is no pathognomonic symptom which can be taken as a test of its existence, we turn next to its etiology.

The importance of rightly investigating the causes of any disease can scarcely be exaggerated, and the imperative obligation of such an inquiry where mental affections are concerned will be abundantly apparent before we have proceeded far. A sailor might as safely voyage along an unknown coast without a chart, as a physician engage in practice without a knowledge of the etiology of disease. It would be impossible for the one to avoid the rocks and sandbanks that beset his way, or to avail himself of favouring currents; and it would be impossible for the other to guide those who may seek his advice through the "thick-coming" circumstances of life that are charged with health- or death-giving power, or to seize those golden opportunities when a link may be broken in the chain of disease. A knowledge of etiology is essential to prophylaxis and a system of hygiene, and it is equally essential to therapeutics; for we shall strive in vain to cure many diseases unless we can detect and remove the cause which is operative in their production. It is also of high importance in diagnosis and prognosis, and in helping us towards a rational classification.

In our study of the etiology of disease generally, we are taught to divide causes into two great classes—the predisposing and the exciting: firstly, those which tend to induce such a state of the system or of a part as shall dispose it to certain kinds of maladies, and weaken its power of resisting morbific agencies; and, secondly, those which awaken latent tendencies to diseased action, or immediately produce that change of structure or of function in which disease essentially consists. With certain diseases this is a useful and excellent arrangement, and helps to give a clear view of the whole history of the disorder. You first observe the symptoms; you then, by a process of reasoning, refer these to the only alteration in the structure or function of the organism upon which they can depend. You proceed back from this to the circumstance or event or condition which is directly responsible for this alteration in structure or function. You proceed still further back to circumstances or events or conditions which created that state of system in which the special circumstance, event, or condition could become operative; and, after this, upon the whole and enlarged survey of the case you make up your mind as to its progress and issue, and as to that course of treatment which will be most conducive to recovery. To take an example: a man with a ruddy complexion, sandy hair, a broad chest,

and an open and ingenuous manner, but with a feeble raucous voice, comes to you complaining of paroxysms of breathlessness, of pain in the left shoulder, of loss of appetite, and general impairment of health. By further inquiry and explanation, you discover that there is a recent permanent contraction of the left pupil; that the voice has been lately changed in character, although there is no laryngeal disease; that there is oppression at the præcordial region, difficulty in swallowing, and a bruit to the left of the sternum. You reflect that the only condition that can satisfactorily account for all these symptoms is dilatation of the aorta, and you accordingly diagnose thoracic aneurism. Pursuing your inquiries further, you find that the man is a labourer in a shipyard, and has constantly to lift heavy weights; that three months before, after a severe strain, he felt faint and sick, and spat a little blood, and that he was formerly of intemperate habits. You then conclude that he was predisposed to aneurism by his diathesis, that this predisposition was heightened by his intemperate habits, and that the walls of the aorta gave way under the exciting cause of the over-exertion to which we have referred. In your recommendations as to treatment, in addition to the special procedure which you may think proper to adopt to promote coagulation of the contents of the sac, you counsel him to observe the strictest temperance in diet, regimen and life, and to quit his present occupation, and to seek some other where powerful and sudden muscular exertions will not be required of him; thus taking into account in your suggestions both the predisposing and exciting cause. Now, in some instances of mental disease you will find a predisposing and an exciting cause as distinctly indicated as in the case just sketched out, and you will be as readily able to make them available as guides to treatment. But in other cases, again, you will find that these causes have no natural separation from each other, that they merge into one another, and sometimes change places. What was the predisposing cause in one case has become the exciting cause in another, and *vice versa*. One patient comes to you who has had several attacks of delirium tremens, who has weakened his brain by long-continued intoxication, and who on receiving a blow on the head has become insane. He is followed by another who received a cranial injury many years ago, who has had occasional attacks of giddiness since, but whose mind has remained sound until he unfortunately gave way to drinking, when he broke down, and became insane. The intemperance was a predisposing cause in the one case, and an exciting cause in the other; and so was the cranial injury. It is the same with numerous other causes. Anxiety, religion, education, and anæmia sometimes predispose to mental derangements, and sometimes excite them. They may alternate or combine their morbific influences. There is, in fact, no fixed line of demarcation in nature separating the antecedents of

insanity into two generations. By imperceptible transitions we pass from one to another till we reach the disease itself, the complement of all of them, the growth of a whole series of events. So gradual is the process of its elaboration, that it is often impossible to distinguish the malady itself from its exciting causes—to say where causes end and symptoms begin. Under these circumstances, I think it impossible to study the causes of insanity with profit, without confusion and ceaseless repetitions, by dividing them into a predisposing and an exciting class. I prefer to regard the whole subject of etiology in another light. Insanity is preceded by a vast number of determinate phenomena, which are not always manifested in the same succession or order of development, but which are either physical or moral in their operation, which either act through the body upon the mind, or, popularly speaking, through the mind upon the mind itself. Those of the first class disorder the mind, secondarily, through a primary disorder of the organism; those of the second disorder the mind directly, and consist, in fact, in the abusive action of some of its powers or energies. I shall therefore divide the causes of insanity into a physical and a moral class, and shall arrange them under these two heads, according to their mutual alliances. This system will not prevent us from noticing their consecution in series, but will allow of their more easy and methodical remembrance and just appreciation. It has the advantage of being founded upon an intelligible principle and broad natural distinction. For, in spite of vague transcendentalisms, we must still maintain that there is such a thing as a world of mind, and that there is such a thing as a world of matter, and that the correlations of these have two remote starting-points. Life and consciousness are intimately bound up, and act harmoniously together; but they have, nevertheless, specific spheres. Their mutual and reciprocal influences are so complex and convoluted, that it is often difficult to explain or unfold them. They are so blended, that it is almost impossible to determine their source. But they exist distinctively; and we may, indeed, as safely predicate a mental and a physical element of insanity, as we may carbon and oxygen of carbonic acid. It is thus natural and appropriate that we should consider the causes of insanity in accordance with their affinities with one or other of its elements. To carry out this method fully and scientifically, it would be necessary to classify those causes entirely anew, as many of the groups into which they have been hitherto arranged contain members of both great classes. But several of these groups, such as age and social condition, are highly convenient, and are therefore here retained, with the understanding that they are really of a compound character, and imply various moral considerations. I shall enumerate the main divisions of causes which I propose to adopt, and shall then proceed to a more detailed examination of them.

Causes of Insanity.

(A.) Physical causes of insanity.
- I. Heredity.
- II. Age.
- III. Sex.
- IV. Social condition.
- V. Seasons, weather, temperature, and electrical conditions.
- VI. Temperament, diathesis, and cachexia.
- VII. Mechanical causes.
- VIII. Peripheral irritation.
- IX. Previous diseases.
- X. Dietetic causes.

(B.) Moral causes of insanity.
- I. Exhausting intellectual exertion.
- II. Sudden exhausting or exciting emotion.
- III. Excessive indulgence of the propensities.

I. Heredity.—Of all classes of disease, insanity has been pronounced the most hereditary; and as this conclusion is based on a wide investigation into the question, we may accept it as tolerably correct. We cannot, of course, enter upon the whole subject of hereditary influence, nor can we even exhaust that department of the subject which refers to mental diseases. We can only possess ourselves of a few of the principal topics which it embraces, and the laws (so far as these have been ascertained) which regulate its deportment.—First, then, as to its comparative frequency in cases of insanity. You will find that widely different estimates have been advanced, from that of MM. Aubanel and Thore, which makes it about 4½ per cent., to that of Dr. Burrows, which makes it 85 per cent. Statistical tables may be found justifying every intermediate percentage; and one courageous thinker has even denied hereditary transmission altogether. You will notice, however, that there is appended to almost every estimate a statement that the information upon which it has been formed is known to be imperfect, and that the calculation therefore probably falls short of the mark. The difficulty of obtaining reliable data regarding hereditary tendency to insanity is so very great, in consequence of the limited acquaintance with family history which some persons possess, the disinclination to disclose what is regarded as a disgrace or a blemish which others manifest, and the stupidity, ignorance, and forgetfulness which are all prevailing, that our knowledge of the extent and boundaries of the bane is necessarily very incomplete. When a patient labouring under insanity comes under your care, you must not rest satisfied

with any general assurance by the relatives that there has been nothing of the kind in the family; you must dexterously cross-examine them, you must win them into a confidential mood, you must get them to institute inquiries and report to you again; and then you will often discover, if your experience is at all like mine, that there is a terrible taint where there was declared to be none. You must not merely ask about insanity, but about madness, delirium, melancholy, fatuity, and every term which you can think of signifying mental aberration; for singular and inconsistent distinctions often exist in the public mind as to the use of the word insanity, which is not seldom restricted to downright raving mania. I had a case brought to me some time ago, in which I felt pretty certain there was hereditary taint, but in which I failed to make out anything in a long conversation, until accidentally it came out that the boy had had two uncles idiots. On mildly reproaching the father for evasion of this very palpable fact, I was indignantly informed that "silliness" and "sanity" were very different things, and that if I had wanted to know about the former I might have said so at first. I would therefore recommend you always to suggest idiocy, silliness, weakness, and also fits or the falling sickness; for it is but rarely that the dementia of epilepsy, no matter how profound it may be, is recognised as madness by the lower orders. Apart from its dementia, too, your inquiries should include epilepsy disorder, as also paralysis, apoplexy, chorea, suicide, &c.; for it is but a narrow view which fails to comprehend the whole nervous system in questions of this kind. I apprehend that your investigations, whether you collate statistics yourselves or contrast those framed by others, will conduct you to the conclusion that insanity is hereditary in at least 55 per cent. of all the cases occurring in this country. Dr. Thurnam, one of the most able and learned of statisticians, traced it in 47 per cent. of the cases under care at the Retreat; and the information upon which he limited it to this figure was confessedly incomplete. We must therefore make an allowance for those cases in which the heredity existed but was not discovered; while we must stop short of the highest estimates, such as that of Holst (69 per cent.), or that of Burrows (85 per cent.), which are somewhat exceptional and fallacious. Dr. Burrows' computation was formed from his experience in private practice, probably amongst the upper ranks, in which heredity undoubtedly attains to a far wider influence than amongst the humbler order, both because of the keener susceptibilities which luxurious habits foster, and the better preservation of weakly lives which wealth ensures. The highest estimates, too, generally include insanity in collateral branches, which is not, of course, necessarily hereditary. The most recent writer upon hereditary insanity, Dr. H. Grainger Stewart, who has gone into the subject with great care

and minuteness, but who does not seem to be aware that many of his conclusions were forestalled thirty years ago by the very gentleman of whose statistics in the Crichton Institution case-book he has so largely availed himself, finds that in 49·61 per cent. of the cases which he has examined there was hereditary insanity; that in 5·43 per cent. there was hereditary disposition to other diseases, the nature of which is not specified; that in 27·19 per cent. there was no hereditary disease; and that in 17·75 per cent. there were no means of settling the question. Now, supposing, as we are justified in doing, that heredity existed in one half the number of cases about which no information was obtained, Dr. Stewart's computation is brought into very close accord with that which I have offered. It is clear that insanity primarily originates in a large number of cases in which there is no connate predisposition to it, that causes are perpetually operative which create it where no latent tendency towards it previously existed, and that a limit is placed by nature to the indefinite extension of heredity. The diminished vitality and viability which are associated with almost all hereditary taints give a check to their propagation. They either intensify into extinction in death, or are gradually diluted till they become innocuous; so that I do not see anything reasonable in those computations which allege heredity of about nine tenths of all cases of insanity.

Reverting to Dr. Stewart's labours, I would draw your attention to a table which he has compiled, showing the various relationships in which the heredity was traced. This reveals, as we should have expected, that it is most frequently recorded in the direct line of parents and grandparents; next, in the collateral direction of brothers and sisters; and, thirdly, in the diagonal of uncles and aunts. Without multiplying figures, I may enumerate Dr. Stewart's subsequent conclusions. These are—that paternal is stronger than maternal influence in transmitting insanity; that this is propagated most decidedly in the order of the sexes—that is to say, that the male parent most frequently passes it on to the sons, and the female to the daughters; and that the female is, upon the whole, more liable than the male sex to hereditary insanity. Of the last two propositions, ample confirmatory evidence can be adduced; but in the first Dr. Stewart stands almost alone. Psychological authorities and popular belief are opposed to him. These agree in ascribing a stronger influence to the mother, both in the determination of capacity and the transmission of mental diseases and defects; and I cannot help thinking, therefore, that Dr. Stewart's figures are misleading, either because they deal with numbers too small to afford a fair induction, or because they are vitiated by some peculiar circumstance in the position of the establishment from the records of which they are drawn.

Some forms of insanity have a stronger tendency than others to

reappear in offspring; and in this respect mental defects come far before mental aberrations. I do not believe it possible for an idiotic mother to give birth to children of average intelligence; nor do I believe that imbecility in a parent can ever give place to normal mental development in his or her children. On the other hand, we know that a father or a mother may suffer from mania, and that their progeny may be altogether exempt from any such affliction. Proceeding beyond this general distinction, however, we have no reliable information. It has been held that melancholia is the most frequently transmitted of the various forms of insanity proper, but I do not think that we are as yet entitled to dogmatise upon this matter. Of course, in heredity, as elsewhere, like produces like; and every form of madness has a special tendency to perpetuate itself from one generation to another. Not only are forms of insanity thus transmitted, but specific delusions have been reproduced. Oxford, who attempted to shoot Her Majesty, believed that he was St. Paul; and so did his grandfather. As a general rule, however, it is only a comprehensive tendency to some kind of morbid action which is transmitted. It is the type of the insanity which is fixed by innate constitution. The specific form is derived from individual circumstance. There is often this hereditary predisposition of which we are speaking when there has been no declared insanity in the ancestors. The wild and undisciplined passions of the father may crop out as mania in the son; the secret sorrow of the mother as melancholia in the daughter; the ambition of the grandfather, as general paralysis in the grandchild. The latent forms of insanity, too, that sometimes distress and agitate the mind that to the world seems all serenity, are surely thus transmitted, and attain in their new soil dimensions that permit no longer of concealment. We know that the attitude of the parental mind is stamped upon the offspring. We can have no hesitation in holding that it forms an important element in the constitution of the new mind, which can be but a reproduction of the qualities of its progenitors, in new combinations modified by external circumstances. We shall thus have a certain degree of hereditary influence drawn from the habits of thought and of life of the parents about the period of conception and utero-gestation, just as we have a like influence deduced from their vital activity. The bodily feebleness of the progeny of the very young and very aged is well known. I believe that we have not only a corresponding mental weakness, but that we have also a sort of natural law of primogeniture, by which the eldest child inherits the highest tendency to mental diseases. To state this more particularly, I have remarked that children born in the early life of their parents are distinguished by a predominance of the passions and animal impulses, those produced in the prime of life by superiority of the intellectual faculties, and those brought forth towards the

close of the reproductive period by a fuller development of the affections and sentiments. We cannot enter here upon questions of transcendental refinement, as to whether the mental development and health of offspring is modified by the degree of affection or aversion for each other subsisting in the minds of the parents at the time of coition—by the erotic transport of a love-match, or the cool indifference of a marriage of convenience; but we can lay hold of this fact, well established and of momentous importance, that the temporary madness of intoxication may entail a permanent form of lunacy upon the being procreated during its continuance. A single act of inebriety as well as habitual intemperance may stamp disease and degradation upon the child begotten under its influence. Several nervous diseases, too, which are not themselves necessarily characterised by mental symptoms, such as epilepsy, chorea, paralysis, advance a stage, and involve the mind in expressing themselves again in a second generation.

An important law of hereditary insanity, first formularised by Dr. Leubuscher, of Berlin, is that its outbreaks are especially connected with periods of development, such as puberty, childbirth, and the climacteric period. I have no doubt of the truth of this observation, that the latent tendency is particularly prone to rise into activity at periods of rapid evolution. A lady who had brothers and sisters insane, but who was always of sound mind herself, married a medical man of much vigour and intelligence, and had issue, of whom the following is a brief history:—

1. Female. Became insane at puberty, and burnt herself to death.
2. Male. Congenitally imbecile.
3. Male. Became insane at adult life, and hanged himself.
4. Female. Became melancholic at climacteric period.
5. Male. Had convulsions at first dentition, became weak-minded.

Another law of heredity, which it is requisite for you to know, is that the danger attending it is less when the procreator from whom it is inherited has not become insane until after the procreation. The amount of risk in such a case will depend upon the origin and nature of the parental madness. If this has been due to some efficient cause, the operation of which did not begin until after the procreation, there will be little or no risk; but if it was only the evolution of a slowly gravescent tendency, or the growth of a life-long predisposition, the danger will be as great as if the actual attack had preceded the procreation. In all questions of hereditary tendency, the circumstances of the parental lives and their relative constitutions must be taken into consideration; for a weak predisposition may be made overpoweringly strong by dissipation, injudicious habits or exhausting diseases, by an alliance with a kindred predisposition, or by deriving auxiliary support from some other

morbid tendency. The predisposition is least to be feared which belongs only to one side of the house, and which is counterbalanced by opposite vital affinities on the other side; and that is most to be feared which flows from both parents, or which, coming from one only, is aggravated by other morbid prochivities possessed by the other parent. It is in this intensification of predisposition attributable to the union of kindred constitutions, that the danger of consanguine marriages consists.

Dr. Arthur Mitchell has lately published the result of his most extensive and patient inquiries as to the effects of marriages of consanguinity in Scotland; and to those of you who are interested in the matter, I would recommend the perusal of his papers, as replete with suggestive facts and lucid thoughts. His conclusions, so far as they affect our subject, are, that consanguinity of parentage tends to injure the offspring; that this injury shows itself in errors of the nervous system—in epilepsy, chorea, paralysis, imbecility, idiocy, and moral and intellectual insanity—more frequently than in any other way; that idiocy and imbecility are oftener thus induced than the acquired forms of insanity; and that when the children seem to escape, the injury may appear in the grandchildren. This last conclusion verifies the existence of a law of interrupted or alternate propagation which seems to regulate the transmission of some hereditary qualities, and which, however unaccountable, is not more wonderful than the phenomena of parthenogenesis. Now, we have nothing to indicate that there is anything intrinsically injurious in consanguinity itself; on the contrary, the phenomena of the self-fertilization of plants and the close breeding of animals establish that it is under certain circumstances a natural and innocuous arrangement, and we have therefore to look to the concentration of morbid tendencies, for which it affords facilities, for an explanation of its evil consequences. Blood relations are much more likely to partake of the same diathesis, and the same proclivities to diseased action, than persons of wholly different stocks. It is the terrible intensification of these which takes place in the children of blood relations, which is sometimes the mean, but sometimes also the square or the cube of the combined tendencies, that has to bear the blame of the disastrous results of such unions, which are to be condemned upon ethical and legal as well as medical grounds, as you may see by referring to two learned papers in the 'Fortnightly Review' for November last.

Whether a mother can communicate to the offspring of a second marriage a hereditary taint which she was the channel of conveying from a first husband to his children, is a question as yet uninvestigated. The indubitable transmission of physical peculiarities in this way, however, lends colour to the idea that modes of mental action may be similarly perpetuated.

II. Age.—The various stages of development and decline, the physiological eras in the history of the organism, have, of course, important pathological relations, and are nowhere more distinctly marked than in the impression which they leave upon disorders of the nervous system. Insanity is, as you would suppose, essentially a disease of adult life. It occurs in by far the largest proportion of cases in those years of manhood and womanhood when the brain has reached its full figure—midway between the feebleness of infancy and the decrepitude of age. This is just what we would have expected. The order in which the faculties unfold during the progress from birth to maturity must have a parallel order of liability to mental diseases. The gradual elaboration of the mind, from its earliest and simplest conditions to its later and more complex developments, implies a corresponding increase in the range of its susceptibilities and the diffusion of its powers over tracts beset with morbid influences. The evolution of the faculties is for ever widening the territory that is open to invasion and scattering its defences; and we thus find that with progressive age there is a progressive liability to mental disease. We must not, however, imagine that there is any period of life wholly exempt from mental disorder. Even in the "inarticulate depths of infancy" there are certain unnatural modes of mental action, while throughout childhood there are far more occasions and cases of madness than is ordinarily supposed. It would be well if we would address ourselves more to the recognition of insanity under such circumstances, and would note such cases with precision when recognised. We might thus elucidate much that is now obscure in mental pathology. By watching the derangements of the nascent and rudimentary mind, we would assuredly enlarge our knowledge of its growth, and augment our powers of analysing the compound elements which go to make up its maturity, and the altered relations of those which constitute insanity. That idiocy and imbecility are present in infancy is, of course, unquestionable; for, being not merely limitations, but actual congenital perversions of development, these must exist from the very dawn of life, and cramp the first movements of awakening consciousness. That other forms of mental disease, however, may exist at the same period, is a proposition which has been often disputed. Burrows held, as a general maxim, that insanity could not exist before puberty; and other psychologists have joined him in this declaration. Statistics, however, prove the contrary; and in recent years we have had some valuable communications upon juvenile madness. Conolly treated the subject, in his own elegant way, in the pages of the 'Medical Times and Gazette;' and Dr. Berkan, of the Brunswick Asylum, has collected the particulars of many cases. I have myself gathered together the records of about fifty cases of insanity occurring before puberty, at every age from

the day of birth. Still, infantile insanity must be comparatively rare, when we consider that so few instances of it are reported, and that such a large proportion of beings are living at that period of life. This is so, firstly, because infancy is not exposed to many of the exciting causes which act on riper years; secondly, because fewer faculties being then developed, fewer can be assailed by disease; and thirdly, because the delicacy of the infant brain is such, that it is unable to undergo severe irritation without perilling life. Diseases of the encephalon in infancy and childhood are generally acute in their nature, rapid in their progress and fatal in their issue, and appear as hydrocephalus, eclampsia, or convulsions, rather than insanity. We thus find that 57 per cent. of all the fatal cases of disease of the nervous system occurring in England take place in the first five years of life. No doubt, a large proportion of these is symptomatised by disorder of the mind; but it is only when the course of the disease is more tardy, where death is avoided, and more permanent lesion of the mind is incurred, that we are entitled to ascribe insanity. We know the rapid growth which takes place in the brain in early childhood; we are familiar with the fact that it doubles its weight in the first two years of life, and that its circulation is subject to frequent and sudden fluctuations; and we can, therefore, understand why it is intensely susceptible and easily injured. We are aware, too, that the simpler and deeper emotions, such as anger, terror, tenderness, and wonder, are exhibited by the child at the breast, and that emotional precedes intellectual development; and we have, therefore, a key to the forms of insanity which chiefly prevail in early life. We can satisfy ourselves that, in all such cases, the feelings and emotions are chiefly involved, and that they are generally in a state of exaltation, and that the perceiving power is deranged along with them; that a little further on in childhood, the instincts are more apt to be perverted, producing morbid and degraded impulses and vicious conduct—that delusions are now present as well as hallucinations, and that these have reference principally to me and mine; and that at a stage still nearer puberty, melancholia, with depressing notions, begins to display itself. Tuke gives the proportion of cases of insanity occurring under ten years of age as ·96 per cent. This estimate is probably considerably too low. It is founded upon the admissions into an asylum; and it is of course obvious that insane children will be much oftener treated at home than adults.

Puberty and the years both before and after it are very prolific of madness. The high functional activity which is characteristic of the epoch sometimes oversteps the boundaries of health, and originates multiform and perplexing disorders. The bodily metamorphosis which then takes place has a correlative mental transformation, and the impulse which the mind then receives sometimes forces it into a

morbid condition. The vehement degree of innervation which exists, when heightened by the stimulation of sensual and passionate desires, or of peripheral irritation, is very apt to pass into hypochondriasis, hysteria, mania, or melancholia: and this is more likely to happen where predisposition exists, where there is the exhaustion of rapid growth, or where the vital manifestations are imperfectly accomplished. In females, when the catamenia are retained, suppressed, irregular, difficult, or excessive, along with an exquisitely nervous temperament, insanity is not unlikely to occur. In males, within a few years after puberty, melancholia often occurs as a result of masturbation or venereal excesses, and mania as a result of sexual impressions. M. Esquirol thinks that melancholia is characteristic of this period. In youth insanity pursues an acute course, and often terminates in a remarkable crisis. Tuke gives 12·77 as the percentage of cases occurring between the ages of 10 and 20.

It is in adult life, however, that by far the largest proportion of cases of insanity occur. It is at this period that all those moral causes which we are hereafter to examine exert their chief sway, and that many bodily diseases and external circumstances which hold an etiological relation to madness come into operation. "The statistics of insanity" show that of 100 persons attacked by madness, 32·53 per cent. will be so attacked between the ages of 20 and 30, and 20 per cent. between 30 and 40. Georget found that of 4409 admissions into asylums, 2522 took place between the ages named. Esquirol states that the maximum number of admissions of cases of insanity takes place from 30 to 35 years of age; that the admissions of males are more frequent from 25 to 30, those of females from 35 to 40.

At the climacteric period, occurring in females between 45 and 50 years of age, and in males somewhere between 50 and 60, a considerable number of cases of insanity occur which can be attributed to the grave constitutional changes then in progress. Perhaps the depressing emotions which may be supposed to afflict the attainment of an unfruitful autumn of life, do, in the female sex at least, prejudice mental health; but it is to bodily conditions, to irritation derived from the pelvic viscera, to altered constitution of the blood and consequent degeneration, or to incipient wasting of the brain, that we must assign the chief place in the causation of the symptoms. Dr. Francis Skae has ingeniously argued, that all cases of insanity occurring at this time and having this causation should be classified together in one order. I think we might as well, and with as much edification, arrange side by side the many and diverse bodily complaints of which the climacteric period may be a cause also, such as anæmia, apoplexy, phthisis, and Bright's disease, or the multitudinarious distempers that may accompany

teething. It may be allowed to Dr. Skae, however, that melancholia is the commonest type of lunacy at this epoch.

When life ripens to its fall, and the chilling influence of old age creeps over it, there is a high degree of liability to dementia. The failure of power which is natural to this period either takes place so prematurely, or comes on so suddenly, and proceeds to such an extent, as to amount to madness. Dr. Clendinning has shown, in his tables, the decrease which takes place in the weight of the brain in persons of advanced years; and it is, indeed, generally recognised that thickening of the arachnoid, shrivelling of the convolution, serous effusions, and general atrophy, are the characteristics of the brain in very old people. This wasting is, of course, incompatible with normal functions. When it takes place early, or proceeds far, it produces senile fatuity.

III. Sex.—Various opinions have been entertained at different times and by different writers as to the influence of sex in predisposing to insanity. Opinions diametrically opposite have been advanced as to the relative liability of the sexes; Esquirol, Haslam, Copland, and Browne, contending that women are more subject to insanity than men; Lælius, Aurelianus, Thurnam, Tuke, and Jarvis, advocating the contrary conclusion. It is true that the first-mentioned group of authors have founded their opinion upon imperfect data. They found the existing number of female lunatics greater than that of the males, and they did not make allowance in their calculations for the relatively higher number of females in the general population, and for the greater mortality of males in our lunatic asylums, in consequence of which an accumulation of females takes place there. Still, notwithstanding these sources of fallacy, I am inclined to adhere to their view of the question, in preference to that of the second group of authors with their neat numerical arguments. If we refer to the Report of the English Commissioners in Lunacy, published last year, we find that there were on the 1st of January, 1865, in English public and private asylums, and in workhouses, 36,692 lunatics in all, of whom 17,034 were of the male and 19,658 of the female sex. We have, therefore, an excess of the females above the males of 2624, or rather more than 15 per cent., to which the excess of females in the population generally does not amount, and for the surplus in which, after this comparison, the higher death-rate of male lunatics will not account. By reference to the last Report of the Scotch Commissioners similar results are arrived at. We there learn that there were in public and private asylums, poorhouses, and private dwellings, in Scotland, on 1st January, 1864, 6359 insane persons, of whom 2942 were males and 3417 females. We have here an excess of 475 females, or, above 16 per cent. Dr. Thurnam, however, insists that in

order to attain to anything like accuracy, we must compare the number of male and female admissions into our asylums, assuming that these fairly represent the cases of insanity occurring for the first time. Upon this point the English Commissioners report that there were 4419 male and 4542 female admissions, during the year 1864, into public institutions and licensed houses in England, leaving out of consideration the returns relating to naval, military, and state criminal asylums. We have, then, 123 more females than males admitted during the year, or an excess of nearly 3 per cent. But turning again to the Report of the Scotch Commissioners, we gather that the admissions of male patients into pauper and private establishments in Scotland, for seven consecutive years ending with 1864, have averaged 663, while those of females have averaged 764; that is to say, 101 over, or 15 per cent. in excess. It is necessary to add that if the deaths are more numerous amongst male lunatics, the recoveries decidedly preponderate amongst the females; so that the accumulation of the latter in our asylums cannot be so great as has been supposed. Dr. Thurnam's method of estimating the relative liability of the sexes is, however, itself faulty. It does not indicate the number of cases of insanity occurring, but the number which have reached such a stage that removal to an asylum has become requisite. Now, for reasons which I shall immediately point out, it is evident that the number of women affected with mental disease, falling short of that intensity where seclusion must be had recourse to, is much larger than that of men. Women suffer most from slight, transient, functional disorders of the brain; men, from serious organic degenerations. Then women, when insane, are, by virtue of their feebler organisation, more manageable and amenable to home treatment than men with their more powerful muscular development; while they are also less disabled by their calamity for useful and remunerative occupations in those quiet, sedentary, constructive, or household employments, for which they are naturally best adapted. Thus it comes about that women attacked with insanity often recover at home, or are permanently detained there, and never, in England at least, come under official cognisance; while men similarly affected are removed to asylums, and swell the admission columns in our Blue Books. We have no means of approximating to the number of such cases in England; but in Scotland we have a return made by the Commissioners as to the number of insane persons resident in private dwellings, which, though probably far from exhaustive, is yet valuable information upon the subject. This, then, shows that in Scotland, in 1864, there were 720 male lunatics resident in private dwellings, and 938 female lunatics; thus proving an excess of females amounting to 218, or over 30 per cent.

Drs. Tuke and Jarvis, who believe in the higher proclivity of males to madness, have dwelt upon their greater exposure to all the

most prevailing causes of the malady, and have urged that to establish the greater intrinsic liability of females it must be shown that they are subjected to equally trying circumstances as regards the production of madness. Now that the fulfilment of this condition is by no means difficult, I shall endeavour to show you, while briefly summarising, what it is desirable for you to know as to the differences in nutrition, development, and psychical tendencies, which distinguish the sexes.

The most casual observation demonstrates that those hidden primordial laws which in the first instance determine the sex, continue to regulate after-existence, and create a divergence which is, perhaps, even slowly widening from the cradle to the grave. In man, we find that greater strength and firmness of conformation and durability of texture which fit him for the active part which he is to play in the world, as the breadwinner, the protector, and the soldier of progress. His bones and muscles are larger and coarser, his brain is heavier, his nerves are everywhere stronger, his maturity is later, the vitality of his blood is more intense, his respiration is more vigorous, and his heart and arteries are more capacious and active. On the other hand, his reasoning powers are of a more elevated order, his imagination is more ascendant, his passions are more forcible and impulsive; while his emotions are less susceptible, are occupied more with abstract or ideal objects, and are more under the control of his far stronger will. In woman, again, we observe a greater softness of the tissues and movements, and delicacy in the organs and functions. Evolution is earlier; the nervous system is more excitable, its receptivity is greater, its reaction less. The absorbent system is more active, the circulatory more fluctuating, and the contents of the pelvis are especially important, from their volume and relations. The sensory, the involuntary, excito-motory functions, the perceptive faculties, the emotions and instinctive feelings, predominate in activity, and all these converge towards the one crown and centre of her life—sexual love. In consequence of the organisation of his frame and faculties, and the part assigned to him in the world, man is more exposed to that wear and tear of the nervous centres which is implied in an active pursuit of the business of life in these pushing and striving days. He is more exposed to dangerous accidents, he is more frequently addicted to drunkenness, and is more profoundly affected by sexual excesses. As we might expect from these circumstances, and also from the greater elaboration of his nervous system, we find him much oftener suffering from severe organic lesions of the brain, and having certain fatal degenerations of the nervous centres almost peculiarly his own. Upon the same principle, we find him more liable to degeneration of the blood-vessels, which is also a fertile source of mental disease. Woman is destined to live within a narrower sphere, exempt from anxious vicissitudes. She is quieter and

more equable in her habits; but the immunity which these conditions might confer is more than counterbalanced by her partial and injudicious education, by her warmer and keener sensibilities, by the social restraints placed upon her desires, and the disappointment, chagrin, and fretfulness which are thus engendered, by the want of healthy objects in life, from which she often suffers, by the unhealthy amusements which she accordingly adopts, and, above all, by the extent and delicacy of her sexual relations. The singular sympathy which in her subsists between the generative organs and the state of the encephalic circulation disposes her particularly to dynamical mental derangements, and makes her liable, in fact, to a whole class of disorders of the mind peculiar to her kind. Her nervous sensibility, too, combined with the high importance of the critical epochs of her life, exposes her to another class of similar ailments. She has further allotted to her a special liability to insanity in connection with the puerperal state. Every asylum contains many cases in which the influence of sex and the sympathy of the brain with the sexual functions are clearly marked. I will only now repeat that women seem, upon the whole, to be more liable to insanity than males, but that the mental disorders from which they suffer are more curable than those of the opposite sex.

IV. Social Condition.—As to the influence of celibacy and marriage respectively in predisposing to insanity, there is a wonderful unanimity of opinion. The statistics hitherto collected by many investigators force upon us a conclusion highly unfavorable to the former state, showing us at least that a much smaller proportion of married persons is admitted into our lunatic asylums. Parchappe, who while inquiring into this subject analysed 17,932 cases, has calculated that of the patients in various asylums embraced by his statistics, 49 per cent. were single, 40 per cent. married, and 11 per cent. widowed. Esquirol states that the insane married are to the insane single as 1 to 2, and that there is very little difference between the married men and women. Dr. Tuke intimates that of 1426 patients admitted into Colney Hatch Asylum in four years, the distribution as to marriage was as follows:

Unmarried.	Married.	Widowed.
645, or 45·23 per cent.	643, or 45·09 per cent.	138, or 9·68 per cent.

Now, the census returns show that there are nearly twice as many married as unmarried people in Great Britain above the age of twenty; so that we may infer that the predisposition to insanity in celibacy is nearly twice as strong as in the married state. The researches of Desportes, Jacobi, Thurnam, and Grainger Stewart confirm this conclusion. The last-named gentleman has shown that of 893 patients admitted into the Crichton Institution whose con-

dition as to marriage was ascertained, 268 were married, 578 were single, and 47 were widowed. He has taken the trouble to calculate the proportions of the married and single amongst those patients who recovered and died, and concludes that the married have the best "*chance*" of recovery, and the widowed the worst; that the mortality is greater amongst the latter than the former, and that the single in both cases hold an intermediate position. Dr. Stewart makes out that out of above 900 cases, 141 were married with offspring, and 14 were married without offspring; and, tracing the progress of these cases, he arrives at the astonishing conclusion, which may well make the sterile tremble, that the married with offspring have a much greater chance of recovery and a much less chance of death than the married without offspring. We can clearly understand the general influence which married or single life may have in predisposing to insanity, or in affording protection against it; but we fail to apprehend what possible connection there can be between the issue of insanity once induced, and the fact whether its victim is childless or the father of a family. Such causes are so very remote, that it must take a kind of intellectual presbyopia to discover them. Of the making of statistics there is no end! Let us have them, however, with some practical bearing, and resting upon something better than the attenuated base of a miserable fraction of cases.

It is interesting to notice, in relation to this subject, the statistics of self-destruction. In the 4595 instances of suicide investigated by Dr. Brierre de Boismont, there were 1501 single men and 579 single women, 1129 married men and 515 married women, 250 widowers and 211 widows, 311 remaining undetermined. It thus appears that in single life the cases of women are to those of men as 1 to 2·76; in marriage, as 1 to 2·49; in widowhood, as 1 to 1·32; while the general comparison of the married and single is very much to the disadvantage of the latter, and closely agrees with Parchappe's figures referring to insanity. This concurrence is just what we should have anticipated when we remember that suicide is in a large number of cases a symptom of declared insanity, and in a still larger number the final and crowning symptom of a masked form of mental disturbance, which we might call mental dyspepsia, which in its minor degrees is of the commonest occurrence, and the extent of which, with all the heart-burnings and the disasters which it entails, is but poorly estimated by reckoning up the number of its "rashly importunate" victims.

If we inquire why celibacy conduces to madness, we are answered by Pritchard that it is because of the vices to which single people are so often abandoned. This may partly account for the fact, but it cannot be the whole explanation; for married people are not exempt from the same vices, and have sources of exhaustion peculiarly their

own." Dr. Skae has, indeed, described a form of insanity as "post-connubial," in this perhaps taking a hint from the Jews, who did not hold a man responsible for twelve months after his marriage. A plea of "recently married" would scarcely be admitted in bar of judgment in our own courts of law. We are not all delirious during the honeymoon; but still there can be no doubt that cases of insanity due to excessive sexual indulgence under such circumstances, and even when the charm of novelty can no longer be urged as an excuse, do occasionally present themselves. The real explanation of the comparative immunity of the married from madness implies both physical and moral considerations.

1st. Those who are known to be disposed to insanity by hereditary taint or previous attacks sometimes on principle abstain from marriage, or are unable to contract suitable alliances.

2nd. Those who are not known to be thus disposed, but whose minds are morbidly constituted and assume those habits of action which lead up to madness, are naturally adapted for celibacy, and do not experience the urgency of those feelings which prompt others to matrimony.

3rd. Those who are liable to insanity are in considerable proportion attacked by it before that age at which marriage is practicable.

4th. Those who from social restrictions or choice remain single are often addicted to solitary vice or to dissipation, and lack that sympathy which is a stay in adversity, and those outlets for the otherwise pent-up affections which the domestic circle affords.

5th. Those who are married lead more natural and regular lives, are more fixed in their employments and pursuits, more cautious in their undertakings, while they are subjected to more salutary moral influences.

Reflecting that the conditions of married life have perhaps been undergoing a change—that our modern civilisation, our railways, our clubs, our innumerable amusements, our growing refinements, our more luxurious or expensive modes of living, our increased population with its greater difficulties of bread-winning, have perhaps deprived the connubial state of some of the influence which it once possessed, and have dissipated some of those feelings which were formerly concentred in it; and remembering the revelations of our police courts and the Divorce Court—the sorrows and anxieties of married life which there find lamentable expression,—I have felt curious to know whether the statistics of the present day agreed with those to which we have referred, and most of which deal with the experience of many years ago. I have accordingly brought together the statistics of seventeen asylums, the reports of which have recently come to hand, and I thus arrive at the following results:

	Male.	Female.	Both sexes.
Single . .	1055	825	1910
Married . .	715	763	1418
Widowed .	90	177	267

This is even more alarming to bachelors and spinsters than the warnings of Esquirol or Jacobi; but the aspect of the case is altered when we separate from 12 English asylums, 2 Scotch, 1 Irish, 1 Colonial, and the Central Criminal Asylum, which are included in the foregoing table, and in all of which the single are in extraordinary preponderance. The English asylums, taken by themselves, stand thus:

	Male.	Female.	Both sexes.
Single . .	399	407	806
Married . .	431	471	902
Widowed . .	68	96	164

This indicates a decided falling off in the proportionate exemption of married people from madness. It is singular to remark that the married people have the worst of it in the manufacturing group of counties, and the best of it in the agricultural.

V. Seasons, Weather, Temperature, and Electrical Conditions. — The seasons certainly exert some influence over insanity, but not that which is generally supposed. It is a popular fallacy that cases of insanity and suicide are more frequent in the gloomy months of the year, and thrive best amidst November fogs. Nothing can be further from the truth. Brierre de Boismont has shown by elaborate statistics that suicides are fewest in November and December, and most numerous in May, June, and July. Of the cases which he has collected and analysed, 298 and 276 occurred in the first-named months, against 483 and 437 in the last two respectively. M. Esquirol has given a table of the admissions into the Salpêtrière during each month for nine years, from which it appears that the lowest number was admitted in January, and that the admissions increased progressively from March till July, when they attained the maximum. The date of admission into an asylum does not of course correspond with the incursion of insanity, which has often existed for some time, and even for a period of years, before the extreme measure is had recourse to. By an estimate embracing above 3000 cases in all stages admitted into one asylum, I have found that the average duration previous to admission was 49 weeks. If this be so generally, the calculation is not materially deranged. MM. Aubanel and Thore have corroborated the conclusions of Esquirol, and have recorded the following observations:

June, July, August . . .	2911 cases.
March, April, May . . .	2584 „
September, October, November . .	2370 „
December, January, February . .	2230 „

They express their opinion that the month of June is most productive of insanity, that July and August stand next to it in this respect, and that January has least influence of all.

Dr. Webster states that the experience of Bethlehem Hospital is to the same effect; and M. Parchappe has published an interesting table which places the matter beyond doubt so far as France is concerned. That the same obtains nearer home we may learn from the report of the Scotch Commissioners, which shows that, in the year 1864, there were admitted into public, private, and parochial asylums in Scotland 1985 lunatics, and that these were distributed through the seasons as follows:

January, February, March . . .	382
April, May, June	637
July, August, September	509
October, November, December . . .	457

The explanation of these figures will probably be found in the higher functional activity of all animal organisms in the spring and summer months. Tennyson tells us that—

> "In the spring a livelier iris changes on the burnished dove,
> In the spring a young man's fancy lightly turns to thoughts of love."

And the direction of the fancy here mentioned is only a sign of the increased vivacity of physiological processes within. The growth of the stag's horns is in itself a sufficient proof of the immense power either of seasonal changes or of laws regulating development which keep time with these, in inducing a vast augmentation of vascular activity, while the almost tetanic state of the frog during the reproductive nisus of the spring exemplifies the hyperæsthesia of the nervous system which accompanies this. Jenner and Hunter have shown that in birds the ovaria and testes enlarge when the breeding season arrives, and waste again when it is over; and all of us know that it is during the enlargement of these glands that birds display their highest instinctive efforts. Then it is that they pair, build their nests, fight their battles, administer their justice, and rear their offspring. When the breeding season is past they sink down to a lower level of life. We have little difficulty in understanding why at periods of active development the organism is more prone to be impressed by external agencies, and to exhibit any innate tendency to divergence; and why the periodic activity of the generative instincts, and probably of the ovaria and testes, in the human subject should both physically and morally increase the liability to mental disease. I cannot assent to the common theory which ascribes the greater prevalence of insanity in summer to the heat of the weather; for I do not think that the height of the thermometer in this country need ever occasion us very serious apprehension. A little summer excitement from time to time visits our

asylum wards in very hot and sultry weather, but we never see anything which would lead us to account for an increased proportion of insanity at that season by attributing it to excessive heat. For my part, I would be more inclined to lay the responsibility upon our cold and prolonged winters, and chill, unequal springs, which are so much involved in the production of what Dr. T. K. Chambers calls the "degenerative diathesis," which may be supposed to have exercised their baneful influence to the full by the time summer sets in. It is chiefly with chronic diseases of the nervous system that we have to do in asylums in this country; and these, as Dr. Chambers has shown in his elegant and philosophical lectures on the climate of Italy, are certainly not favoured by what we consider summer weather. Of chronic affections of the nervous system, no deaths at all were recorded in Genoa in 1860; but epilepsy caused in London in one year 1 death in every 180, and in Genoa only 1 in 253; while at Milan the like diseases are not sufficiently numerous to constitute a separate class. It is acute affections of the nervous centres that prevail in Italy. Thus, we are told that encephalitis, meningitis, and spinitis caused in Genoa 1 death in 59, in London 1 in 119; apoplexy and cerebral congestion, in Genoa 1 death in 12, in London 1 in 40. The experience of every asylum physician will testify how rare is the latter class of diseases when compared with the former in this country. We have here, then, a high but even temperature; a clear atmosphere, with only a slight degree of hygrometric saturation; and a bright and intense sunlight, favouring an exactly opposite condition of the constitution from that under which the insane in this country generally labour, coexisting with those diseases to which active circulation, arterialized blood, and easily torn capillaries are most liable; we have a climate, in short, nearly corresponding with our best summer weather, affording no encouragement to the growth of those nervous diseases which terminate in insanity as it is encountered in this latitude. We are justified therefore, I think, in exonerating summer-heat from the charge laid against it, and in transferring the accusation to the saturated air, bleak winds, rapid and wide variations of temperature, general frigidity and leaden skies of winter and spring. Of all kinds of weather, a conjunction of a moist state of the air with severe cold seems to me to be the most deleterious to mental health. It has both a physical and a psychical effect. It prevents proper evaporation from the surface of the body and the air-passages, occasions internal changes by driving the blood from the surface, or contracting the cerebral capillaries by reflex action, and extinguishes that joyousness and gaiety which are often the reflection of sunshine. It has a most depressing effect upon persons whose nervous systems are already weakened or diseased, and hence we find that the largest number of deaths in lunatic asylums take place in the winter and spring months. The

difficulties of management are, I think, aggravated during winter and early spring from the same cause. The largest number of deaths from brain-disease in the world at large take place in April and May. Hippocrates speaks of the greater frequency of apoplexy in damp and rainy weather; and Sir Henry Holland says that he has noticed three periods since he began practice, during which there was "a more than wonted frequency of apoplexy and paralytic seizures," and that he has observed at these times "frequent and rapid changes in the barometer, often with great depression of its level." The same conditions, again, operating in deep valleys lying between precipitous mountains, appear to check and pervert development, and encourage idiocy and imbecility. It is to these frequent changes and fluctuation in the pressure of the atmosphere that disturbance of the bodily functions is chiefly due. A rarefied is better borne than a condensed atmosphere; although if the rarefication be considerable, a sense of great fatigue with derangement in the balance of the circulation and cerebral congestion follow.

Recurring to the effects of temperature, it may be remarked that it is stated that in Turkey and tropical countries insanity is of much rarer occurrence than in northern Europe. I do not know upon what ground this statement proceeds; but, admitting that it is correct, as is probably the case, the explanation of it is so mixed up with questions of race, religious belief, and habits of life, that but little is left for the influence of climate or temperature. The latter, however, when excessive in degree, may be itself conducive to mental disorders. It has a general debilitating effect, depriving the heart and arteries of their tone and power, altering the constitution of the blood, which in turn stimulates the liver by its deficiency of water and surplusage of hydro-carbon, producing nervous irritability and pervigilium, and ultimately depression and insanity. It may achieve its effects upon the nervous centres by the blood-changes, or through visceral sympathies; but it probably acts oftener through the instrumentality of the vaso-motor nerves. A series of exact observations on the effects of great heat, locally and generally applied, is a great desideratum in medical literature; it could be well supplied in this district, where so many workmen are constantly employed before furnaces in glass-works, &c. Let me direct attention to the case of F. S—. Three years ago he had syphilis, with secondary symptoms, convulsive twitchings of the muscles on the right side of the face, and partial loss of power in the left arm and leg, from which, under iodide of potassium, he completely recovered. He continued well until about seven months ago, when his work as a boiler-maker called upon him to labour all day, with his face, front, and head exposed to intense heat, while cold water trickled on his shoulders and back. He felt faint during the day, and at night was sick and vomited. The next day the left side was again paralysed,

and his intellect was somewhat impaired. He was dull and stupid and forgetful. He gradually recovered until four months ago, when he inadvisedly contracted marriage. Within a week of taking this step he was worse than he had ever been before, and he is now, as you may see, much demented, with a dilapidated memory, and with a very curtailed use of language, and also partially paralysed. Even a more striking example of the effects of heat is presented by R. W—, who, when engaged as a railway clerk, had to spend the whole day in a small room, where during the summer the direct rays of the sun fell through a window in the roof upon his back for a considerable period of each day, and where the heat from a fireplace expended itself upon the same part of his person during the winter months. After being subjected to this heat for some time, he began to experience attacks of vertigo, and to lose the clearness and consecution of his thoughts. A little further on his absence of mind became so great, that he wandered away from his home without purpose, and forgot his acquired knowledge. He is now an inmate of this asylum, and is in a state of acute dementia, varied by irregularly recurring attacks of cerebral hyperæmia, in which the head is intensely hot while the extremities are cold, and in which he manifests considerable mental perturbation. In the first case, that of F. S—, I presume that the varied temperatures experienced during the day exerted their pernicious influence at night when active exertion was over, when the vaso-motor centres, exhausted by the adverse appeals thus made to them, suffered partial paresis, failing to give forth those streams of nervous energy which maintain the small blood-vessels at their proper diameter, thus permitting the dilatation of these vessels in the nervous centres, which in turn probably terminated in the rupture of some of them at an already weakened part. In the second case, that of R. W—, it is probable that the long-continued application of heat to the back, operating upon the cervical and upper dorsal ganglia, caused contraction of the cerebral capillaries, diminished blood-supply, and consequent impairment of the nutrition of the vesicular neurine and deterioration of mental power; that this was succeeded by reaction, paresis of the vaso-motor centres, and capillary congestion, such as we see following the primary pallor produced by ether spray, and also by a disordered condition of the vaso-motor centres, so that they have continued to act irregularly since the removal of the first cause of their derangement. I need scarcely point out to you that this case is very confirmatory of Dr. Chapman's views, as is also the fact that there has been a deposit of tubercle in R. W—'s lungs.

But heat is influential in producing insanity in still another way, and that is through the medium of sunstroke. Whatever view we may adopt as to the pathology of insolation, we can have no doubt that its efficient cause is protracted exposure to a very high tempera-

ture, and that the action of this is facilitated by a dry state of the atmosphere, by physical prostration, by previous debility, by intemperate habits, and by a vitiated state of the air inhaled. During the dreadful march of the 43rd Regiment from Jubulpore to Calpee, in 1858, in which five officers and twenty-two men died of insolation, the heat was so great that the thermometer stood in the small tents at night at 127° Fahr., and in the large tents varied from 115° to 118°. Under such circumstances, it is not to be wondered at that the nervous system shows signs of profound disturbance. The vertigo, anorexia, nausea, lassitude, sleeplessness, sharpness of the pulse, and incontinence of urine which are premonitory of the attack, and the sudden insensibility, stertorous breathing, maniacal paroxysms and convulsions which characterise the seizure, all indicate a grave disorder of the encephalic centres. The brain may be poisoned by the heated and polluted blood, or it may be damaged by extreme congestion. Many of the symptoms point to oppression of the medulla oblongata as their source; and if this be so, we can understand how the congestion occurs, through the relations of this organ with the vaso-motor centres. Whatever the immediate condition may be in heat-apoplexy, it does not pass off without leaving grievous vestiges behind it. The brain is permanently weakened by insolation; paralysis, chorea, mania, melancholia, and dementia are among its sequelæ. Its immediate effects are enumerated by Sir Ranald Martin as ardent fever with acute delirium, remittent and intermittent fevers complicated with dysenteries, hepatic inflammations and congestions. The more remote effects include impaired vision, an impression that the sun is perpetually shining upon the body, deafness and tinnitus aurium, distressing formication and desquamation of the cuticle, difficult respiration, insomnia; emotional irritability, indicated by proneness to laughter and tears, agitation, alarm at sudden noises, hemiplegia, local palsies, and epileptiform attacks. I cannot adopt Dr. Handfield Jones's theory, that all these more abiding effects of insolation are referable to simple impairment of functional energy in the cerebro-spinal centres. I believe that they often signify a subacute form of meningitis, with changes in the nutrition of the brain-cells and fibrils, and the walls of the blood-vessels. Old soldiers who have served in the tropics, and suffered from sunstroke, are sometimes admitted into our asylums in this country. There is one such case in this institution. I allude to J. M—, who was with the 89th Regiment in Bombay, and was struck down by sunstroke, from which he recovered, but without regaining his senses, so that he was sent home as incapacitated for service. He labours under chronic mania; is for a time quiet and industrious, and is again idle, mischievous, incoherent, and actuated by the most eccentric beliefs. He is always at his worst during very hot, sunshiny weather. In two other cases of insanity caused

by insolation I have noticed the same unusual susceptibility to the influence of solar heat. Although it is chiefly from India that our stock of cases of insanity due to sunstroke is derived, we are not without instances of the operation of the same cause in our own climate. Dr. Handfield Jones gives two cases in his able work on 'Functional Diseases of the Nervous System;' and I remember having under my care, in the Warwick County Asylum, a man who had suffered sunstroke when working in the fields on one of the hottest days of the autumn of 1864. He and another man, who was working along with him, on returning home at night, were seized with giddiness, sickness, and stupor. My patient became insane—his companion died. The form of insanity in this case was hypochondriacal melancholia. The man thought that his body was in a state of putrescence, that his bowels were always acting involuntarily, that he had the foul disease, and had sold himself to the devil. When I first saw him, six weeks after the stroke, he had also hydropericardium.

One other action of high temperatures in conducing to insanity remains to be dealt with, and that is through the agency of burns and scalds, which, when severe and wide-spread, may be destructive of mental soundness. They impart a shock to the whole nervous system, which is sometimes fatal to its equilibrium, and affect the intercranial circulation through the spinal cord and vaso-motor nerves, probably by paralysing the latter, so that dilatation of the minute vessels, and perhaps interstitial changes in their coats, with extravasation of blood or effusion of serum, ensues. Dr. Brown-Séquard has explained that in cases of extensive burns a change is in all likelihood wrought in the blood-globules, owing to the high temperature to which so many of them are exposed, and that there is also a large diminution in the cutaneous excretions. His experiments, however, demonstrate that it is by reflex action through the spinal cord that burns produce their deadly effects upon the viscera. You have no doubt all seen the delirium of which they are productive soon after their occurrence. Their after-effects resemble those of insolation. I have never seen a case of insanity directly attributable to this cause; but I have traced in epileptics, who from accidents happening in their fits have been badly burned or scalded, an unusually rapid incursion of dementia. In one case, where the epilepsy had only created slight intellectual obscurity, a burn on the shoulders and neck was shortly followed by complete but unexpected fatuity. In order to produce an effect of this kind, the burn must, of course, be of large dimensions, as one of more limited character might even be beneficial to an epileptic as a counter-irritant. The sympathetic fever which accompanies a burn may suspend for a time epileptic fits, and light up the darkness of dementia, probably by the increased vigour of the circulation which then exists. Dr. Radcliffe has known four cases in which epileptic fits, which had

occurred daily before the incursion of sympathetic fever consequent upon an injury, and which recurred afterwards with equal frequency, were altogether abolished during the continuance of this condition.

It is highly probable, though it is as yet scarcely capable of proof, that there are certain cosmical influences which, through the nervous vitality, act on mental life, and occasionally contribute to disease. The electrical condition of the atmosphere which occurs in India, in which the hairs of a horse's tail repel each other, and those on the human head stand erect, has been noticed by Dr. Crawford to produce irritability of temper and severe headache, and to be frequently followed by attacks of apoplexy. Dr. Barclay has also noticed that cases of insolation occur in the greatest number immediately before a thunderstorm. Remembering that all the changes of condition in the component parts of the living body which are momentarily taking place, are accompanied by a disturbance of electrical equilibrium, and that free contact is constantly maintained with surrounding objects, we cannot fail to perceive that the human organism has important electrical relations. No man of delicate constitution can have failed to remark in his own person a peculiar depression of spirits and restless anxiety as the precursors of a thunderstorm; and I am confident that I have observed in asylum wards an exacerbation of melancholia and mania under similar circumstances. The effects of lightning upon the nervous system are either alterative poisonous or altogether destructive. They have been known to show themselves in the relief of paralysis and long-standing spasmodic affections, and in the cure of blindness and deafness, as well as in the creation of delirium, mania, tremors, convulsions, neuralgia, rigidity, and instantaneous death. Without that ecchymosis or scalding of the surface which is so common in persons who have been struck by lightning without laceration, fracture, or any discernible signs of external violence, we may have the most serious injuries inflicted upon the brain and spinal cord. We may have permanent paralysis, well-marked concussion, apoplexy or chorea, amaurosis or cophosis, daltonism or aphasia, unconsciousness travelling into dementia, or excitement mounting into mania. For the most part these affections are transitory, but sometimes they are long-continued and difficult of removal. The forms of madness which are most often thus induced are melancholia, or dementia, characterised by complete prostration of memory and vital power, and these are oftener met with in men than in women, because their greater bulk renders them more liable to be struck by lightning, to which their out-door occupations and wandering propensities more frequently expose them. A nonogenarian, who died recently, and furnished a paragraph to the newspapers, left behind him, as his best legacy to posterity and the secret of his own longevity, the discovery that it is

necessary for human beings to sleep due north and south, parallel to the great electric currents of the earth, if they would enjoy health and prolong their days. There may be more implied in this theory, after all, than the mere extravagance of a crack-brained philosopher. Besides, electrical influences, lunar attraction, the velocity and direction of winds and hurricanes, and geological formation, all, probably, exercise some control over the origin and progress of mental disease; but the nature and extent of that control is as yet in no degree made out: indeed, its existence would still by many be considered hypothetical. The complexity and subtlety of the subject renders it eminently difficult of investigation. To the comparatively new science of meteorology, however, which is now being pursued with such care and success, we may look for much light upon this very dark corner of etiology.

VI. Temperament, Diathesis, and Cachexia.—Our only means of arriving at a knowledge of constitutional qualities and modes of action, is by an observation of external signs. These have for practical purposes been formed into four groups, which are denominated temperaments, and which may be recognised by certain definite appearances, and correspond with certain definite functional tendencies, but which may be commingled with each other in very various degrees. Experience has shown that each of these temperaments has special proclivities towards disease, and that some of them incline more than others towards insanity. The nervous temperament, pure or combined with the sanguine, seems to incline most in this direction. The predominant activity of the brain, the vivacity of habit and the delicacy of health which characterise it, entail great susceptibility to all morbid influences from without or suggestions from within. The animation, irascibility, and volatility which invariably mark it, are apt to pass into the wild extravagances of mania or the hysterical alternations of emotional madness. Next to it the sanguine temperament is probably that which should come in order of importance as a cause of insanity; it also implies great cerebral activity and quickness, irritability of the nerves, fanciful constructions, and proneness to deviation. After it comes the slower and more sombre and durable fibrous temperament with its tendency to melancholia, and the sluggish lymphatic, finding the perfection of its natural lethargy in dementia. As we have no means of ascertaining the relative proportions of these temperaments and their combinations in the population at large, we cannot speak with precision as to their respective prevalence amongst the insane. Besides, a classification of temperaments is by no means simple in a country like this, where several races have been blending for centuries, so that their characteristics are now mixed up in infinite gradations.

In diatheses we have something more definite and better understood, because less comprehensive than temperaments, to consider, and something more nearly connected with the causation of insanity, because diathesis implies a special predisposition to certain kinds of diseased action, which may develop themselves at some stage of life, or imprint a character upon any ailment that is accidentally induced. In cachexia, again, we have simply a developed diathesis—a constitutionally diseased condition of the blood and tissues. A diathesis may be looked upon as a remote cause of insanity—a cachexia as a part of its constitution. Insanity is not rarely a group of symptoms expressive of that generally depraved systemic condition to which cachexia is the term applied. We shall not separate diathesis and cachexia in our examination of them as causes of mental disease, but shall shortly sketch their conjoint operations.

1. The nervous diathesis and cachexia which are characterised by spareness of body, fineness of hair, mobility of countenance, restlessness of the whole muscular system, animation and quickness of manner, and proneness to agitation upon the slightest occasion, depend upon high functional activity and disproportionate development of the nervous tissues. Those who manifest them are easily startled, are worried by trifles, are acutely sensitive, and where the cachexia is present are extravagantly so, probably displaying at the same time muscular tremor, hysterical excitement, or some other sign of nervous disorder. They contemplate life through an emotional atmosphere; they take unreal views of things, mix up their own vivid personality with every passing event, cultivate æsthetical tastes, and hurry with anxious impatience from project to project and from mood to mood. They are incapable of deliberation or quietude, are jerky and abrupt in their movements, and prostrate in their exhaustion. A constitution like this, whether pure or mixed with other morbid tendencies, such as the strumous or the atheromatous, is fruitful of mental infirmity. The eccentricity which is one of its most constant accompaniments verges upon madness, and often passes into it. The exalted sensibility and hasty activity which are its essentials break down breaches in the fortress, which afford ready ingress to marauders from without, and egress to the rabid inhabitants. They raise the mind to an emotional pitch that is morbid, or sink it into abject feebleness. Hysteria, catalepsy, neuralgia, paralysis, eclampsia, convulsions and epilepsy, drunkenness and debauchery, are some of the channels by which the nervous diathesis approaches lunacy, and the impulsive and emotional phases of insanity are those which it most frequently assumes. Women are oftener of the nervous diathesis and cachexia than men, probably by virtue of their sexual organization, and their more sedentary and enervating lives. It is from the upper ranks of society that the largest number of cases of insanity associated with a nervous cachexia are derived. The diathesis is fostered into the

cachexia by luxurious habits, and thrives so ill under hardship and poverty, that it rarely survives the period of infancy, and leaves few representatives in adult life. To an inordinate indulgence, through successive generations, in the luxuries and refinements to which this diathesis disposes, and upon which it feeds, we must attribute that type of mental defect which is seen in the spoon-bill idiots or luxury cretins, as Professor Laycock has designated them, of whom Lord Dundreary is a dramatised specimen. We have an illustration of the diathesis in L. B—, a Swiss governess, whose small and delicate features, fine and variegated hair, sharp and vivacious manners, exquisite sensibility, and muscular twitchings, afford clear evidence of her nervous excitability. We may fairly infer that she was thus predisposed to the insanity which has overtaken her, and which is displayed in passionate outbursts of mania when she is crossed or slighted.

2. The strumous diathesis and cachexia, characterised by a general deficiency of vital energy, imperfect nutrition and reproduction, which are, as you may suppose, amongst the most important causes of mental defects and perversions. They operate, indeed, in this way through so many tissues and organs, that it would take us too long to enter upon any detailed examination of their various methods of action. We can only indicate these very generally.

a. They may, by their presence in the parent, contribute to the idiocy of the child by preventing those nutritive and morphological changes which are necessary to the development of the embryo.

b. They may check mental evolution by the establishment of their persistent wasting action, and the extinction of that constitutional vigour which is required for healthy growth. This they are especially apt to do at periods of active development, as at dentition and puberty.

c. They may unduly hasten special development, or exhaust the powers of mind, and induce dementia by the premature exertion of them to which the irritability of a feeble nervous system sometimes leads.

d. They may deteriorate mental power by impoverishing the condition of the blood through imperfect assimilation, and thus furnishing inadequate and unsuitable materials for cerebral nutrition.

e. They may disorder the mind by lighting up slow chronic inflammation in the brain or its membranes, which, if it does not destroy life, abbreviates it, and leaves the mind a prey to fitful and diseased impulses, or a mere wreck of its natural dimensions.

f. They may irreparably damage the mind by the deposit of tubercular matter in the brain or its meninges.

g. They may derange the mind reflexly through the tubercular degeneration which they may induce in other viscera, such as the lungs, liver, spleen, pancreas, ovaries, or mesenteric glands.

The diathetic tendency to tubercular degenerations, whether congenital or acquired, may involve the organism generally, or may exhaust itself upon some organ or limited portion of tissue. Its elective affinities are for the mucous membranes and their glands in the first place, for the lymphatic glands and the blood-corpuscles in the second place, and consecutively for the other tissues. The defective supply of the pabulum needful for healthy nutrition and development which is due to the diathesis goes on to cachectic degenerations, as yellow tubercle, miliary or grey tubercle, albuminous tubercle, and amyloid or waxy tubercle. The frequency with which these transformations are found in the bodies of those dying insane suggests the existence of an intimate relation between the conditions which they betoken and that with which they are thus associated. We must conclude that the nerve centres are particularly apt to participate in that morbid operation in which the strumous cachexia consists, and that they have also a conjoint influence in promoting, accelerating, or modifying the degenerations which are typical of it, by their action upon the tissues themselves, and the motor power of the capillaries.

3. The arthritic diathesis and cachexia dispose to insanity principally through that deficient nutrition of the blood-vessels which is symptomatic of them, and which is so apt to be localised within the cranium, producing there obstruction to the circulation, irritation, and perhaps inflammation of the cortical substance. Atheromatous degeneration of the capillaries of the brain is supposed by some to be the essential pathological condition of general paralysis. Calcification of the intercranial vessels is often associated with senile dementia, and arterial dilatations sometimes become the excitants of melancholia, panphobia, and other reflex mental disorders. This may be the case in those who are subject to gout, which is indeed so often followed by mental derangements, that a German writer (Vering) has described a specific gouty fatuity or mania from the influence of the disease upon the brain, and a gouty melancholia and idiocy from its influence upon the ganglionic system. The predisposition to gout is mostly associated with great activity of the nervous centres. Sydenham observed that it kills "more wise men than fools;" Cullen, that it affects especially "men with large heads;" and Spencer Wells, that it attacks most often those members of both Houses of Parliament who are most distinguished by a glorious ancestry, and by successful personal exertions in the arts and sciences. The superior activity of the brain thus indicated is, of course, apt to deteriorate under certain morbific influences into the purposeless energy of insanity. During the acute paroxysm of the disease gouty persons are invariably irritable and unreasonable, or dejected and morose, but it is not under such circumstances that the baleful influence of the diathesis upon mental health is most

ostensibly displayed. It is in the interparoxysmal periods that we must look for those more persistent derangements which can only be fairly accounted insanity, and which are brought on in various ways.

a. The existence of this gouty diathesis or cachexia may lead to certain imperfections in the removal of the tissues subjected to wear and tear which may ultimately weaken mental power.

b. The presence of uric acid in the blood may poison or irritate the vesicular neurine of the brain, and thus create irregularities in its actions.

c. The disorders of other viscera (such as the kidney) which are so frequently produced by the efforts of the system to throw off the pernicious matter from the blood, or which are but masked and anomalous expressions of the gouty habit, may also become reflex causes of insanity, or indirectly tend to induce this condition by the general impairment of health, for which they are responsible.

d. The occurrence of chalky tubercular deposits within the cranium, in the fissure of Sylvius, or the choriod plexus may directly disturb the functions of the brain.

e. The occurrence of similar concretions around the joints or in the fibrous tissues may disturb the mind reflexly or by the suffering which they occasion.

4. The rheumatic diathesis also deserves a place amongst those which predispose to madness. It does so by the general debility which it occasions, but it has also a specific action. The materies morbi of rheumatism, though it attaches itself chiefly to the nervous system, has no necessary connection with mental derangement. We have, doubtless, in our hospital experience seen numerous cases of rheumatic fever in which the poison exerted no deleterious influence on the mental functions; and we have, perhaps, also seen cases where delirium complicated the course of the disorder. As the rheumatic poison is universally the same, while its effect in the production of delirium is only exceptional, it is clear that it is no necessary element in the result. The delirium never appears but where there has been previous exhaustion and depression of nervous power, together with a cognate tendency to mental disease. Under such circumstances, however, the poison becomes an irritant to the cerebral tissues, and inaugurates an æsthenic delirium which is in no degree allayed by the appropriate treatment of rheumatism, but must be combated by nervine sedatives. This sometimes passes into a more protracted type of insanity. In causing insanity, rheumatism acts in one of three ways:

a. It lights up inflammation of the intercranial membranes by a metastatic process, thereby entailing temporary or permanent mental aberration. This happens only in severe cases of acute rheumatism, and concomitant with it there is invariably the subsidence of the affection in some large joint.

b. It creates simple excitement of the cerebral tissue, a complication most frequent in subacute cases.

c. It occasions such a change in cerebral nutrition as contributes to hypochondriasis and melancholia.

I have just had under my care a worn-out street-fruitseller, who is crippled by rheumatism, who is also epileptic, and who during six months has had three sharp attacks of epileptic mania, each being preceded by an attack of rheumatic sclerotitis.

5. The syphilitic diathesis and cachexia, whether inherited or acquired, are powerful in conducing to mental disease. I shall again explain how they may do this, indirectly, through the fear which they create in the mind—the anxiety, apprehension, and anguish which they sometimes awaken, and which have been denominated syphilophobia. What we must consider now, however, is their physical action. The specific poison which a chancre introduces into the system is so subtle and penetrating in its effects, in whatever manner these are brought about, that it would be singular if the delicate nervous textures were exempt from participation in them. We find accordingly that both dynamical and organic diseases of the brain are traceable to syphilis, the inveterate venom of which permeates the whole system, and outlives years of forgetful imprisonment, to break forth into mischievous activity when the body is weakened or disabled by some other complaint. We may classify the methods of operation of this virus into five divisions.

a. It may excite certain mental diseases, such as recurrent mania or stupidity, by a simple functional derangement of the vesicular neurine. Such cases are always characterised by intense headaches, by temporary amaurosis or deafness, by neuralgic pains, or local or general convulsions. Like most syphilitic diseases, they have nightly exacerbations, and often exhibit a marked periodicity of recurrence. I have seen syphilitic depression of spirits, with utter hopelessness as to the future and disgust with the past, and accompanied by drawing of the face to one side, come on regularly at seven o'clock each evening, last for six hours, and then give way to natural gaiety of spirits and normality of countenance. These dynamical syphilitic disorders of mind may generally be differentially diagnosed from organic syphilitic affections by the intermissions and intervals of health and comfort which mark their course, and by their great amenability to treatment. A few doses of iodide of potassium have a most magical effect upon them.

b. It may conduct to insanity, through periostitis of the cranial membranes, which by its contiguity to the vesicular neurine sets up irritation there, or actual inflammation.

c. It may cause insanity by morbid products deposited in the brain, by syphiloma, which may exist either as a diffuse infiltra-

tion, or as a tumour of indefinite soft and homogeneous appearance—the gummy tumour, as it is sometimes called.

d. It may cause insanity less directly by similar deposits in remote organs, such as syphilitic infiltration of the lungs or liver.

e. It may conduce to insanity by that depraved state of health which always accompanies its action, which assumes the form of syphilitic chlorosis in women, and which interferes with cerebral nutrition.

6. The cancerous diathesis and cachexia are generally characterised by melancholic tendencies, and have some special relations to insanity. The mind, during the progress of the malignant disease, is "sicklied o'er with the pale cast" of despondency, just as the bodily visage is overspread by a peculiar sallowness. This sometimes deepens into delusion, as where schirrus of the duodenum is believed to be a serpent gnawing at the part, or where epithelioma is held to be a special providential chastening on account of some real or imagined transgression. Wide-spread functional derangement, constitutional irritation, and febrile action of the hectic type, are almost invariably associated with the pronounced cachexia, and sometimes involve the brain. Then this organ may be itself the seat of a malignant growth, which is oftenest primary in this locality, and this will infallibly produce certain mental derangements. Dementia and melancholia are occasionally thus caused. Cancerous growths in other viscera may also secondarily prejudice mental health.

7. The hæmorrhagic diathesis, in which the constitution of the blood is altered so that it is in appearance thin and ichorous, and deficient in fibrin, and in which there is defective contractility and tone of the walls of the capillaries and minute arteries, so that they are thin, feeble, unusually capacious, and prone to laceration, predisposes to spasmodic and neuralgic diseases in youth, and also to phthisis, and hypochondriasis in mature life, and to dementia at any time from hæmorrhage into or upon the cerebral substance.

8. The fibrinous diathesis and cachexia in which there is a tendency to certain degenerations of the blood and fibrous tissues, to exudations of unhealthy plasma, or to coagulations of fibrine within the blood, may be indirectly instrumental in disturbing mental equilibrium, by the thickening of the cardiac valves, pulmonary or hepatic cirrhosis, hypertrophies of the sub-mucous gastric tissue of the testicle or uterus to which they are known to conduce. When degenerations of this kind exist, the functions of the assimilating and depurating organs are but languidly performed, health is broken down, the waste of the brain is imperfectly repaired, so that its strength decays, while it is assailed in its weakness by morbid representations from the degenerated viscera. It is also itself liable to morbid fibroid developments. It suffers various

degrees of induration, up to the consistence and elasticity of fibro-cartilage, as the result of inflammatory exudation or simple hyperæmia. But it seems also to be liable to like induration, as the result of perverted nutrition, and independently of any inflammatory process. Without thickening or injection of any of the meninges, without adhesions or apoplectic foci, we may have partial or general hypertrophy, and induration of the cerebral substance, due to unusual development of its white connective tissue.

(*To be continued.*)

Art. II.

On Three Cases of Imperforate Hymen with Retention of the Menstrual Fluid. By H. M. Tuckwell, M.D., Physician to the Radcliffe Infirmary, Oxford; and late Radcliffe Travelling Fellow.

Case 1.—R. G—, æt. 16, was admitted into the Radcliffe Infirmary under my care on January 3rd, 1867. She states that she first began six months ago to suffer from pain in the lower part of the back, shooting round into the groins and down the legs. This pain has within the last few weeks become very severe, and is accompanied by a sensation of bearing down, especially distressing when she attempts to pass water or when the bowels act. She has likewise noticed of late a swelling in the lower part of the belly, which is also the seat of pain. She cannot say whether her sufferings have increased at any regular periods, but she is sure that she has never menstruated. Her water has for some time been scanty, and passed with difficulty. She is costive, and suffers much when the bowels act. Her general health has been throughout good. A healthy-looking, well-nourished girl, with well-developed mammæ. No unnatural sign or symptom, beyond constipation and difficult micturition, is observed until the abdomen is examined, when a tumour is there seen and felt, of about the size and shape of the uterus at the fifth month of pregnancy, occupying the hypogastric and lower umbilical region. This tumour feels elastic, and is painful when handled. Percussion over it yields a dull sound, the rest of the abdomen being everywhere resonant. On separating the labia, no vaginal opening can be seen. Above is the clitoris; below, the orifice of the urethra; and below this, a bright-red membrane with a granular surface, occluding what should be the vaginal orifice, and

slightly bulged forward as if from pressure behind. On passing the finger into the rectum, a passage can only be made by keeping the finger close along the posterior wall of the bowel, for the calibre of the latter is reduced by more than one half. No trace of uterus can be felt; but a firm though fluctuating tumour pushes the anterior wall of the bowel into contact with the posterior, and seems to occupy very nearly all the pelvic cavity. By pressing on this tumour with the finger of one hand, and on the occluding membrane of the vagina with that of the other, fluctuation is communicated clearly from one finger to the other. A catheter being passed without difficulty, about two pints of urine are drawn off. The abdominal tumour above mentioned has now in part disappeared; but there still remains in the hypogastric region a deep-seated tumour, hard and resisting to the touch, which may well be compared to the uterus in the third month of pregnancy.

Ordered—Ol. Ricini ʒss, statim.

January 4th.—Chloroform being administered, a free incision into the occluding membrane is made by Mr. Briscoe, and is followed by a gush of very thick, black, treacle-like fluid, about a pint in quantity. The finger passed into the vagina is lost in a great cavity without walls or boundaries. The divided membrane is quite thin, and evidently no more than an imperforate hymen.

Ordered to bed, and to be kept perfectly quiet in the horizontal posture.

5th.—She is going on well, without a bad symptom. A large quantity of the same black fluid, about as much as escaped at the operation, has oozed out of the opening.

7th.—She is progressing favorably. The bowels have acted, and urine has been passed without pain. The discharge continues, but is much less in quantity.

8th.—She was seized suddenly yesterday, between three and four p.m., with a severe pain in the lower part of the abdomen, starting from the left iliac region, and thence radiating over the whole abdomen. The pain is now severe, and the abdomen everywhere tender on pressure, especially in the hypogastric and iliac regions. There is no vomiting. Pulse 128. Tongue coated. Skin 100·2° Fahr. The discharge has quite stopped.

Ordered—Pulv. Opii gr. ss, statim, et repet. quartâ quâq. horâ. Turpentine stupes to be applied to the abdomen.

9th.—The pain is relieved. The abdominal tenderness is now limited to the hypogastric and left iliac region. The abdomen is everywhere resonant on percussion, but no meteorism is present. Pulse 124. Tongue still coated. Skin 99·4.

13th.—She has been improving daily. Last night a little thick and fetid pus escaped from the opening in the vagina. The wound

in the vagina is now found to be in great part closed; two small openings, about as large as a pin's head, remaining at the lower part of the cicatrix, through which openings a little pus still oozes.

14th.—Chloroform being administered, a director is thrust through one of the above small openings, and the membrane is slit up and then across in a crucial form. About one ounce of the same fetid pus escapes. On passing in the finger, the vagina is felt of its normal size, its upper end completely filled by the large gaping os uteri, whose lips are thin, and beyond which no trace of cervix can be felt. The end of a full-sized elastic male catheter, wrapped round with lint and oiled, is next introduced into the wound and fixed there by tapes.

From this time forth she recovered without a bad symptom. It was found necessary to introduce twice a sponge-tent to dilate the contracting orifice. At the end of two more weeks menstruation came on, and, with the exception of a slight recurrence of pain in the lower part of the abdomen, terminated naturally. I have seen her twice since. The vagina admits readily the index-finger. The os uteri is felt of its natural size, but the cervix is very ill defined. She is quite well, and menstruates regularly.

Case 2 was under the care of my friend and colleague Mr. Briscoe. I am indebted to his kindness for permission to watch the case while under treatment, and for the greater part of the following account.

M. A. C—, æt. 19, was admitted into the Radcliffe Infirmary on October 10th, 1866. She states that seven years ago she attended at this Infirmary as an out-door patient, for bad pain in the lower part of the back and left groin, especially severe at one period in every month. As she derived no relief from medicines, she left off attending, and shortly afterwards noticed a swelling in the lower part of the belly. As she grew older, her sufferings increased, still being aggravated once every month, and she was often compelled to have her water drawn off with an instrument. Five years ago, a small quantity of black, thick stuff escaped from the passage; and this was soon followed by the discharge of a little stinking matter. This discharge has constantly been present ever since; always scanty, invariably oozing out in small quantity directly after she has passed water, and attended with so abominable a stench that no one can bear to be near her. She has suffered repeatedly from attacks of vomiting, and declares that life is insupportable from pain and misery.

A pale, unhealthy looking girl. On examining the abdomen, a large, painful, ill-defined tumour is felt in the left half of the hypogastric and left iliac region. The labia and nymphæ are very large, hypertrophied, elongated (reminding one of the prepuce of a boy

with stone in the bladder), and moist. Behind these, when separated, no trace of a vaginal opening can be seen. There is no protrusion of the hymen; and it is only when the parts are stretched by widely separating the legs, that an elastic sensation, as of distant fluctuation, is communicated to the finger pressed on what should be the orifice of the vagina. The opening through which pus escapes after micturition cannot be detected; but there is little doubt that there is a small, probably fistulous, communication between the urethra just at its orifice and the distended vagina. That there is no opening from the vagina into the bladder, is clear from the fact that the pus is always discharged unmixed after the clear urine has been voided, and that the urine drawn off by a catheter is perfectly clear and natural. The finger in the rectum detects a large, globular, fluctuating tumour, which presses back the anterior wall of the bowel.

Chloroform being given, the urethra is hooked up under the pubes with a male sound; the forefinger of the left hand is passed into the rectum as a guide, and a scalpel is carefully pushed into the vagina in the direction of the tumour. This is followed by a gush of abominably fetid pus, about twelve ounces in quantity. A bit of oiled lint is introduced into the wound.

She progressed steadily without a bad symptom, fetid pus continuing to discharge from the wound, which gradually contracted and almost closed. Meanwhile the abdominal tumour disappeared.

On the tenth day after the first operation, the occluding diaphragm was again and more freely divided by a vertical incision. The diaphragm could be felt by the finger passed into the vagina to be very tough and quite half an inch thick, so that the deformity would be, perhaps, called occlusion of the vagina rather than imperforate hymen. The vagina had contracted to nearly its natural size; and, quite filling its upper end, could be felt the large and gaping os uteri without any cervix. The opening was subsequently kept patent by the occasional introduction of the finger.

She is now fat and healthy. The vagina is of its natural size. The large, thin-lipped os uteri is felt completely blocking up the upper end of the vagina, and is evidently fixed firmly to the parts around, without any trace of cervix to be felt. She has not menstruated.

Case 3 forms a fitting supplement to the two which precede it. It is a preparation in our Pathological Museum.[1] I do not know

[1] This excellent nucleus of a Pathological Museum owes its existence mainly to the exertions of Dr. Acland. It contains 1070 preparations, besides a rich collection of calculi, presented by past and present surgeons of the Radcliffe Infirmary—Mr. Hitchings, Mr. Hussey, Mr. Symonds, Mr. Briscoe. Of these 1070, 739 were recently purchased by the University, at Dr. Acland's suggestion, from the relations of the late Professor Schroeder-Van-der-Kolk, after his death.

what the symptoms were which preceded the death of the girl, in whom no operation had evidently been performed; but they were, doubtless, very similar to those described in the above cases. The following is, with a few slight additions, the description of the preparation written by myself in the Catalogue:

A large double sac, not unlike an hour-glass in form, is seen suspended in spirit. The lower half of the sac—the enormously dilated vagina—is of about the size of a fœtal head. The upper half—the uterus, so distended that its walls are not thicker than those of the bladder—is smaller than the lower half, from which it is partially separated by a very thin septum (the remains of the stretched cervix uteri), but with which it communicates by an aperture of the size of half-a-crown, in the centre of the septum. One side of this double sac has been sliced off so as to display the interior. On looking through the opening thus made, it will be observed that in the lower sac, at the part where the natural vaginal orifice should be, there is no vestige of any orifice, but the wall of the sac is as thick and complete there as in any other part. The labia with hair attached to them are left *in sitú*, and a black tube is passed up the urethra into the bladder, a section of which is preserved. The greatly distended right Fallopian tube, with the ovary and broad ligament, is twisted back and bound by adhesions to the back of the uterus. This condition of the parts, as well as the abundant flocculent false membranes which beset everywhere the peritoneal investment of the uterus and Fallopian tube, and which have been minutely injected with a blue fluid, bear evidence of chronic peritonitis. *The uterine opening of the tube is about as large as a crow-quill.* The tube itself, at a very short distance from the uterus, is dilated to such a size as to admit readily a finger. A little further on, at a short distance from the fimbriated extremity, the tube is bulged out in a somewhat saccular form, and would admit readily two fingers. It then rapidly diminishes in size to the fimbriated ex-

Many of the specimens are unique; such as, for instance, the injections of lymphatics with mercury, which illustrate Van-der-Kolk's discovery of newly formed lymphatics in false membranes. There is, further, an admirable series of pelvic deformities, from the slightest obliquity, a consequence of unreduced dislocation of the femur, up to the most extreme contraction from mollities ossium. Besides these, a whole trunk, with the legs detached, shows in a remarkable manner the deformity caused by mollities. Van-der-Kolk's connection with the asylums of Utrecht and Amsterdam enabled him also to collect no less than 112 skulls; some from lunatics, others from idiots; some representing disease, others injury of the cranial bones. The remaining 331 specimens came chiefly from the Christ-Church Museum, and have been for the most part dissected and prepared within the last twenty years by Mr. Charles Robertson. Van-der-Kolk's MS. Catalogue has been translated by the writer, and the two collections are now amalgamated, and arranged according to the plan adopted in the museum of the College of Surgeons. A Synopsis of the general pathology, framed after the model of the Synopsis of our Anatomical and Physiological Collection, and in continuation of it, is now in the press.

tremity, which, with the ovary, is, as already stated, firmly bound to the uterus by tough bands of adhesion. The left tube, also largely distended, though less so than the right, was not twisted back, but occupied its normal position. It has been removed with the slice taken away in the dissection and preparation of the sac. The sac and the tubes were filled out with black, tar-like blood. *Nowhere could any rent be detected in either of the tubes.* The rectum is contracted so as scarcely to admit the little finger, and so flattened against the posterior wall of the pelvis by the pressure of the distended vagina (which must from its size have quite blocked up the pelvis) that no passage of fæcal matter can have taken place for some time before death.

Remarks.—These three cases seem to be worthy of publication on several grounds:—first, because they fairly represent the general condition of, and symptoms present in, girls who are thus deformed; secondly, because they show the necessity of immediate operation, and the almost inevitable consequences which follow if no operation be undertaken; thirdly, because they exemplify the danger of death from peritonitis even after the operation, simple though it be, has been performed; fourthly, because they bear witness to the wonderful power of Nature, who can restore to their proper size and relations parts so extraordinarily distorted; fifthly, because they illustrate by a post-mortem picture, more fully than has, I think, hitherto been done, the amount of distortion to which the several parts of the genital apparatus are subjected by so slight a deformity.

I have spoken advisedly of the *almost* inevitable consequences of leaving the case to itself. For it has happened that the occluding membrane has spontaneously ruptured, and the impending death has thus been averted. Scanzoni[1] relates a case where this occurred, and about two pounds of stinking treacle-like blood rushed out, to the consternation of the girl and her friends. Dr. A. Farre[2] has known the very same thing happen. There can be no doubt that the same thing occurred, though very imperfectly, in Case 2, above narrated, and that the opening which allowed the small quantity of the contents of the vagina to escape suffered also air to enter, and to convert the whole into a great foul abscess.

The fact seems remarkable, but it is nevertheless a fact, that after the unimportant operation of dividing the membrane, even though the girl seem to be progressing quite favorably, there comes a time when the whole aspect of the case may suddenly change, when peritonitis may set in and rapid death follow. This danger is imminent any time between the second and fifth day after the operation; but the third day seems, on the whole, to be the most critical. Bernutz and Goupil[3] have collected several such cases. In two out

[1] Die Krankheiten der weiblichen Sexualorgane, p. 493.
[2] See 'Clinical Notes,' by Dr. Marion Sims, p. 326.
[3] 'Sur les Maladies des Femmes,' p. 30 et sqq.

of four of the kind operated on and related by Sir B. Brodie,[1] one nearly died of peritonitis after the operation; the other died, and a quantity of the menstrual fluid was found in the peritoneum. There can, be no doubt, too, that sharp peritonitis supervened in the first of my cases *on the third day after the operation*, when everything seemed to be going on well. When this peritonitis does not yield to remedies, but is followed by death, blood resembling that in the uterus and tubes is found in the peritoneum; in some cases the Fallopian tubes being ruptured;[2] in others no rupture being detected, but the blood being apparently forced backwards through the abdominal opening of one or other of the tubes.[3] A careful examination of Case 3 suggests an explanation of this difficulty—why, namely, these severe symptoms of peritonitis do not, as a rule, show themselves before the operation and only at a certain interval after it. A uterus distended as in that preparation must of necessity have become completely paralysed, and have quite forgotten its power of contraction. Relief is then given by the operation to the pressure from below: but it is only *after a time* that the uterus can recover itself. It then slowly contracts on its contents, and, so doing, it must tend to force part of them through the widely gaping mouth of the already distended Fallopian tube. The tube now either gives way, or some of its contents are squeezed back through its abdominal opening into the peritoneum. I submit, therefore, that it is more probable, considering how widely open the uterine mouth of the tube is, that the uterus causes the accident *directly* by driving the fluid backwards, than *indirectly*, as some suppose, by contracting on the uterine mouth of the distended tube, and thus preventing the escape of the fluid out of the tube onwards into the uterus. In Case 1, it will be seen that the menstrual fluid did not all escape at once, but continued to ooze out *gradually* as late as the third day after the operation. The first gush came, plainly enough, from the vagina; and the remainder escaped little by little as the uterus recovered its tone and contracted on its contents. There can, I think, be little doubt that a small quantity of the fluid escaped from the left tube and found its way into the peritoneum. It might seem to some that the pus which collected in the vagina was the cause of the peritonitis; but against any such supposition and in favour of the former view are the facts, that the onset of the pain was sudden, as if something had given way, and exactly at the seat of the left tube; that the girl was steadily recovering from the peritonitis before the discharge of pus took place, and that the discharge was not followed by any marked relief to the symptoms.

[1] 'Lond. Med. Gazette,' 1841, p. 810.

[2] See Locatelli's case, Bernutz et Goupil, op. cit., p. 55; Paget, 'British Med. Journal,' July 23rd, 1859; Munk, 'Lond. Med. Gazette,' vol. xxvii, p. 867.

[3] Sir B. Brodie's case, loc. cit.

So great is the reparative power after the operation has been successful, that not only, as in Case 1, and in five of Scanzoni's cases,[1] does menstruation go on regularly as if no disease had ever been present, but women have even been known to conceive and bear children.[2]

Lastly, the following points in the treatment of the disease suggest themselves:—that the simpler the first operation, the better for the patient;[3] above all things, that any pressure on the abdomen or any forcible attempts to evacuate the blood be most scrupulously avoided; that the recumbent posture be strictly kept and the most zealous care be taken to procure perfect rest for at least five days after the operation; that at the end of eight or ten days the second and final operation be undertaken; a free crucial incision, as recommended by Mr. Hutchinson, being then made, and the opening being kept patent by some such plan as that devised by Mr. Briscoe. The subsequent introduction of one or two sponge-tents will help to establish a permanent vaginal opening.

Article III.

A Preliminary Notice of the Akazga Ordeal of West Africa, and of its Active Principle. By Thomas R. Fraser, M.D., F.R.S.E., Assistant to the Professor of Materia Medica in the University of Edinburgh.

(*Read before the Royal Society of Edinburgh, 29th April*, 1867.)

1. *Employment as an Ordeal, and General Characters.*—In several recently published works on African travel, accounts are given of the remarkable and violently poisonous properties of an ordeal called akazga. This would appear to have been first described by Du Chaillu, and submitted by him for examination to Professor John Torrey, of New York, in 1860. The quantity given to that gentleman was insufficient for a detailed investigation, but he reported that the plant appeared to belong to the Loganiaceæ, and that it was probably a species of strychnos.[4]

[1] Loc. cit.

[2] See Hutchinson, 'Holmes's Surgery,' vol. iv, p. 494; Merriman, 'Lond. Med. Gazette,' 1841, p. 810.

[3] Dr. G. Hewitt and Dr. Marion Sims propose to simplify the first operation even more than was done in the above cases. They recommend that a very minute opening be made in the occluding membrane, so as to let the fluid ooze out drop by drop, believing that by so doing they lessen the chances of subsequent peritonitis.

[4] 'Explorations and Adventures in Equatorial Africa,' by Paul B. Du Chaillu, p. 257.

Soon after this, a description of its employment in judicial investigations and many other particulars were published by that most entertaining of writers on African travel, Mr. Winwood Reade.[1] Some specimens of the plant were presented to the London Pharmaceutical Society, in 1862, by Mr. Simmonds, and were chemically examined by Messrs. Attfield and C. H. Wood, who believed that they obtained indications of the presence of strychnia.[2] In November, 1866, a paper was read before the Academy of Sciences at Paris, by MM. Pécholier and Saintpierre, in which the results are given of the examination of the toxic properties of a Gaboon ordeal, which they term "boundou," and which I have no doubt is the akazga ordeal. They conclude that this poison has a very similar action to strychnia, producing tetanic convulsions, and in no direct manner affecting the cardiac action.[3]

A few specimens of the akazga were sent to this country, in 1864, by the Rev. A. Bushnell, of Baraka; and these were very kindly given to me for examination by Mr. Thomson, of Glasgow; and a further supply came from the same quarter in 1865. I am also indebted to these gentlemen, and to Dr. Nassau, of Bonita, for the very interesting and trustworthy information they have supplied regarding the employment of the ordeal; by which, principally, I am enabled to communicate the following particulars.

The poison is known among the various tribes who employ it as *akazga*, *boundou* (or *m'boundou*), *ikaja*, and *quai*. Akazga is probably derived from *nkazga*, which signifies pain or hurt. It is employed as an ordeal on the West Coast of Africa, in a district which extends for a considerable distance north and south of the equator and many miles inland, and also in the adjacent island of Corisco. Witchcraft is believed in almost universally over Africa as the cause of all deaths, of unexplainable misfortunes, and of many crimes; and to detect the witch this poison is employed. The medicine-man, who generally owes his exalted and influential position to the impunity with which he can swallow large doses of his favorite drug, names those who are to be subjected to the trial. The supposed witch—who may be of either sex and of any age, and who is frequently, therefore, a very different person from our ideal—is obliged to drink a certain quantity of the infusion prepared from the bark, and to step over a number of akazga sticks placed parallel to one another at the distance of two feet. If this can be done, the person tried is pronounced innocent; if guilty, difficulty is

[1] 'Savage Africa,' 1863.
[2] 'Pharmaceutical Journal and Transactions,' vol. iii, 1861-62, p. 300.
[3] 'Comptes Rendus,' 1866, p. 809.

experienced in stepping over the sticks; they appear like large logs, to surmount which suitable efforts are made, and these are rendered more and more difficult by spasmodic muscular twitches, until the victim staggers and ultimately falls in tetanic convulsions. If the poison do not now immediately prove fatal, the knives and clubs of the onlookers quickly free the tribe of the supposed witch-possessor. In those cases in which the trial is successfully undergone, a copious flow of urine is described as occurring, and by this means the poison is supposed to be removed.

It is believed that several thousand persons are annually subjected to this ordeal, and that the fatal cases are about 50 per cent.

The akazga is not only used as a detecter of witchcraft, but also as a protector against its influence. Portions of the root are placed over and under the door for the common good of the household, or the charred wood is rubbed on the forehead to confer its peculiar benefits on the individual. It is used for numerous other superstitious purposes of a like nature to those customs which have existed in many countries now enjoying greater enlightenment. It also possesses a medicinal reputation, and is used in the treatment of various skin diseases, such as krakra,[1] and in suppression and other urinary disorders.

The akazga was sent to me in bundles which consisted of long, slender, and crooked stems, having their roots generally attached to them, but sometimes their leaf bearing branches only, and containing also a few complete plants, with roots, stem, and branches. The plant is usually about six feet in length; but some specimens were only four, and others as long as eight feet. They have generally a diameter of half an inch, and this varies from a quarter to one inch. The bark is of a yellowish-orange colour, and in some parts light red; and over it a grey efflorescence is frequently found. A few of the stems are of a dark-brown colour, with numerous yellow tubercles. The bark adheres firmly to the stem, but it can be readily detached after exposure to a gentle heat for some days. Its internal surface is light-brown. The space between the bark and the wood was found in a few pieces of akazga to be occupied by a large number of minute sparkling crystals, but it has not yet been determined whether these consist of a vegetable or mineral substance.

The wood is dense and hard, and, from the number of concentric circles, the plant is apparently of very slow growth.

The leaves are opposite, and oval-acuminate in form; the

[1] A pruriginous eruption common in West Africa.

apex frequently consisting of a linear prolongation more than an inch in length. They have five parallel ribs, three of which are prominent.

The bark has an aromatic, strongly bitter taste, which is not persistent, and a slight bitterness may be perceived in the leaves and wood.

These general characters at once suggested that this plant is one of the Loganiaceæ; but, with the materials in my possession, it was impossible to identify it. Professor Belfour kindly placed at my disposal a large collection of West African plants, and with his assistance and that of Professor Dickson these were compared with the akazga, but we found nothing of an exactly similar description. Professor Oliver, of Kew, who is intimately acquainted with the flora of West Africa, also examined some of my specimens, and considers that there is great reason to suppose that the plant is an undescribed one.

2. *Chemical Properties.*—When the inner surface of the bark is exposed to the action of nitric acid, a brown colour is produced. Hydrochloric acid causes a pinkish red, which changes to brown; and sulphuric acid, a brown colour, and subsequent charring.

Nitric acid caused a brownish orange on the outer surface of the wood; hydrochloric acid, a faint yellow with reddish spots; and sulphuric acid, a brown, followed by charring.

The dry bark may be readily reduced to a fine powder in a mortar. If this be boiled with successive portions of alcohol of 85 per cent., a clear, reddish-brown tincture is obtained, which becomes clouded on cooling. By distilling off the greater portion of the spirit, and evaporating the residue at a low heat in a vapour-bath, a brown, shining extract is procured, weighing from 12 to 15 per cent. of the bark employed, having a bitter taste, and, with concentrated nitric acid, changing to a brownish-yellow colour, which was not materially affected by heat, nor by solution of protochloride of tin. It is obvious that the active principle of akazga is contained in this extract, and to separate it the following method has been adopted, after several attempts with various processes:—

The extract is treated with a very dilute solution of tartaric acid, which removes 77 per cent., and then filtered. The clear, yellowish-brown, acid solution is shaken with successive portions of ether so long as any colour is removed, and by this means also a small quantity of an aromatic oil is separated from it. After decantation, a solution of carbonate of sodium is added to the liquor, as long as it causes a nearly colourless, flocculent precipitate, which can readily be ascertained by filtering and testing a small quantity. It is again shaken with ether, the decanted ether is agitated with three successive portions of distilled

water, by means of which all impurities are removed, and finally received in a bottle containing a dilute solution of tartaric acid, and shaken with it. As soon as the ethereal solution is brought in contact with the acid, it becomes opalescent, but again assumes its normal appearance when agitated with it. This change is of some value in indicating the frequency with which the alkaline solution should be treated with ether, as, when the former becomes exhausted, this will no longer produce a milkiness in contact with tartaric acid. When this stage is reached, the tartaric solution is exposed to a gentle heat—to free it completely from ether—filtered, and again treated with carbonate of sodium, by means of which a bulky, colourless, and flocculent precipitate is obtained. This is collected on a filter, washed, and dried by exposure to a gentle heat for a short time, and then by the action of sulphuric acid *in vacuo*.

By this means, a colourless, amorphous substance is obtained, which is the active principle of the akazga poison, and which possesses the general properties of a vegetable alkaloid. From 500 grains of the powdered bark I succeeded in obtaining 10 grains, or 2 per cent. An examination of its chemical properties proved it to be a new and hitherto undiscovered substance. For it I propose the name *akazgia*, which is derived from akazga—apparently the most usual, as it certainly is the most euphonious, of the synonyms of this ordeal-poison.

As this alkaloid has been separated from a plant which is almost certainly new to science, I have to express the hope that when the plant is described, akazga will be adopted as its specific name, and thus the usual connection of nomenclature between the vegetable alkaloid and its source will be maintained.

Akazgia is soluble in about sixty parts of cold absolute alcohol; in about sixteen parts of spirit of 85 per cent.; in about one hundred and twenty parts of anhydrous sulphuric ether; and in thirteen thousand parts of distilled water at a temperature of 50° Fahr. It is freely soluble in chloroform, in bisulphide of carbon, in benzole, and in sulphuric ether of specific gravity 0·735. It seems to crystallize with difficulty, but may be obtained in the form of minute prisms by the very slow evaporation of a solution in rectified spirit. An analysis of its platinum salt, and a determination of its combining proportion with dry hydrochloric acid, yielded 290 in the former, and 293 in the latter, as the equivalent of akazgia. A sufficient quantity has not been yet obtained to determine its formula.

Akazgia and its salts when heated become yellow, then melt, and give off fumes of a pungent, disagreeable odour, and finally become charred, but leave almost no residue if the heat be continued for a sufficient time. When so treated in a small test-tube,

these fumes have an alkaline reaction; but when burned on an open plate they are acid.

Its solutions have an alkaline reaction and neutralise acids; and the salts are freely soluble in water, and have a very bitter taste. The bitterness is, however, very different from that of strychnia in its intensity and persistence. I found that when the $\frac{1}{100}$th of a grain of strychnia was dissolved in very dilute hydrochloric acid and placed on the tongue, an intense bitterness was caused, which continued for more than three hours. In an exactly similar experiment with akazgia, the bitterness produced was not so intense, it had greatly diminished in an hour and a half, and it had completely disappeared in an hour and forty-five minutes.

Concentrated nitric, hydrochloric, and sulphuric acids change its colour to brown; but these in a diluted state—as well as many of the organic acids—form nearly colourless solutions with akazgia. It is precipitated from these solutions by hydrate, carbonate, and bicarbonate of sodium and of potassium; by ammonia; by iodide, sulphocyanate, ferrocyanide, and chromate of potassium; by phosphate of sodium, protochloride of tin, trichloride of gold, dichloride of platinum, potassio-mercuric iodide, carbazotic acid, tincture of galls, solution of iodine, and various other substances: but these precipitates are *never* crystalline. Corrosive sublimate causes an amorphous white precipitate, which is dissolved by heat, and reappears in a non-crystalline form when the solution has cooled. Chlorine produces a colourless amorphous precipitate which does not disappear on the addition of ammonia. When akazgia is treated with nitric acid and heat, a yellowish solution is formed; and protochloride of tin causes in this a grey precipitate, which is dissolved by again applying heat. With concentrated sulphuric acid and peroxide of manganese, bichromate of potassium, or any other of the usual oxidizing agents, the same succession of colours is produced (from blue to brown) which results from a similar treatment of strychnia. A violet colour is also produced by electrolysis.

3. *Physiological Action.*—The physiological action of the alcoholic extract of akazga is very similar to that of extract of nux vomica. In a gradually increasing series of doses, it was found that six tenths of a grain is the minimum fatal dose for a rabbit weighing about three pounds. When this was injected into the subcutaneous cellular tissue, reflex movements were exaggerated in nine minutes; tetanus occurred in ten, and the animal was dead in eleven minutes.

Five tenths of a grain given to a rabbit of the same weight caused a violent tetanic convulsion in eighteen minutes; and convulsion after convulsion rapidly succeeded each other, with

gradually diminishing severity, until fifty-four minutes after the exhibition of the poison, when the animal assumed a natural position, and recovered so quickly as to appear perfectly well in little more than an hour.

The active principle, akazgia, produced exactly the same phenomena in corresponding doses. According to the analysis, each grain of this alkaloid should equal seven grains of extract, or fifty grains of dry bark; and I found the physiological results to agree admirably with this.

One tenth of a grain of akazgia was dissolved in a few minims of very weak acetic acid, and injected into the cellular tissue of a rabbit weighing three and a quarter pounds. A spontaneous and slight spasm, with increase of sensibility, occurred in four minutes; a long-continued tetanic convulsion, commencing with a violent spring, followed in eight minutes, and this was immediately succeeded by a second; on the termination of which, a succession of muscular twitches occurred, and continued for a few seconds after the respirations had ceased. The rabbit was dead in eight minutes and forty seconds after the administration of the poison.

To a rabbit weighing three pounds, one twelfth of a grain of akazgia was administered in exactly the same way as in the last experiment. No effect was produced until eight minutes, when its back became slightly stiff, and this was soon followed by a spontaneous spasm. The rabbit fell in a tetanic convulsion in fifteen minutes, and the effort which it made to raise itself when this had ceased induced another convulsion. These continued to succeed each other until twenty-two minutes, when a very violent attack of tetanus occurred, at the end of which the rabbit lay for many seconds without any respiratory movement, and with its muscles faintly quivering, exactly as they do at the termination of a fatal administration. Instead of dying, however, a sudden gasp occurred, and was followed by respiratory movements, which quickly became normal. When the rabbit attempted to rise, the posterior half of its body was found to be partially paralysed, and it could barely elevate its head and shoulders with its yielding forelegs. There were, however, no further spontaneous spasms, though slight convulsive movements could be readily excited by faint touches; and from this condition, also, it completely recovered in an hour and forty minutes. The paralysed state gradually disappeared, and in about two hours after the exhibition of the poison the rabbit was jumping about in an apparently normal condition.

These experiments are sufficient to show that the active principle of akazga has exactly the same action as the extract, and a proportional activity to it.

It is difficult to make an exact comparison between the

effects of akazgia and those of strychnia, as I have not succeeded in finding any investigation in which the minimum fatal dose of the latter substance was attempted to be determined. But, whether peculiar to akazga or not, it is interesting that a certain quantity of an ordeal-poison should be able to produce a condition which appears to approach as closely as possible to death, and that an extremely slight increase of the dose capable of doing this should very quickly cause violent symptoms and a rapidly fatal termination. The medicine-man is thus enabled, by administering a proper dose, to produce effects with such rapidity as to prevent his victim from completing the test of stepping over a certain number of akazga sticks; while, should he so wish it, a very slight diminution of quantity will postpone the appearance of the symptoms, until this, in itself easy, task can be accomplished.

Another point of interest is, that an alkaloid should be found so closely resembling strychnia in its chemical and physiological properties. There are several instances in which the same natural order produces very similar active principles, so that this is by no means an unexampled occurrence. In the Loganiaceæ itself, strychnia, brucia, and igasuria already exist; and these are nearly identical in their physiological actions. In chemical properties, brucia and igasuria have much in common; and they are both readily distinguishable in this respect from strychnia. Akazgia conveniently completes this group, as its chemical properties are nearly allied to those of strychnia, whilst its connection with all the members is maintained by the similarity of its physiological actions.

Article IV.

Contributions to the Current Literature of Cholera. By George Gaskoin, Surgeon, Knight of the Portuguese Order of Christ. London.

We are told that by the word "murree," derived from the primitive dialects of India, the disease known to us as Asiatic cholera is certainly indicated. It occurs in some of the puranas or theological writings of the Hindoos of a yet older date than the Brahminical Shaster. The word is not of Sanscrit extraction, though adopted by the Brahmins. In its original signification, it would appear to mean "the death." There exists, it is said, at Vizianuggur, before the site of an ancient temple there, a monolith whereon is engraven a description of cholera, which in point of accuracy can scarce admit of challenge. "Blue lips, a shrunken face and hollow eyes, the knotted belly, with limbs cramped and curled as if by fire," characterise the

"murree," "which descends by wicked conjuration of the priest to destroy the righteous brave. The breath clings thick to the warrior's face; his fingers are probed, snaky, and clawed, while in writhings he expires, the victim of Siva." This inscription is referred to an age contemporaneous with the growth in Europe of the Athenian power, that is to say, more than a century before the invasion of India by Alexander; and even then, if we may believe so much, the temple before which it stands was of ancient date. From the spirit of detraction which its language betrays against the Brahminical priesthood, it has been attributed to some disciple of Budha, the celebrated reformer of the Brahmin sect. Critics have not been wanting who would refer it to a comparatively early date. However this may be, the distich is known to the Hindoos of the present day. It has been adopted into their sacred books, and is sung at their *poojahs*, or festivals, in deprecation of the wrath of Siva.

This scrap of antiquity, which has found its way to the public in an unimposing form through a publication of some merit which deserves more attention among us than it has obtained,[1] seems to show that cholera was early known in India as a contagious disease, or at least as a disease of the camp. Such native descriptions as are elsewhere met with seem hardly to tell so much. A slight allowance being made for difference in intensity, it can scarce surprise us more to meet with a description of cholera in Indian or Chinese literature than in works of domestic medicine, such as the pages of Buchan, among ourselves. It seems nowhere that we know of mentioned in the native writings as a plague, and no measure is given of its universality. In nothing do the ideas of the ancients seem less advanced than in respect to contagion, and perhaps in this field the convictions of the vulgar have ever been held in check by the sapiency of physicians. When we learn from the native annals of medicine that smallpox is caused by indigestible food, we need not be surprised at cholera being regarded simply as an "indigestion." Popular conclusions, however, often serve to correct doctrinal teaching. From the excellent writer Orton[2] we catch some of the conclusions of the Hindoo mind upon this subject. "The natives of India," he says, "are an unenlightened race. Some idea of the value of their opinion on any doubtful subject may be formed from the fact of their universally believing that the malaria fevers are owing to drinking bad water!! But in some instances they had discovered the cholera to be contagious. Mr. Chapman, in the Bengal Report, states they commonly avoid the sick as much as possible, and desert infected villages." He also mentions the

[1] 'Suggestions in reference to the present Cholera Epidemic,' by William Sanderson, C.E. Macintosh & Co., London, 1866.

[2] Orton, 'On Cholera,' p. 315, 2nd edition, 1831.

native physicians abandoning their charge for fear of contagion.

Some writers on cholera have affected to divide the disease accordingly as it proceeds from a bodily derangement, or simply from external causation; where the cause is in the constitution, the case of the patient is allowed to be far worse. If, as some have opined, the essential character of the disease lies in a shedding of the epithelium and an intestinal lymphorrhœa, an impression which is mechanical or physico-chemical might be adequate to these effects above all in an exhausted or attenuated frame.

On the other hand, the recent researches of Beale are favorable to the idea that the foundation for the complaint is laid in some local flaw or impairment of nutrition in the delicate epithelial lining of the intestines.

In most countries, the crude and inappropriate quality of the ingesta is held sufficient to account for cholera. A Portuguese physician of note, writing in the beginning of the seventeenth century,[1] says, that in the northern parts of Africa and Arabia the inhabitants are much subject to this disease in a bad form, from the use they make of an unleavened cake of flour and oil ill-baked, which they eat cold, called "cuscus" in their vernacular. He identifies the Asiatic with the European form of the disorder; but it is far worse, he says, in India than in Lisbon or Amsterdam.

The highly esteemed and unfortunate Amatus,[2] his predecessor, in treating of the disease as it occurs in Portugal, attributes much of it in his Jewish co-religionists to their use of cold food. He is extremely prolix on the score of radishes. "This esculent," he says, "digests everything but itself," *distribuit sed non distribuitur*. Set to stand in a cup of water, it speedily corrupts the element. But this is childish. When a particular contagion is dominant, or an "epidemic constitution" confirmed, a piece of undigested gristle or stone fruit, and possibly a trip with the foot or fret in the temper, may bring on fever, ague, plague, or cholera. Inexplicable as this may seem, we may well admire the evasion of the physicists who pride themselves far more on their exactness. In a series of effects, which they cannot at all understand, as when we see water burst out suddenly into steam at a determinate temperature on a slight succussion (such as a leaden shot dropped into its midst), or when a salt solution sets quickly in crystals from a trifling point of contact, and other more remote effects, they adopt the words "molecular sensibility."

Most authors in treating of cholera insist on the time of

[1] Zacuti Lusitani, 'Op. Omnia,' Lyons, 1649.

[2] Amati Lusitani, 'Medicin. Cent.,' Venetis, 1653. For an account of his misfortunes read 'Purchas, his Pilgrimes,' p. 661. London, 1625.

year in which it appears. Sydenham deems it singular that it should be sure to come in August, yet cease with the advent of September: and this writer also speaks of the disease as an "indigestion." At certain seasons of the year, in England, as in other parts of Europe, cholera has ever presented itself in exceptional instances as a formidable disease; but, in fact, the English form is little liable to be confounded with the imported Asiatic disorder of the same name, however mitigated the latter may have become in the transit to our shores. At the same time it must be allowed that the comparative intensity of the symptoms is what we rely on as a means of distinction. Let the faculty be allowed to any constitutional disorder of becoming contagious; we know that they vary infinitely in this respect from ophthalmia to syphilis. We are still wholly ignorant of the laws which bestow on a sporadic complaint the type and currency of a contagious plague, enabling it to renew and repeat itself successively and multitudinously for a certain cycle in time. It must yet be acknowledged that some existing forms of disease are simply the result of contagion, of which a sure instance offers itself in the transplantation of smallpox to the Western Hemisphere. There is, it is true, much in the character of river, coast, hill, and climate in the peninsula of Hindostan to flatter the idea that malignant cholera has its birthplace there; but even in India its behaviour is often rather that of an imported plague which arrives all at once from a distance to the point of incidence, rather than evolved as an aggravation of a seasonal complaint. So much of mystery, indeed, surrounds the cradle of an established contagion, so insidious and tentative are its first approaches, and so permanent in most cases its tide of conquest, that we may well hesitate to pronounce in any instance that it has originated out of particular local conditions or area of soil, when experience shows that by sheer contagious power it can be readily introduced and acclimatised in every land and among all classes of people.

It may be that this particular contagion is as natural to the climes of Asia as camp-fever to extremes of war in Europe; yet evidence fails us precisely on this point: indeed, we do not so much as know with any certainty whether the kind of fever which now inevitably ensues on distress in armies has in all ages been a denizen of Europe; nor with more of confidence dare we say that epidemic and contagious cholera has up to the date of 1831 been ever a stranger to our shores. Amatus, Moyses Alatius, and Zacutus, all aver that they have seen contagious colic complicate other pestilence, just as in India cholera and malignant fevers alternate with or accompany each other. As regards camp-disease in Asia, many sources of information disappoint us, though the field of research is far from exhausted.

The gigantic proportions of Asiatic armies at various epochs of history, the want of compactness and strict regulation that distinguishes them from those of Europe, and the prodigious recklessness of human life, especially in the destruction of large cities, which have marked their progress, seems to present the very conditions which would convert a seasonal into a contagious disease. Yet, if cholera had been a subject of popular dread from the frequency of its occurrence beyond the borders of Hindostan in the sixteenth century, we might hope to learn it from the pages of the many travellers who have written about those times.

From Ludovicus Romanus, who was in India A.D. 1506, we derive no help whatever, nor from Antonio Tenreyro who went over the same ground in Western Asia A.D. 1526. Though glanced at by the fine point of Congreve's wit, Mendez Pinto is in favour with his countrymen, and his observations have pretty well stood the test of time. A voluminous writer, he is also a fair sample of the adventurers of that period—one of those waifs and strays from the mother-country who roamed over the Eastern continent as necessity or interest impelled in the service of their own or some native sovereign, or else embarked in a private venture. Many years he spent in armies, among crowds and prisons, well acquainted with starvation and misery in varied forms—a man of loquacious, social, communicative turn. The notices which his volumes contain of disease are interesting, nor are they few; but he has left no record of cholera.[1] He serves the King of Burmah, who takes the field with a vast host amounting to many hundreds of thousands, a prodigious tyrant, and in his way a great conqueror too. At the siege of Prom, after an unsuccessful assault, more than 30,000 dead bodies lie strewed about the camp, with daily additions to their number from the wounded. The air, and especially the water, it is remarked, is infected with blood and putridity. As might be expected, a pestilence ensues, through which, according to report, more than 80,000 died, and among them 500 Portuguese; but we have no proof that this pestilence is cholera. Frey Gaspar de Cruz, who went eastward to China, describes, A.D. 1569, earthquakes and dire pestilence from seasonal change, but without particularity. He also is present in a camp where dead bodies lie about thickly strewn, and wishes himself back to Goa. Indeed, we need yet to be informed whether putrefaction of animal remains which produces contagious fevers and dysenteries in Europe, even in Asia any more than here, can produce cholera.

Vasco da Gama, as is well known, rounded the Cape, and arrived at the roadstead of Calicut, on the western shores of

[1] 'Peregrinacao de Fernando Mendez Pinto,' prim. edicao, 1614, Lisboa.

India, in the year of our Lord 1498. He returned to Lisbon in the August of the year following, leaving no factory in Hindostan, his voyage being one of discovery. The intimate relations of the Portuguese with the natives of India only began with the next century, after the arrival in Calicut of the second larger expedition under Cabral, nor is there any point of history to interest us until the massacre of fifty of their number by the populace, instigated by the Zamoryn sovereign of Calicut. This cruel and treacherous prince proved a bitter enemy to the Portuguese, who, however, established themselves in alliance with the king of Cochim. Of the histories of that period, some of high value remain in manuscript, others have found their way into print. The 'Lendas da India,' by Gaspar Correa, vol. i, book iv, p. 489, published by the Lisbon Academy of Sciences, in recent times, contains the earliest notice of cholera by a European hand which I have seen. After describing at large the exploits of Duarte Pacheco, in command of the allies when defending the coast of Cochim against the Zamoryn during the spring of the year 1503, at the close of the chapter the chronicler expresses himself thus:—"Now, the king of Cochim was on his guard, and he learned for certain that in the course of the war the loss on the side of the Zamoryn could not be less than 20,000 men, in nairs and oarsmen, besides the many who were wounded and disabled. To which greatly contributed the current spring disorders, and also smallpox; besides which there was another disease, sudden-like, which struck with pain in the belly, so that a man did not last out eight hours' time."

In these early descriptions of cholera the disease is always termed "uma dôr," a dolour or throe, which expression answers to the "griping tortures," &c., of our descriptions. When the Portuguese[1] arrived in India, the Malabar coast swarmed with Mahometans, but the war was carried on chiefly by a sort of Hindoo Mamelukes, the Nairs of this country, a fighting caste, Soodras, but very intolerant and proud, who, no doubt, took care to keep the line distinct between themselves and the followers of the Prophet. The following description of their habits of ablution may be found interesting, as it touches on an important point—the degree of respect which is due to Hindoo civilisation, in its bearing on disease. It is derived from the writings of Linschoten, a Dutch traveller of the next generation, when, however, the manners of the natives would not have undergone much change. "In every place," he says,

[1] In their sieges in India, we read of the Portuguese fighting for a spring, and of their feeding on dogs, cats, and carrion. When three elephants are killed on the shore at Calicut, they are salted down for the fleet, and eaten. Such cholera as they may have had is not described in history.

"where they dwell, they (the Nairs) have a pit or well digged, wherein they do hold water, which standeth openly by the way where every man passeth by. Therein every morning when they rise they do wash themselves all over, beginning first at the foot and rising up to the head, as well with men as women, without being ashamed to be seen of such as go by or look down upon them; and the king himself likewise; which water is so green, slimy, and stinking, that a man cannot but stop his nose that goeth by it; and they certainly believe that whensoever they should forget to wash themselves in the water, they should be wholly uncleane and full of sinne; and this washing and making cleane must not be done in any running water, but it must be in a place where the water standeth in a pit or well, and by their Bramines conjured by many wordes and ceremonies; otherwise they were of no virtue, but wholly unprofitable for their idolatrous services."[1]

"The Malabarese," says a well-known traveller, "worship the water of the Rameswara, near the pagode called Ramanakoil, to which they likewise attribute a peculiar virtue in cleansing them from their sins."[2] The beneficent idea of the legislator, however, seems to have lapsed in a series of minute and idle observances; it may be better discerned in that institution of caste which commits to the left hand the function of purification after nature's uses, while the right has the privilege of ministering food.[3] The drinking water, it must be observed, both in these and the opposite coasts, was of generally inferior and saline character, or, at least, of unequal quality in various parts. Those who traded by sea, as they were only able in the summer months, carried each his store of water under lock and key "con chiave in certe giare");[4] nor is it to be objected that these were Parsees. The painfulness of the journey was extreme, and the supply liable to run short; an equal difficulty was experienced with food.

In his botanical work,[5] Acosta says that he saw a good deal of cholera when in India, and that on his return he had written on the subject with a view to publication; his experience was probably not more than that of other Portuguese in the middle of the sixteenth century. "It is," he says, "frequent in India, this *morshy*, a frightful and virulent disease; the Arabs call it *hachaiza*, beyond all doubt a distinct contagion, *peste particular*."

[1] 'The Voyages of Jan Huygen van Linschoten,' English translation, London, 1598.

[2] 'A Description of the Coasts of Malabar,' &c., by Philip Baldæus, 1672, from the Dutch.

[3] 'Viaggio all' Indie Orientali del F. Vincenzo Maria di S. Caterina,' Roma, 1678.

[4] Idem.

[5] 'Tractado de las Drogas y Medicinas de las Indurs Orientales con sus Plantas,' &c., por Christoval Acosta, Burgos, 1578.

After the first brief and, as it may seem, uncertain portraiture of cholera, by the hand of Gaspar Correa, in the year 1503, we may now pass on to a fuller description of the disease in the pages of the same author, which occurs in the fourth volume of the 'Lendas.' It refers to an epidemic of cholera at Goa, forty years after, in the year 1543, and we may remark that it is here called a new disease. The volume has only recently issued from the press, and does not exist now in the public libraries of this country.[1] The wording of the original text is preserved, it is ancient but not obscure; no attempt has been made to give to the translation a Spenserian turn; a simple rendering of the text is all that has been aimed at.

Chapter XXIV.

On the great decease in Goa of many persons from a new disease called Mordexy and the difficulty which ensued of burying so great numbers as well as in administering the sacraments; at this time the Bishop made a fresh division of parishes—a circumstance which led to long disagreements.

"In the spring of this year there appeared in Goa a mortal throe which those of the country call moryxy,[2] common in all classes of persons, no less in the child at the breast than in the octogenarian; in the stalled beast and in domestic fowls also, for it was common to all things living, male and female alike. Nor could any cause be assigned for this agonising affliction. The sound as well as the sick fell victims to it, and nothing did it respect. This dolour struck in the stomach, produced by coldness according to some, but afterwards it seemed they only used the word on account of the quality of the pain. So grievous was the throe and of so bad a sort, that the very worst kind of poison seemed there to take effect, as proved by vomiting with drought of water accompanying it, as if the stomach was parched up, and cramps that fixed in the sinews of the joints and flat of the foot, with pain so extreme that the sufferer seemed at point of death, the eyes dimmed to sense, and the nails of the hands and feet black and arched. For this none of our physicians found a cure; the patients barely lived the day or at most the night through, in such sort that out of a hundred that fell sick of it scarce ten escaped, and those were mostly such as on recommendation of the country-born at the very onset made use of native medicines such as were of trifling efficacy (*de pouqua sustancia*). So great was the mortality at this period, that the bells tolled all day long—twelve, fifteen, twenty burials daily; at last, the governor ordered that the bells should be tolled no more, lest that people's hearts should die within them for very faintness. And seeing that the disease was so

[1] 'Lendas da India,' published by the order of the Royal Academy of Sciences at Lisbon, under direction of R. J. de Lima Felner, vol. iv.

[2] *x* in Portuguese is commonly pronounced *sh*. Móreshy, in the Mahratta language, means simply indigestion, and is employed exclusively in that sense at Goa at the present day. When they want to signify cholera, they employ the word "vait," from the Concani language, a dialect of the Mahratta, in use at Goa.

fierce, he gave command to the faculty that they should come together to open and examine some man who had died in the hospital of this complaint, morexy. In the whole of his body they found nought amiss, but only the stomach shrivelled and contracted like the gizzard of a fowl, or crumpled like leather in the fire; therefore, said the Masters, that the cause of this disease fell on the stomach, crumpling it up, and this was the manner of death. And thus it was from the pressing need in the burial of the dead, the clergy not being sufficient to the task, the bishop Dom Afonso Alboquerque marked out two more parishes: Holy Mary of the Rosary, and our Lady of Light, he named for parishes then, out of which proceeding of his there was debate more than enough, because certain of the clergy were unwilling to part with the tythes that accrued to them from their parishioners."

In such a school the Portuguese became familiar with cholera. Accordingly in the next generation of physicians we see those points pretty well decided which entertain us now — the necessity of dealing with the earliest symptoms, the significance in prognosis of the renal secretion, the influence of acclimatisation, the risk of infection from the clothes of the deceased, though in handling the cold clay there is none. The employment of full bleedings is discussed, and Galen's axiom contradicted (Galen, lib. i, ad Glauc., cap. 13) by a practical man.[1] The intelligent Bontius, of Java, in the beginning of the seventeenth century, seems scarcely to have arrived at a thorough knowledge of the complaint.[2] He sees the Governor-General of the Indies expire—"that great man, John Peter Coen"—and knows not that it is cholera;[3] he mistakes it for a chest affection. The Dutch physician is also amiss in saying that cholera always ends in a convulsion.

But the remedies of the country so decried by the chronicler, though he allows them to have been efficient in the early stage of the disorder, were they of so "unsubstantial" a sort as is asserted by Correa? To resolve the question, Garcia d'Orta and Acosta will be our guides. First, there was that yellow bark, macer, praised by Dioscorides and Galen, well-nigh of as high repute for long, long ages as cinchona is with us now. The best of this grew in the island of Santa Cruz, near Cochim, and in the coasts and country round about. Vast quantities were exported to all parts east, as far as China and Japan, and westward towards Europe. Now, however, it has fallen into neglect, and we shall not seek to assign to it a botanical place. The Brahmins uttered their surprise at the imbecility of the Portuguese, who, guided by a commercial instinct, gave their whole

[1] Abraham Nehemiæ Lusitani, 'Meth. Med.,' lib. i, ch. x, Venetis, 1604.
[2] 'Diseases, &c., of the East Indies,' from the Latin of Bontius, London, 1769.
[3] Observation xi, page 97, id.

attention to the spices, neglecting this sovereign drug. In brief course of time, however, it found its way to the mart of Lisbon. In India the colonists soon learnt to know it as the tree for the stools, "*arvore contra as camaras.*" Every kind of purging, without exception, was treated by this *macer*, and with marvellous good effect. The favorite way of administering it was fresh bruised with sour milk. "Some," says Acosta, "give of this dry bark half an ounce, which should stand through the night in four ounces of sour milk. Such a quantity shall be drank twice a day, morning and evening; immediately after, they give a spoonful of rice without salt, and free from fat, and fowls for diet or broth, and panada of the same; but," continues he, "if the case is more severe, they strengthen this with the Arab treatment, who use opium both on land and at sea for *every kind of purging* (the preparation with nutmeg); and for vomiting, or flaccidity of stomach, the same with mint-water, and also gum mastick in powders."

Nor need the reader be surprised at cold sour milk being given for purging. In Italy and hot climates acids are approved. The use of remedies of cold nature for purging derives authority from Paulus Ægineta, and has sometimes been carried to excess,[1] but in the main it has held its ground: thus, we see Bontius at Java, a century later, relying for the cure of cholera on the juice of the billinbing, a sort of gherkin, "the most acid fruit," he says, "the earth ever produced," and also the carambola subacid and astringent, as they used in Europe the unripe medlar. Besides the *macer* of the Brahmins, and the herb in Canarin language called *coru*, which "*staunched*," there were others of the same class, as the *avacari*, and for astringents the kino, pomegranate, and bael fruit; and also the betel, the centre of whose production was Malabar, the cobra or snake-root, and the sprigs and powder of the different myrobalani; and especially there was the moringa, approved by Acosta: all these were relied on for the treatment of morshi, nor were the aromatics denied, such as turmeric, and of the peppers especially "that sort called Canarin, which is hollow. The Brahmin and Canarin physicians use it," says Acosta, "against the collerica passio, called there morshi, and by the Arabs hachayza.

If it can be said of cholera that it belongs to one soil and region more than another, the city of Goa certainly offers many of those conditions of river, coast, and alluvial soil which are insisted on as favorable to its ravages. Indeed, any physician a stranger to India who perused a description of the western coast below the Ghauts would naturally entertain the idea that this region was unfavorable to health. Its superabundant humidity,

[1] On this subject see 'Amatus Lusitanus,' Curat ii, Cent. i, p. 58, et Scholia; and also Curat lvii, Cent. vi, p. 158.

exuberant vegetation, its position at the mountain foot, with the admixture of land and sea-water which is so fatal in encounter to animal life in minute organisms—add to this rice cultivation—these indisputably suggest ill-health. But, its humidity apart, which is extreme, there is nothing exceptional in Malabar, nor anything so remarkable as its long-standing intercourse with foreign countries. The institutes of Menu, which are of unknown antiquity, in their mention of an additional rate of interest for sea risks, prove that India was always a focus of commerce; and, as long as known, Malabar has teemed with mercantile life, has been fed with the idea of trade. The predominant feature of Indian disease, bowel complaint, is less rife on the west coast than on the eastern side of the peninsula and the hills, though here, as in the Persian Gulf, the supply of fish for food is very great. There are particular points of mountain fortress and sea-coast which cholera has not touched. If enervating, the climate is not so hot and stifling as in the Lower Carnatic and the Circars, and still less so than in Arabia, with its choking blasts, on whose shores the Portuguese are said to have died like dogs. At Ormuz, once the common mart for Europe, the hills are of salt and sulphur. There was no drinking water but imported, and that not good. Gomoro, which succeeded it, was in summer time a mere lazaret, with the worst of fevers and fluxes of blood and dysenteries (*intensissime dysenterie*).[1] The water was brackish and bad, and contained the dracunculus as well. At Aden and Socotora it was scarcely better. Muscat is provided with water, but for heat the Persians name it a hell. These and Jeddah were points of commerce to the Malabarese, and to the new comers as well, and they were points of conflict too. With the latter everything was rough in those days. The waterbuts of their sailing craft were filthy, and the holds pernicious to the sense. Describing St. Francis Xavier's voyage out, the historian says, "*Teterrimus odor sentinæ fœdorque et alluvies.*"[2] Ships are driven from their course, and the loss of life was surprisingly great on the score of disease. An expedition fails, a ship arrives from Portugal with all the crew sick, and having counted ninety deaths. The work of Purchas, so correct a compendium of early authorities, says "That in Malabar they had fluxes, fevers, and *vomitings*, in dangerous and to very many *in deadly manner*, as appeareth in Goa, where in the king's hospital, which is only for white men, there die 500 in the year."

Contemporaneously with Gaspar Correa in Goa during the first fifty years' occupation there lived a learned physician named Garcia d'Orta. He practised for thirty years in that

[1] F. Vincenzo Maria, loc. cit.
[2] Maffei, G. P., 'Historiarum Indicarum Colon.,' Agripp.. 1593.
[3] 'Purchas, his Pilgrimes,' "On Asia," vol. i, ch. 9, p. 617, the 5th book.

city, where he was physician to the viceroy, and permitted to call himself *fisico del rey*. An enthusiastic botanist, he maintained at his expense two gardens, one on an island near Bombay and one at Goa. He has left a valuable but now very scarce work on medicine and the botany of India, written in the form of dialogue or colloquy, which, in spite of the difficulties which beset the task, was printed in Goa in the year 1563, the solicitations of his friends alone causing its publication in the native tongue. The book has been chiefly prized for its notices on the botanical wealth of India. It constituted, in fact, a new æra in botanical science. Epitomes were made of this book by Carolus Clusius, Bontius, and others, which retain little of the raciness of the original. Acosta has plundered him remorselessly, but there is truth in what he says, that the work is a difficult study. Orta was well versed in Galenic and Arabian lore. He is bitter against the low class of doctors who follow in servile sort the native teaching; this teaching was empirical; the Arabian, or rather, as a result of intercourse, the Chinese, faculty of mind predominating. Yet he turns over cases of chronic diarrhœa to the native physicians, for they understand the coru better than he does; it is bitter, and they give it dry, but the rogues, he says, use opium too, and ofttimes nought but that. In the treatment of cholera it will be seen that he took that middle course which the experience of Sydenham recommends, and on which hitherto we have not improved.

The title of Orta's work is as follows:

"*The simples, drugs and medicines proper to India now set forth, and their nature explained, in form of conversation or dialogue, by Dr. Garcia d'Orta, Physician to our Lord the King; also certain fruits of the country are here brought under notice, and questions discussed touching thé art and practice of medicine and other good uses. The whole seen and approved by the Very Reverend the Licenciate Senhr. Alexos Dias Falcam, judge of the 'House of Supplication' inquisitory in these countries.*

"*With privilege of the Count Viceroy. Printed in Goa, x day of April*, 1563.

Colloquy XVII.

Conversation on the shrub *costo*[1] and on the *collerica passio*, the speakers are Ruano, Orta, the patient, a page and maid-servant, Don Geromino present by the side of the patient (the latter part of the colloquy only).

"*Maidservant loquitur.*—There is a lad here with a message.

"*Orta.*—Let him come in.

"*Page.*—My master Don Geronimo, sir, begs you will come at

[1] Costo. A root or stem much like white hellebore. In use with the Romans, and much sought for by the Chinese. Galen praises it for abdominal pains. Our herbal has its Costmary.

once to see his brother, although he knows it is not your hour for visiting; the case will not bear delay, and my master, sir, will be obliged to you.

"*Orta.*—Why, what ails the gentleman, has he been long unwell?

"*Page.*—It's morxi, he has been ill two hours.

"*Orta.*—I'm at your service.

"*Ruano.*—Is this the disease that cuts people off so suddenly, and from which so few recover? Tell me, what's its right name at home and here in India? What kind of thing is it—and what is good for the complaint?

"*Orta.*—It is called with us *cholerica passio*, and by the natives *morxi;* some of us have turned the name to mordexi, which is a corruption of their speech; the Arabs know it as hachaiza. In Rhazes you will find the same thing under the name of Saida. Far worse is it here than with us at home; for, very commonly, a man dies of it in twenty-four hours, or between that and four days, which is the outside; but as there is no rule, look you, without exception, I have seen one of extraordinary vigour of constitution who kept on for twenty days vomiting æruginous bile, and yet he died at the end; however, you had best go with me to see the patient, and then you'll better be able to judge of the symptoms.

"*Ruano.*—Thank you; I should like to go.

"*Orta.*—The pulse, you see, is mostly very low, as often as not it cannot be felt at all; the body is cold, with a cold sweat on the skin, and yet the patient feels hot and is clamorous; there is great thirst, the eyes very much sunken, no sleep. He throws up continually, and in both ways a great deal comes from him, until out of mere want of strength he can expel no more. Ah! here we are already, keep close to me. I'll show the way. God's blest salvation here. When did this complaint set in?

"*Patient loq.*—These two hours past. I was took all at once with a purging and a sickness. Nothing but water comes up, neither bitter nor sour to the taste.

"*Orta.*—Tell me, now, what have you eaten to-day?

"*Patient.*—Well, fish drest more ways than one, and I have had rice-milk and cucumbers. What I throw up tastes ever of cucumber.

"*Orta.*—Come, bustle here, we must have a stove to warm the room; put warm things to him, and let us have rough cloths to rub him with. Set to work briskly; and mind he does not drink any water, or as little as may be. If he will have it, best that which has had burning gold quenched in it, and we must touch his feet a little with the hot cautery. He will have a vomit, and also a glyster, which he will get straightway from the apothecary's; and some warm oils to rub him with on the back and spine, and his legs must be well rubbed too, mind you that. And when the vomit has acted and the glyster has come away in the stools, then see to let me know how he is going on, whether much or little has come away from him, if he is getting warmer or more free from cramps; and tell me if you can feel his pulse, or if it be any stronger, and then we shall be able to

judge what is fit to be done. For this disease, look you, Don Geronimo, is a serious affair, both doctor and attendants must mind their duty, everything at the right moment. Ah! the apothecary is here; now, sir, we must have forthwith, please you, a vomit of gruel flavoured with cumin and nicely sweetened, for I have found it serviceable. The glyster should be of barley gruel, too, to which add rose oil and rose honey, and strain. The oils you shall rub him with must be castor-oil and oil of rue,[1] because of the poison we have here to deal with. And now about dinner, let us say a boiled fowl and a fat one, but every bit of fat removed, and stuff it with some slices of quince, fresh if it may be had, but else preserved, taking care only to wash it with white wine, and add a little cinnamon, rose coral, or gold water—how say you? The doctor, my friend here, knows as well as I do, and perhaps better than any man breathing, where to lay his hand on the right thing, as a man of experience should, and especially in a land like this; and, in deference to him, I should think now a partridge of Ormuz would be the thing, or one of the country, or a good bush hen at a pinch; but if not to be had, I say try the fowl.

"*Ruano.*—Any way, you speak for both; and as for this, it is long we are agreed.

"*Orta.*—God's salvation first, and mind to come soon and let me know how you are getting on. (*Exeunt.*)

"*Ruano.*—Really this disease is frightful. I have seen many in pestilence, but never one so struck down of a sudden. What I have heard taught is this, some kinds of food are apt to turn bad and breed poison; such, if I misjudge not, are chiefly melons,[2] and cucumbers, gherkins, peaches, and apricots. You see he has got his disease fresh on him, so soon, too, after eating cucumber. Did you note how hard he draws his breath?

"*Orta.*—Ay, that's just the way of it. I attended a man of no little wealth and station here who lay thirty hours under this complaint; and as for flux from the bowels, no show of such thing, and no sign of vomiting either, no pains in the legs whatever, and this clean beats me, nothing but that want of breath. Think how bad a man must be when he cannot get his breath.

"*Ruano.*—What people do you find most subject to this complaint—and what time of year brings it on?

"*Orta.*—It seems most to fall on large feeders, and those who indulge in all kinds of food, as I had experience of in a young canon here who died from eating cucumbers, and the same also may be said of those who go too grossly with women. June and July are the months when it happens most; it is, as you know, the spring of the year in these parts. The Indians give it the name of morxi (indigestion), which tells you at once their idea that it comes from excess in eating.

"*Ruano.*—How do the natives use to treat it?

"*Orta.*—Their physicians make great account of rice-water, with

[1] Celsus.

[2] In another part, Orta attributes the disease to eating the jack-fruit.

pimento and cumin, which is called canje.[1] They also cauterize the feet just as I have done with that good gentleman, and they have a way of thrusting long pepper into the eyes to see how far sense is gone; for the cramps they put a tight band round the head and also circlets around the legs to the knees downwards, very tight, and thence continued to the feet;[2] and they give them betel and other things; not, indeed, without some spice of reason to back them, but all done in a rough kind of way.

"*Ruano.*—And how do you Portuguese treat it?

"*Orta.*—Well, we attend to diet, and let them have partridges or fowls with the broth of the same,[3] also toast bread sopped in wine with cinnamon spice. Though, indeed, these warm things I approve not much, my use of peppers is chiefly external. I rub the stomach with oleum dalmaticum and nardinum made warm by heat. The most I do besides is to cleanse the stomach with laxatives only, and by glysters according to the occasion.

"*Ruano.*—One must bear in mind the imbecility of nature so as to help her at a pinch, as she is often overpowered by the peccant humour.

"*Orta.*—See you, the poison generated in the humour, bad as it is, does not prove itself here to be of a catching sort; and that is a further reason for giving it prompt evacuation. And we just soothe the stomach with the cinnamon, and also externally with the dalmatic oil, and we do also put on cups by way of retention, but this is only when we may hope most or nearly all the bad humour to have parted from the body.

"*Ruano.*—Will you tell me, now, if in your practice you have hit upon anything very serviceable for this disease? Have you any notion of your own?

"*Orta.*—Well, I must say yes. I have a great opinion of treacle, either by itself or with wine or mixed with water, or cinnamon water, according to choice or convenience; and I have tried the cobra-wood with advantage, and our preparation of unicorn is not amiss, and also the contrayerva of Malacca the same as we use in wounds from poisoned arrows; but the medicine I have most trust in is the bezoar[4] (pazar, as the Persians call it). I shall speak of it by-and-by, that is really a sovereign thing, its restoring effect upon the heart is truly something wonderful. I have had patients who, after taking a little of the stone in their drink, told me quite of themselves, without knowing a bit what I had given them, that no sooner had they drunk it down than they seemed quite set up; their soul seemed to come back to them again, they said, and the Bishop of Malacca did famously well in my hands by the use of the bezoar, and afterwards the treacle. Stay till most of the humour is evacuated; you can then give the treacle, by way of glyster, in increasing quantities.

[1] Canja, in Goa language; conjee water.

[2] For purging, Alexander of Tralles advises ligatures to the arms and hands; for vomiting, to the feet and belly.

[3] This treatment agrees with that of Sydenham. A great faith in poultry was derived from Avicenna.

[4] Theriaca, on which many books have been written, Venice treacle, much used in the great London plague.

"*Ruano.*—It is the first time, as far as I know, that I ever knew of treacle being used in glysters.

"*Orta.*—I see nothing out of the way in such practice, above all—in dealing with a virulent disease. It served me well enough in treating a head civilian here, a magistrate in the city under royal appointment. He was ill to death after a dish of prawns he had indulged in. I held firm to my remedy, though my colleagues set themselves against it; but after seeing how well it succeeded in his case, they were much delighted, and have used it in their practice since.

"*Ruano.*—Have you met with other diseases in India which wring life out of a body like this—and will you be so kind as to tell me, too, what external remedies you most rely on in this disorder?

"*Orta.*—A good many die here from sheer prostration,[1] either after eating poisoned prawns or from excess with women, and the native physicians call this the dry mordexi. As to your other question, I use oil of nard, and dalmatic oil, and I take care that what they eat is well warmed; by fire, I mean, not by spice, and I always forbid yolks of eggs, for they turn and corrupt digestion; and I have spoken already about the bezoar. As to costo, what I have to say is this: when Mateollo Senenfis alleges from authority that angelica root is of the same species as costo (not that, as he says, he either uses it or approves of it), and when he says that those who use it in the place of costo act more agreeable to reason than those that use menta romana—all I have to say on the subject is that, angelica root is not costo; for all that I know, angelica may be a better thing."

Art. V.

On the Diagnosis and Treatment of Stricture of the Urethra in its Early Stage. By W. F. Teevan, B.A., F.R.C.S., Surgeon to the West London Hospital, &c.

It may be laid down as a general rule, that any stricture of the urethra (traumatic excepted) which requires to be split, cut, or ruptured, implies that the patient neglected his case, or that the surgeon failed to detect the stricture in its early stage. Of all diseases afflicting the human frame, none gives such timely warning to the surgeon as stricture of the urethra. Its symptoms are so well marked, and its progress so slow, that every surgeon ought to be, and is, able to cope early and successfully with the enemy. How therefore is it that severe strictures of the urethra are so common amongst all classes of the community? Are they due to the patient's neglect? Rarely. When a man in any station of life experiences any abnormal sensation in his genito-urinary tract, he usually

[1] For a death by cholera sicca in the Persian Gulf on board a native craft, see Lumsden's 'Overland Journey from India,' London, 1822, p. 68.

applies at once to a surgeon for relief. Hence, at every hospital a large mass of the out-patients is made up of men who come complaining of a gleety discharge, pain in the urethra or perineum, increased desire to micturate, or some other abnormal sensation. If these patients do not get relief, they make their appearance at another hospital; and it will thus be found that amongst the out-patients at every hospital there is a large number who are *en route* from hospital to hospital, trying to get cured. My own experience teaches me that no patients are so punctual and constant in their attendance as those suffering from genito-urinary diseases. It may therefore be conceded that severe strictures of the urethra are seldom due to the patient's neglect. To what, therefore, must we attribute their existence? To the fact that the existence of a stricture of the urethra is rarely diagnosed in its early stage—that patients are treated for gleet, or irritable bladder, when in reality these evils are the consequences of a much more serious condition of things. Inasmuch, therefore, as there is a time when stricture of the urethra can be diagnosed when causing scarcely any inconvenience, and speedily cured without pain or danger to the patient, I will proceed to inquire into what are the earliest symptoms of stricture of the urethra, and the best means to detect its existence at a time when it cannot be ascertained by the instruments usually employed in England. I must here qualify the expression "cured." Pathologically, there is no evidence to show that a stricture is ever cured: practically, all strictures are curable. Surgeons have not as yet taken any means to ascertain whether the condition of things which remains after a stricture is said to be cured is ever removed. I have invariably found in all those cases of reputed cured strictures, when I examined them with the bougie à boule or endoscope, that the pathological state was persistent in greater or less degree. Hence it is that all strictures said to be cured relapse, unless dilatation be occasionally employed, even to an almost indefinite period.

A very erroneous opinion prevalent is the professional idea of stricture, which is one of degree, and not of kind. Regard is had, not so much to the pathological condition, which is the primary evil, but to the constriction, which is quite secondary to the other, and resulting from the primary. *It ought to be the surgeon's object to attack the primary condition when the secondary result is scarcely appreciable.* By detecting a stone in the bladder at its earliest stage we are enabled to substitute for lithotomy a much less serious operation; and in like manner, if we ascertain the existence of a stricture in its early stage, we can treat the patient by a procedure altogether harmless. Until such time, however, as the profession turn their attention

to the detection of stricture in its earliest formation, such a result cannot be achieved. It may without any exaggeration be said that nearly all English surgeons would consider that there could be no stricture if a No. 10 catheter could be passed into the bladder with ease at the first visit. I will, however, show that such an opinion is entirely fallacious. A full-sized instrument may pass with facility, and there may yet be several strictures. Every long inflammation of the urethra is attended with the deposit of lymph in and underneath the mucous membrane. When the inflammation has worn out, or is cured, that is seen to happen in the urethra which happens everywhere else—the part begins to waste. The wasting is attended with contraction. Hence stricture of the urethra is *atrophy, not hypertrophy* (Dr. Henry Dick). This is entirely opposed to the current pathological views of stricture, but I feel sure that it is the correct one. Hypertrophy first—atrophy afterwards.

Now, what are the earliest symptoms of stricture of the urethra—in that stage which is only attended with very slight contraction? The increased desire to micturate, or difficulty in making water, is the common usual symptom of stricture. But long before that stage is reached, other symptoms have been present, which should not be overlooked. It is believed that stricture of the urethra is not usually attended with gleet. Gleet, however, is present in the early stage of every stricture. As the stricture gets worse the gleet gets better, for the simple reason that the wasting of tissue is continuing. Hence whenever a patient with gleet goes to a surgeon, the urethra ought to be explored. I have detected a stricture when the patient's first gleet has only been of six months' duration. Constriction of the calibre of the urethra takes place very much earlier than is supposed, and therefore an examination ought never to be omitted. Gleet, irritability of bladder, incontinence of urine, pain in some part of the genito-urinary tract, and various abnormal sensations, are the usually earliest symptoms of stricture. It often happens that the patient complains of nothing beyond some abnormal sensation: his teeth chatter, or his legs tremble when he is making water. At other times he complains of coldness in the scrotum, or a feeling of cold water running down his back. The chattering of the teeth and the trembling of the legs are usual concomitants of severe strictures, but they also often are present at a stage when there is no difficulty in making water. In the early formation of stricture, loss or impairment of the sexual power is very rare, whilst in the advanced stage it is very common.

In some cases the early symptoms of stricture are obscure, fugitive, or intermittent, but in all instances the patient seeks the surgeon's help for something wrong in his genito-urinary

tract; hence it is well always whenever a patient complains of anything abnormal which is not obvious to the eye to examine his urethra. In cases of stone in the bladder, such examination is usually omitted with disadvantage. The state of every part of the genito-urinary tract ought to be known to the surgeon.

By what means are we to ascertain the actual existence of a stricture? There are only two instruments which can effect the diagnosis, and these instruments are almost practically unknown in this country—the bougie à boule, and the endoscope. Surgeons are under the impression that the ordinary instruments can diagnose the existence of stricture. Stricture can be so ascertained but only in its advanced stage. It ought to be the surgeon's care to prevent the constriction from ever passing into that stage. It is thought that if a No. 10 bougie or catheter be passed, and no obstruction met with, that there can be no stricture. A No. 10 may pass with the greatest ease, and there may yet be stricture. Sensation in the ordinary instrument is distributed equally in all directions all over it; and hence, when the instrument experiences either very slight or no resistance, nothing appreciable is felt by the surgeon. In the bougie à boule, however, the sensation of resistance is all concentrated on one spot, and hence the sensation transmitted is extremely delicate and accurate. Thus, therefore, the bougie à boule will speak, as it were, when the ordinary instrument remains dumb.

But many would say that if a No. 10 size instrument can be passed, there can be no stricture. The contraction which takes place is very gradual and slow; and if the calibre of the natural urethra be looked upon as admitting No. 12, that patient's urethra which only admits No. 10 has already undergone two degrees of contraction, and if the natural size of a given urethra be No. 14 and only admit No. 10, there must then be reckoned four degrees of contraction. The earliest contraction would be represented by the diameter of a ring the size of the natural urethra when distended to its utmost. It ought always to be remembered that contraction begins at the highest number of the catheter gauge, and it ought to be our object to diagnose its existence when it can only reckon on one or two degrees.

But it will be said, how is a stricture to be recognised when its existence is so recent? The bougie à boule will unvaryingly detect the presence of a stricture long before it can be ascertained by any other instrument in use. The bougie à boule is nothing more than an elastic catheter or bougie of the size of 3 or 4, with the end surmounted by a spear-shaped head the size of 10 or larger. The value of the bougie à boule consists in its having a sharp shoulder, inasmuch as it is the shoulder, on the withdrawal of the instrument, which hitches against the stricture. Therefore, those bougies à boule which have their

shoulders levelled off to the stems are useless as diagnostic instruments. The ordinary instrument detects a stricture during the act of entry into the bladder, whilst the bougie à boule does it *in the act* of *withdrawal* from the bladder.

The "ball staff" was first invented by that distinguished man Sir Charles Bell. It was not appreciated in this country, but Leroy D'Etiolles seized upon the idea, modified the instrument by making it of elastic material instead of metal, and thus this instrument, invented by an Englishman, will, in all probability, be known in England by its French name. About ten years ago Dr. Henry Dickfurther increased the usefulness of the instrument by getting Charrière to make its shoulder very sharp, and to that gentleman belongs the merit of having been the first in this country to advocate its use.

About four years ago I accidentally became acquainted with the bougie à boule in Paris, at Lasserre's, whilst seeking for an elastic catheter which would remain in the bladder at all times without any tying. From Lasserre's I brought over many bougies à boule, and after an extensive trial I introduced them to the notice of several medical societies. Every surgeon who has seen me use the instrument has been so convinced of its very great usefulness that he has at once desired me to get one for himself.

It must be remembered that the bougie à boule is not an instrument for the treatment of stricture—it is for the diagnosis.

Some bougies à boule are made pervious like a catheter, and they all of them possess stylets, so that they have sufficient firmness to pass a constricted spot. The most useful ones are those whose heads vary in size from nine to fourteen.

Now, the following is what may be called a typical case: A patient comes to a surgeon complaining of gleet, irritable bladder, or experiencing some abnormal sensation or pain in the genito-urinary tract. A No. 10 elastic catheter will in all probability glide into the bladder with ease, and the patient will not suffer much pain at any particular spot. If the catheter be withdrawn and a bougie à boule with a head the same size as the catheter be introduced, it will be found that there is a difficulty in passing certain spots, and that the patient experiences pain when the instrument is going over certain places. On withdrawal, the bougie will hitch against certain spots, and it will require considerable force in some cases to disentangle the instrument. The sensation communicated by the bougie when passing through constricted spots is so entirely peculiar and different from what is experienced when the instrument is travelling along a healthy urethra, or along healthy parts of a diseased urethra, that no possible mistake can arise to any one who has had even a very limited experience of the instrument.

It may possibly be said that the above sensation is due to spasm of the urethra. This is not so. In spasm all parts of the bougie are equally grasped by an uniformly spasmodic urethra, whereas the constriction detected by the bougie is entirely different, and the *bougie is found to move quite freely either in front of or behind the constricted spot*. Then, again, the same constriction is found to be present at any and every examination, and the spot does not shift.

There are several sources of error to be avoided by a beginner. Firstly, the condition of the prostate may be such as to give rise to resistance; secondly, the feeling experienced in passing through the triangular ligament is to be distinguished; and, lastly, the narrowed condition of the meatus externus which is quite normal to some urethræ must not be mistaken for stricture of the orifice. When, however, it is considered that nearly all strictures are in front of the triangular ligament, there can rarely arise a case of mistake. When the bougie is used on the healthy urethra, the instrument is found to travel along a soft velvety surface, and as it is withdrawn through the triangular ligament, it hops as it were over that part which is immediately beneath the floor of the urethra. The feeling communicated to the bougie as it passes through the prostatic urethra is one of solidity and straightness, and differs considerably from the wavy flexure described in the penile urethra.

Whenever a patient complains to a surgeon of anything wrong in his genito-urinary tract, the urethra ought at once to be explored with a No. 9 bougie à boule; and if nothing be detected, another bougie, the largest the meatus will admit, must be passed. In this way no stricture can escape detection whilst yet young.

In examining for suspected stone the sound is first introduced, but the bougie à boule ought to be the first instrument used, as it will give the surgeon information nothing else can, and in some cases will at once determine whether a stone ought to be cut out or crushed. Now, if it be ascertained that the patient's gleet, irritability of bladder, or some other pathological symptom, be due to the existence of a stricture, what treatment is the best? Without doubt, gradual dilatation carried on by means of the olivary bougie. The instrument used in England is no more to be compared to the olivary bougie than is the old-fashioned tub to the modern clipper. The olivary bougie passes through the urethra with infinitely less resistance than the ordinary bougie. I have seen students unable to pass the bougie in a perfectly healthy urethra, and then by substituting the olivary bougie they have accomplished the end with the greatest ease to themselves. As regards the patient's feeling, the olivary bougie is infinitely superior. The olivary bougie

never causes pain, or, at all events, such a very modified degree of pain as scarcely to be appreciable, whereas the ordinary instrument can be rarely passed without causing considerable pain as it is getting into the bladder. The reason why the olivary bougie causes so very much less pain than the ordinary instrument is simply because it does not evoke so much resistance as the other. The little oval ball at the end of the olivary bougie acts as an "avant courier" for the rest of the instrument; and as its diameter is less than that of the stem, it opens up the urethra for the passage of the body of the instrument.

The healthy urethra is in a state of collapse, like an empty vein, and hence it is no longer a tube, as its surfaces are in approximation with each other much in the same way as two leaves of a wet book. The urethra is only tubular when urine is passing through it. It must, therefore, be borne in mind that passing an instrument into the bladder is not so much the passage along a canal, but a separation of surfaces, and the insinuation of something between the approximated surfaces in order to allow something else to follow. Therefore it is clear that the oval ball of the olivary bougie which represents the end of a wedge-shaped instrument is infinitely better for the accomplishment of the end in view than the blunt extremity of the instrument which is uniform in diameter.

The transition in size from one English catheter to another is much too abrupt for usefulness. Between every English size an intermediate one is required. Hence the French gauge is more useful than the English.

Dilatation ought to be carried up to the largest size the urethra will admit, and if the meatus externus retards the introduction, it ought to be enlarged. In many cases where the stricture is what is called elastic, the leaden bougie will be found more useful. At first a bougie ought to be passed every second day, and left in for ten minutes; as the case proceeds, the introduction ought to be less and less frequent, and when the cure is supposed to be completed a full-sized instrument ought to be passed, either by the surgeon or the patient, *once every six months for the remainder of the patient's life.* Unless this be done, there is no safety against a relapse.

The endoscope is very useful for diagnosing the early stage of stricture, and will often give information as to the exact pathological condition of the mucous membrane of the urethra, which the bougie à boule cannot. But if there are several strictures in a given urethra, the endoscope will usually only detect the one nearest the orifice. Then again, for ordinary hospital practice, the bougie à boule is the preferable instrument. An exploration of the urethra by the bougie à boule can be effected in a minute or so, whereas an endoscopic examination cannot be

conducted under one quarter of an hour. The endoscope, consequently, is chiefly of use in exceptional cases of stricture for obtaining knowledge which cannot be afforded by the bougie à boule—such as the appearances of the mucous membrane, whether it is ulcerated or not. For severe strictures, in which it is very difficult to pass an instrument, the aid of the endoscope is invaluable in pointing out the entrance into the strictured part.

In the early stage of stricture the urethroscope shows that the mucous membrane is considerably deeper in colour than natural at the strictured part; the injection of the stream of urine against the narrowed spot keeping up a permanent congestion of the part. On withdrawing the urethroscopic tube the mucous membrane remains stationary, and thus the appearance is altogether different to what takes place in the healthy parts of the tube, where the mucous membrane falls and closes in on the end of the tube as it is being withdrawn.

Subjoined are a few cases, out of a great many, illustrating the value of the bougie à boule.

Case 1.—C. K—, a robust-looking postman, æt. 30, came to me (November 3) for a continuous pain in his perineum. He described it as a "burning pain." Gleety discharge of many years' duration, knees trembled when making water, but had no difficulty in the act. Cannot get married on account of his gleet. Passed No. 9 bougie à boule and found two strictures; one in the penile urethra, and the other at the bulb. The instrument caused him much pain when being withdrawn through the stricture. No strictures could be detected when a No. 9 ordinary bougie was passed.

This patient was treated by dilatation twice a week, the largest instrument the urethra would admit being passed. On December 9 he was quite free from the gleet and pain, and got married the ensuing month. Comes to me regularly every two months to have an instrument passed.

Case 2.—J. W—, a pale-looking commercial traveller, æt. 26, put himself under my care (April 22) for a gleet of nine months' duration. Passed No. 9 bougie à boule, and found a slight constriction at bulb. He was treated by gradual dilatation once a week, and on May 24 was quite well.

Case 3.—J. M—, a dissipated-looking fishmonger, æt. 26, came under my care, February 23. Complained that he trembled when making water, and suffered from occasional pain in the perineum, but did not experience any difficulty in urinating, neither had he any increased desire to micturate. Had a gleet of four years' duration on him. I examined him with a No. 10 bougie à boule and found three strictures. Was treated twice a week by gradual dilatation, and on May 29 was quite well.

Case 4.--H. H—, a pale-looking carpenter, æt. 28, came to me on December 1 for difficulty in making water. Seven years ago caught a gonorrhœa, which degenerated into a gleet, but has been quite free of it for the last year. Sweats and shakes at knees when urinating. Desires to make water every hour. I passed a No. 9 bougie à boule, found one stricture at bulb and another in penile urethra. Treated by the introduction of the olivary bougie once a week. Was quite well on May 1.

Case 5.—W. N—, a healthy-looking printer, æt. 22, put himself under my care on December 5 for a gleet. Caught his first gonorrhœa nine months ago, which degenerated into a gleet, and remained on him ever since. Passed No. 11 bougie à boule and found a slight constriction in front of bulb. He was treated by gradual dilatation, but not beiug very regular in attendance was not cured till May 29.

Case 6.—J. C—, a pale thin-looking man, æt. 26, lately in the marines, applied to me (March 27) for "irritability of bladder." Last October was discharged from army for inability to hold his water. Three years ago contracted his first and only gonorrhœa, which degenerated into a gleet and remained on him ever since. Has been seventeen weeks in a military hospital, and an out-patient at two of the metropolitan hospitals. Cannot hold water for a longer period than two hours, and if he takes beer wants to urinate every ten minutes. I examined him with a No. 9 bougie à boule, and found three strictures. He suffered much pain on withdrawal of instrument. I treated him by passing the olivary bougie twice a week, and by May 22 he was quite well. He is now a stoker at the gas works.

All the above cases were, during their treatment, submitted by me to many medical gentlemen for examination.

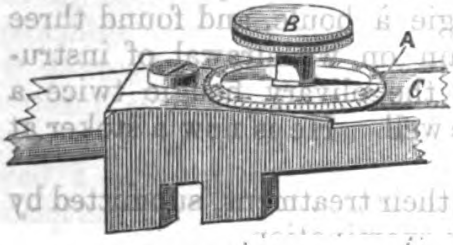
Art. VI.

Note on the Regulation of the Pressure on the Artery in the Application of the Sphygmograph. By Balthazar W. Foster, M.D., M.R.C.P.L., Physician to the Queen's Hospital, Birmingham.

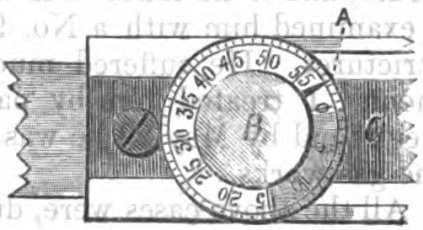
The sphygmograph of Marey has already won a high position as an instrument of physiological and clinical research, but it nevertheless possesses imperfections, especially in the means afforded for regulating the pressure upon the artery under examination. All who have worked with the instrument with any constancy must have regretted the inefficiency of the pressure screw ("*vis de réglage*," Marey), but as yet no satisfactory

alteration has been suggested. In using the instrument as hitherto sold, it is often necessary after its application to vary the amount of pressure exercised upon the vessel by means of the pressure screw, in order to obtain a fair average trace of the pulse movements. When the sphygmograph is applied to the forearm, this screw certainly enables us to increase the pressure by causing the descent of the spring which rests upon the artery, but it affords us no information concerning the amount of extra weight thus bearing upon the vessel, and it gives us no aid in ensuring the exercise of the same pressure in any two unconnected observations. A practised observer can no doubt soon regulate the instrument so as to obtain the most perfect trace, but the unskilful hand finds no small difficulty in so doing; and owing to the great modifications in the trace which may be produced by varying degrees of pressure, often obtains imperfect or erroneous results. And as the screw has to be readapted in nearly every case, no amount of practice can enable the experimenter in making a second observation after any interval, to exercise with certainty, exactly the same amount of pressure, unless the screw has meanwhile been carefully retained in its position, a condition incompatible with frequent use.

In consequence of these difficulties, I suggested some months' back[1] an addition to the screw, which I have since had applied and used with advantage.



A, Index. *B*, Pressure-screw. *C*, The spring.



Screw seen from above.

The woodcuts represent two views of the pressure screw (B), as placed in Marey's sphygmograph, and show also the additions I have made. These consist in connecting an index (A) with the screw, around which a circle has been described and graduated. The index moves along with the screw, and indicates on the circle the pressure equivalent in any observation.

By this simple arrangement, without any alteration in its form, the sphygmograph may be made much more reliable in the hands of beginners, for each one will be able to record at the side of the pulse trace, the pressure exercised upon the artery beyond that necessitated by the application of the instrument.

[1] Physiological section, British Association Meeting, Nottingham, 1866.

Moreover, in making a series of observations[1] in the same or even in different cases, the spring can be fixed at that point which allows it to follow most accurately the movements of the vessel, and the conditions in all being thus rendered practically the same, the conclusions based upon a comparison of the traces become much more trustworthy. In cases of aortic aneurism, there can be no doubt that the most valuable assistance in diagnosis may be occasionally obtained from a careful comparison of the pulse traces collected on opposite sides of the body; and as important conclusions may be drawn from very slight deviations in form, we can scarcely overrate the value of any addition to the sphygmograph which will increase the accuracy of the conditions of application in the two observations. The modification in the screw will also aid in the determination of the effects of increased pressure in influencing certain parts of the pulse trace. Marey has, for instance, pointed out that the arterial tension may be estimated in this way, and, I believe, we may also obtain very useful information concerning the force of the ventricular systole.

In conclusion, I may observe that while ordinarily the use of the pressure screw brings an unusually large extent of the spring in contact with the vessel, this may be avoided in the case of the radial, by a little attention to the relations of the artery (Wolf, op. cit.) An increased prominence of the ivory pad which rests upon the vessel, or a slight alteration in the curve of the spring, would, however, obviate this difficulty. I am aware that the slight addition suggested in this note by no means dissipates all the difficulties surrounding this question of pressure, but it will, I hope, be found useful in increasing the accuracy of the application of the sphygmograph. I have now used this plan for twelve months, and if its description leads to the proposition of any more perfect arrangement, it will certainly not have been devised in vain.

[1] Mr. Meyer, of Great Portland Street, made the above-described alterations for me, and calculated the equivalents of pressure to be as follows:

When the index points to 5, the extra pressure equals 20 grammes in weight.

,,	7	,,	31	,,
,,	10	,,	40	,,
,,	12	,,	52	,,
,,	15	,,	64	,,
,,	18	,,	79	,,
,,	20	,,	93	,,
,,	25	,,	123	,,
,,	30	,,	160	,,

A scale increasing more gradually would no doubt be preferable. This could be easily arranged, as every instrument would have to be graduated according to the elasticity of its spring. I have remarked in several sphygmographs which I have examined a wide difference in this respect. Wolff also particularly mentions this in his 'Charakteristik des Arterien Pulses.' Leipzig, 1865.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

HALF-YEARLY CHRONICLE OF PHYSIOLOGY.

By HENRY POWER, F.R.C.S., M.B. Lond.,
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1. Dr. O. FRÄNTZEL.—*Essay on the Structure of the Spinal and Sympathetic Ganglion-cells.* ('Virchow's Archiv,' 1867, Bd. xxxviii, p. 549.)
2. F. BIDDER.—*Observations on Cases in which Conduction occurred in both directions in the Lingual Nerve, after this had been divided and united with the Hypoglossal.* ('Reichert's Archiv,' 1865, p. 246.)
3. CARL HEINEMAN.—*On the Influence of Strychnine Poisoning on the Movements of the Frog's Heart.* ('Virchow's Archiv,' xxxiii, p. 394.)
4. F. BIDDER.—*On the Consequences of Section of the Roots of the Spinal Nerves in a Frog.* ('Reichert und Dubois Raymond's Archiv,' 1865, p. 67.)
5. Dr. SCZELKOW (Charkow).—*On the Presence of Volatile Fatty Acids in Muscles, and on the Changes these undergo in Tetanus.* ('Archiv f. Anat. u. Physiologie,' 1864, p. 672.)

1. Dr. FRÄNTZEL states that, having been much engaged in studying the pathology of the nervous system, he thought it requisite to investigate and corroborate or otherwise the observations of Drs. Beale and Arnold upon the structure of the ganglion nerve-cells. His preparations were made chiefly from recently killed guinea-pigs and rabbits, but in a few instances, also, from human embryos. He found the form of the cells very various, round, oval, polygonal, club-shaped, or beautifully pear-shaped. They easily changed their form on pressure, and as readily regained it. Every cell consisted of a mass of protoplasm not invested by any cell-membrane, but presenting a coarsely granular appearance, and a double contoured round nucleus with nucleolus. The cells of the spinal ganglia lie in a special sheath of connective tissue formed by an expansion of the neurilemma of the nerve-fibre connected with each, which frequently presents a striated aspect, and contains several elliptical nuclei, and

when many are gathered together they are enclosed in a kind of capsule, which is in close connection with the perineurium.

These *capsules* he considers as being without doubt the structures which were formerly regarded as the *cell-membrane* of the ganglion-cells. As regards the relation between the nerve-fibres and the cells, he has constantly observed straight nerve-fibres enter the spinal ganglion-cells, and was able occasionally to follow them as far as, but no farther than, the nucleus. These nerve-fibres correspond to the dark-bordered fibres described by Arnold. He was never able to convince himself that more than one such fibre entered a single cell. He does not doubt the existence of spiral fibres, which he has seen very beautifully in the spinal ganglia of human embryos, but he has been unable to discover any proof of their nervous nature since, on the one hand, he has never been able to trace their passage into double contoured nerve-fibres, nor, on the other, has he been able to trace them beyond the outer border of the cell, nor into any such thread-like network as has been described by Beale, Arnold, and Courvoisier. After trying various modes of preparation, he adopted that recommended by Deiters (bichromate of potash and chromic acid). A network then appeared on the outer surface of the cells, but he was soon convinced that the network was not firmly connected with the superficies of the cells, but rather cleaved to the capsule when the cells contracted, in consequence of the action of the acid. From these observations it became clear that the network did not belong, properly speaking, to the cells; and further research showed that the irregular polygonal spaces bounded by the lines forming the network contained in all instances large, round, granular nuclei, by which they were nearly filled up. He frequently thought that he could trace a thread of communication between the fibres of this network and the nucleoli of the ganglion-cells, but more exact observation as constantly showed that they coursed either over or under the cell. Hence he was brought to the conviction that these lines were in reality the boundaries of flat cells; and, subsequently, he was enabled to detach and isolate them. The result of his observations, therefore, is to show that the capsule of the spinal ganglion-cells is lined by a layer of irregular polygonal epithelial cells with large nuclei. Frantzel next turned his attention to the structure of these sympathetic ganglion-cells, and after much investigation arrived at precisely the same conclusion respecting them as that just given in regard to the spinal ganglion-cells.

2. In Bidder's experiments, dogs of from two to four months old were used. These were narcotized by injection of from 60 to 100 drops of Tr. Opii into the jugular vein. Only the left side was experimented on. Both nerves were divided; and *both*, on section, made the animals feel lively pain. The cerebral end of the lingualis was connected with the peripheric of the hypoglossal in two cases, and the central extremity of the hypoglossal with the peripheric of the lingual in four cases. The ends were retained in position by silk threads passed through the neurilemma. After union of the two

ends, the remaining stumps were extirpated to the extent of eight to ten lines, and the wound sutured.

Immediately after the operation, the lingual muscles of the left side were completely paralysed, and the vibratory movement noticed by Schiff was observed. It never occurred, however, before the eighth day, and reached its highest intensity at the twelfth day (third and eighth days, Schiff). Bidder attributes it to progressive fatty degeneration of the peripheric extremity of the hypoglossal (and not, as Schiff did, to cell and nuclear growth around the cut extremities), and has satisfied himself of the fact by direct observation.

The distal extremity of the hypoglossal preserves, notwithstanding the progressive fatty degeneration, its power of exciting the muscles to action for many weeks.

The vibration cannot depend on changes in the muscular tissue, since this remains quite unaltered. Moreover, after the section there was loss of gustatory and tactile (pain) sensations on the left side, cuts and pinches not being felt. Hence, also, the animal often caught this side of the tongue between the teeth. After two months, the whole tongue seemed somewhat atrophied, and the fungiform papillæ of the left side had disappeared. This was due to the absence of the trophic influence of the lingual, and not to the division of the hypoglossal nerve.

The experiments were usually commenced on the animals three months after the operation. A fortunate union, even with every care, only occasionally takes place. There is an extraordinary tendency on the part of the hypoglossal ends to come together again, even after nearly half an inch of the nerve has been extirpated. Further, it must be borne in mind that though the impulses of the will may be no longer conveyed by the nerve to the muscles, it may retain the capability of being stimulated by other excitants, as electricity—perhaps because the latter are much stronger.

The cut ends of the lingualis were not equally inclined to union, for the tongue remained insensible to pain for three months, especially in those cases where the centric end of the lingualis was united with the peripheric of the hypoglossal; whereas, when the centric end of the hypoglossal was united with the peripheric of the lingual, the animal, at the end of three months, or before, shrieked on being pricked with a needle; yet this cannot be absolutely referred to a union of the motor and sensory nerves, since the hypoglossal, it will be remembered, also contains sensory fibres: but it does show that centripetally coursing fibres in different *nerve-paths may* unite under appropriate conditions, and that sensations can thus be conducted to the cerebrum.

One of the best series of the experiments was made on animals in which the *central lingual end* was united to the *peripheric hypoglossal end*. They cut down on the lingualis from a new point, namely, the interior of the mouth, thus avoiding the old cicatrix and wound. The animal was narcotized as before; its mouth opened, and the nerve sought for at the anterior border of the pterygoideus internus. A glass plate was inserted beneath it, and induction currents applied. On each occa-

sion of the current being broken, there was a convulsion of the whole body with loud groans, clearly showing the centripetal conduction of the lingual; also the whole tongue was convulsed. But it now remained doubtful how much of this last was reflex through the sound hypoglossal. The lingual was, therefore, divided, in order to exclude all reflex movements. At the moment of section, the animal sighed deeply, and the tongue on the same side was evidently convulsed; and this occurred repeatedly on isolating the distal extremity and opening the current probably through the styloglossi and genioglossi. The same occurred on pinching the extremity.

The animal was now quickly killed by blowing air into the veins; but the cicatrix was so tough from several weeks' suppuration having occurred, that it was found impossible to discover the point of union of the two nerves. All four ends were cicatrized up in this mass.

Microscopical examination of the trunks of the two nerves, above and below the point of section, showed healthy nerve-structure in the upper portions (centric) of both nerves. But the peripheric extremity of the lingual had lost its white colour, was pale and transparent, and consisted only of nerve-sheaths, and was destitute of white substance, containing only fat-globules.

In the hypoglossal nerve some fibres were degenerated—others were normal. These last had, without doubt, united with the lingual fibres in the cicatrix. The results of this experiment cannot be considered as perfectly satisfactory, on account of the failure in the anatomical part of the proof.

In a second experiment, in a dog two months old, union of the outer wound was complete in ten days. After three months no trace of sensation or motion was present in the left side of the tongue. On performing the same operation and the same experiments as in the former case, the same results were observed. Here, however, the anatomical examination was more satisfactory; the centric extremity of the lingualis was continuous with the hypoglossal, through a bridge of reddish colour, traversed by a whitish cord; both nerves, but chiefly the lingualis, were thickened near the point of union. The central end of the hypoglossal and the peripheric extremity of the lingual were distant half an inch, and were swollen at their extremities. The peripheric end of the lingual was quite pale, but that of the hypoglossal was of normal colour and appearance. Microscopic examination of the above-mentioned bridge showed, beside connective tissue fibres, numerous nerve-fibres, not distinguishable from healthy ones. In this case they state that the peripheric end of the lingualis consisted exclusively of degenerated fibres (fatty), but the peripheric of the hypoglossal contained only a few—the greater part being healthy. Hence it seems certain that impulses proceeding from the centre had passed during life along the central part of the lingualis and the peripheric extremity of the hypoglossal, preventing them from decay.

3. The knowledge at present possessed on the influence of strychnine-poisoning on the movements of the frog's heart is to this effect:—

1st. During tetanus the rhythm of the movement is but little altered. 2nd. In poisoning with large doses, the activity of the heart becomes gradually greatly diminished. 3rd. Local application of strychnia to the heart quickly causes cessation of its movements. Heineman's experiments were undertaken in frogs, and he carefully examined the effects of simple exposure of the heart in healthy frogs.

We now know three cardiac nervous centres:—

1. A regulo-motor (Traube), situated in the medulla oblongata (vagus).

2. A musculo-motor, situated in the heart itself; and,

3. An excitory system (V. Bezold), whose centre lies in the brain and spinal cord.

Curara in small doses leaves the vagus intact, and increases the activity of the excitory cardiac nerves; whilst in large doses it diminishes the latter, and renders irritation of the vagus resultless. Whether the extremities of the vagus become paralysed is still a moot point, as is also the question of the paralysis by curara of the motor nerves of the muscles. Heidenhain believes that even after large doses of curara, the ends of the vagus in the heart *remain functionally active;* his opinion being grounded on the cessation of the heart's action, which occurs on electrical irritation of a certain point (junction of the superior vena cava and auricle) on the posterior or dorsal surface of the frog's heart. Heinemann's experiments, however, tend to show that the extremities *are paralysed*, and that the cessation of the heart's movements in this experiment must be differently explained. They show also that the period of the diastolic stoppage after poisoning by strychnine depends on irritation of the extremities of the vagus.

A point of practical importance proceeds from these results—that, as regards the administration of curara after strychnine-poisoning, whilst large doses of curara do not remove the pulse-diminishing power of strychnia, they essentially tend to prevent the occurrence of the transitory diastolic stoppage of the heart.

4. Bidder's experiments also show that the trunk of the vagus is not tetanized by strychnia, as is the case with other motor nerves, from which, therefore, the vagus essentially differs. He remarks that

"As regards the reflex excitability of the spinal cord, so far as it relates to the reflexion upon voluntary muscles it is greatly augmented by strychnia; but this is not the case for the vagal centres. Centres of different function, therefore, react differently towards strychnia."

Even after hours of tetanus produced in the voluntary muscles by strychnia, electrical irritation of the vagus will occasion stoppage of the heart's action. (In this statement Bidder is in direct contradiction to Martin Magron and Buisson.) His experiments do not show whether the reflex irritability of the vagus centrum is augmented or no, but it is probably not increased.

In a male frog the skin was detached from the back by a cross cut, the muscles detached from the bones, and the four last vertebral arches cut through, and the cauda equina exposed; on the left

side, the posterior roots of the three large nerves going to the hinder extremity and the peripheric portions were raised out of the canal and divided. The extremity was thus made wholly insensible to the most powerful irritants, whilst the voluntary movements were unaffected. Then the anterior roots were also divided, which put an end to voluntary movements, whilst galvanization of the peripheric extremities effectually excited vigorous contractions. All these phenomena were brought out so clearly that he was loath to kill the animal, and instead sewed up the wound and took care of it. In four weeks the wound was healed. No gangrenous destruction of the toes occurred, nor even any excoriation; it was a little œdematous—but this soon passed off. It became very thin, and showed an inclination to hybernation, regularly keeping its head under water and remaining motionless, with closed eyes, though on irritation it could move livelily enough. Five months after it was still alive and well; and he now began to think of sacrificing it, with a view of seeing the results of nervous section, especially as regards the question of "vis insita" and the independence of muscular irritability on nervous force, and as regards the independency of the sympathetic nerves on the influence of the spinal cord; and lastly, as regards the relation of the spinal ganglia to the normal nutrition of certain nerve-fibres. On all three points he obtained decisive results. Careful daily examination showed no difference between the two limbs as regards the current of blood, size of the blood-vessels, colour of the skin, volume and consistence of the blood-vessels. Five months after the original experiments, he split the skin down the back, and exposed the gastrocnemius and the sciatic nerves. The sciatic was very white. Application of strong currents to it excited in no part contraction of the gastrocnemius, whilst feeble currents directly applied to the muscle produced vigorous contractions, just as on the right and sound side.

Microscopical examination of the nerves showed fatty degeneration in some places; and elsewhere they presented quite pale, equally broad, very delicate, but sharply defined contours, with numerous small isolated fat-globules. The cutaneous nerves, however, *presented no remarkable alteration from health, which he attributes to the presence of the ganglion on the posterior roots, this being still undamaged, and having been carefully avoided in the original experiment.*

On examining the spinal canal and cord, it was found that both the proximal and distal portions of the cut nerves had entirely disappeared, the latter as far as the ganglion.

As conclusions from this case, Bidder states—

1. The muscular tissue possesses in and by itself a vital power of shortening quite independent of the influence of the nerves distributed to it, though in life the impulse occasioning contraction usually pursues the tract of the nerves.

2. The branches of the great sympathetic entering the nerves of the posterior extremity do not require the uninterrupted influence of the spinal cord for the maintenance of the functions dependent on them; though in the combination of all parts of the entire organ-

ism, as also of the nervous system into one whole, there probably is an influence proceeding from the spinal cord, and acting on the organs or parts of organs supplied by the sympathetic.

3. The anterior roots of the spinal nerves have their nutritive centre in the spinal cord; the posterior roots, on the other hand, maintain their normal condition even when separated from the spinal cord, which appears to be due to the presence of the spinal ganglion.

5. Sczelkow, in his experiments on muscles, found that—

1. The quantity of volatile fatty acids in quiescent muscle varies from 0·1076 to 0·3445 per cent.; on the average, it is 0·2058 per cent.

2. In tetanized muscle, the quantity of the same compounds is 0·0487 to 0·1867; on the average, it is 0·1208 per cent.

Only the presence of acetic and butyric acids have been determined with certainty by Scherer and Wiedler; but Sczelkow has also ascertained that formic acid is present, and he believes, but is not certain, that propionic acid is a constituent.

Respiration.—Animal Heat.

1. Prof. Traube.—*On the Action of CO Gas on the Respiration and Circulation.* ('Verhand. der Berlin Med. Gesellschaft,' edited by Gurlt, Hirsch, and Posner; Heft i, 1866, p. 67.)
2. E. de Renzi.—*On the Temperature of various Parts of the Body at different Periods of the Day, and on the Capillary Circulation.* (Gazetta Med. Italiana, 1865, p. 312; and 'Canstatt's Bericht,' p. 97.)
3. E. Öhl.—*On the Increase that takes place in the Temperature of Nerves at the Moment of Excitation.* (Annali Universali, cxc, 1864-1865; and 'Canstatt's Bericht,' 1865, p. 99.)
4. Julius Arnold: *An Essay on the Finer Structure and Chemistry of the Suprarenal Capsules.* ('Virchow's Archiv,' vol. xxxv, 1866, pp. 64—108.)

1. The conclusions drawn by Traube from his experiments are—The insufflation of CO gas mingled with atmospheric air produces the most violent dyspnœa, providing that the quantity of the gas rises above a certain proportion. Below this proportion no respiratory movements are produced; but there is a considerable increase in the frequency of the pulse, and a remarkable diminution in the size and tension of the arteries. The insufflation of small quantities of CO gas produce the same symptoms and subsequent effects as long-continued insufflation of hydrogen. (See 'Med. Central Zeitg.' 1862, 299.) On the introduction of larger quantities of CO gas, besides the strong dyspnœa, complete loss of sensibility of the cornea occurs.

2. Researches in which injection of arterial blood saturated with

CO gas was performed into the peripheral extremity of the crural artery in animals in which artificial respiration was maintained with pure air, show that this also induces respiratory movements and dyspnœa, which always first appeared *after* the injection. The dyspnœa thus induced lasted usually a quarter of an hour. A second injection was followed by no results.

These results lead to the conclusion that the combination of CO with the hæmoglobulin is a poison acting as an excitor to the respiratory nervous system, but ceasing after a while to affect it.

3. Experiments were performed on animals rendered motionless by woorara, and then *insufflated* with CO gas mingled with air, in order to observe the action of this poison on the circulatory apparatus (13 experiments).

With uninjured vagus and medulla, a few seconds after insufflation there is a rapid increase of the frequency of the pulse, and after half a minute this may amount to double the original number; then it gradually falls in frequency to half the original number; and it then again may rise to a little above the original number, falling greatly as death supervenes. Hence four stages: first and third, pulse rises—third and fourth, falls, in frequency. The changes in the pressure do not run a parallel course, but pass through five stages:

1. Quick and strong fall.
2. Quick rise, till it reaches original level.
3. Fall, perhaps greater than first.
4. Rise to original level.
5. Fall till death.

Of these five conditions, the two first may fail to appear.

On the whole—

1. Carbonic oxide gas acts as an excitor to the inhibitory nervous system of the heart, whence follows the *primary diminution* of the pulse frequency.

2. Carbonic oxide gas acts as an excitor to the centre of the vasomotor nervous system, whence follows the increase of the pulse frequency following the primary retardation.

3. Carbonic oxide gas acts as a weakening agent on the cardiac muscular tissue, hence the diminution of the pressure observed at the commencement of the gaseous intoxication is the result of this action.

4. The *secondary* increased frequency of the pulse does *not* depend upon paralysis of the inhibitory nervous system.

Animal Heat

2. Renzi finds that the temperature of the blood increases as it passes from the arteries to the veins. The temperature of the axilla and of the closed hand are precisely the same in summer. There is no increase in temperature in passing from the distal to the proximal ends of the extremities; on the contrary, in some instances, the temperature actually falls. The skin of the upper limbs is warmer

than that of the lower, and that of the inner surface of both extremities is warmer than that of the outer surface. The temperature of the body continuously rises from early morning till 2 p.m., at which time it is 2° or 3° Fahr. higher than at an earlier period or in the evening, and after which it gradually falls.

This rise in temperature is most conspicuous in young animals; and it does not appear to depend on the kind or quality of food, nor upon the act of digestion. At the same time, food does cause a difference at first, occasioning (unless it be very hot) a diminution and then an *increase* in temperature. The temperature of the surrounding medium exercises only a subordinate influence on animal heat; the temperature of the body always decreases in darkness.

3. Öhl finds that, on introducing a thermo-electric needle into the sciatic nerve of a dog, and connecting the extremities with a thermo-electric multiplying apparatus, a deviation of the needle of the latter constantly occurs, showing that it possesses a high temperature; but if the nerve be now excited, either by electricity or by mechanical irritation, an additional deviation takes place, showing that the temperature of the nerve has become decidedly elevated, whilst a fall takes place as soon as the functional activity diminishes. Öhl also finds that section of the vagus in the neck of dogs, cats, and rabbits causes dilatation of the vessels of the intestinal wall, whilst they contract if the peripheric extremity be electrically excited. With the dilatation of the vessels an increase of temperature is coincident.

4. Arnold, like previous inquirers, distinguishes the cortical from the medullary part of the supra-renal bodies, the whole being enclosed in a capsule. The cortical substance he subdivides into three zones; namely, 1st, an external, or zona glomerulosa; 2nd, a middle, or zona fasciculata; and, 3rd, an internal, or zona reticularis.

1. The zona glomerulosa is subdivided into a number of rounded spaces by trabeculæ dipping in from the capsule, which contain nuclei surrounded by a globular mass of finely granular plasma, but no cell-wall. The thickness of this layer is small in man.

2. The columnar layer, or zona fasciculata, composes the greater part of the cortical layer; it exhibits a striated appearance from without inwards, owing to the direction of the connective-tissue pillars, the striæ lying close and forming, with other oblique and transverse fibres, transversely oblong spaces, enclosing bodies with clear nucleus surrounded by protoplasm. There are no tubes with proper walls in this part, as is well shown on transverse sections, but only a regularly arranged reticulum containing many vessels. There is no essential difference between this and the preceding layer, except in the shape of the spaces enclosed. This layer is well developed in the human suprarenal capsule, and presents much resemblance to a series of tubes.

3. The third layer, or zona reticularis, is more uniform than the other two, and, like them, consists of parenchymatous bodies and interstitial tissue.

The arrangement of the medullary substance is more difficult to determine, being of softer consistence. It also consists of inter-

stitial tissue and parenchymatous corpuscles, the former bounding, in the peripheric parts of the medullary substance, spaces of a long, oval shape, with their long diameter perpendicular to the central vein, and for the most part arranged over one another in two rows. These are subdivided by a delicate reticulum into smaller spaces, in which lie large nuclei with protoplasma surrounding them.

In the central parts of the medulla, the interstitial tissue forms a network in whose narrow meshes lie parenchymatous corpuscles.

The vessels are exceedingly abundant. Those of the zona glomerulosa are arranged in the form of knots, which correspond to the so-called capsular spaces. Those of the zona fasciculata run, at pretty regular distances from one another, radially from the periphery towards the medulla. These lie in the connective-tissue trabeculæ of this zona, and constitute its chief constituent.

Those of the zona reticularis form a very close capillary network, in whose meshes only a few parenchymatous corpuscles lie.

The course of the vessels in the cortical substance is as follows:—The vessels arriving at the surface of the cortical substance, and arranged in the form of limited vascular districts, form arterial knots in the zona glomerulosa. Out of these proceed white vascular tubes, which perforate the zona fasciculata in a radial direction, and at equal distances. By manifold division and reunion, these vessels form a very close vascular network in the zona reticularis. The vessels of the medullary substance spring from those of the zona reticularis as fine venous radicles, which first run parallel to the surface of the suprarenal capsules, and then pass towards the central vein. Between the venous channels sinuous spaces occur, which, like the vessels, possess a homogeneous and very delicate wall.

In the cortical parts of the medullary substance arteries and veins are found. The first originate out of the arterial vessels, which pass to the surface of the suprarenal capsules, and then run through the coarse connective-tissue pillars to the medullary substance, and form in these a network out of which the venous vessels originate, which ultimately pour their blood into the central vein.

As regards the function of these bodies, Arnold points out, on the one hand, their resemblance to those glands which exert an influence on the chemistry of the blood, and, on the other, their similarity in structure in some respects to the liver and to the spleen.

He finds that the suprarenal capsules contain a substance which becomes intensely red when exposed to light and air, and gives, in its alcoholic solution, a strongly coloured precipitate with neutral acetate of lead. This substance differs from hæmatin in being soluble in alcohol and water.

Digestion.

1. Dr. Michael Freyfeld Szabadföldy.—*Essays on the Histology of the Mucous Membrane of the Tongue.* Plate I. ('Virchow's Archiv,' Band xxxviii, Heft ii, p. 177.)

2. C. Eckhard.—*Essays on the Salivary Secretion.* (Henle and Pfeuffer's 'Zeitschrift,' Bd. xxix, Heft i, p. 74. 1867.)

1. Szabadföldy's observations on the histology of the mucous membrane of the tongue were made on dogs and rabbits, which are well adapted for the purpose, and on man, in whom there is more difficulty. He removed the epithelial layers of cells by maceration of the organ in dilute soda solution, and examined the sections in a fluid composed of equal parts of glycerin, water, and alcohol, with a little admixture of oxalic acid. He admits the three well-known forms of lingual papillæ, viz., the filiform, the fungiform, and the circumvallate, and he estimates the number of the fungiform papillæ in man at from 160 to 290, whereof the third part at least occupy the tip of the tongue. The number of the circumvallate he reckons at from eighteen to twenty.

The epithelial layers are thickest on the dorsum of the tongue, thinnest at the lip, and in the parts adjoining the circumvallate papillæ. He describes the muscular fibres (as Hyde Salter did long ago) as dividing dichotomously near the surface, and capable of being followed into the papillæ themselves, the sarcolemma splitting into fine bundles of connective tissue which, mingled with elastic fibres, constitute the chief part of the substance of the papillæ.

The matrix of the filiform papillæ and of the secondary papillæ which form on them contains in general only vessels, but one of the secondary papillæ in each system always contains a nerve or more rarely a nerve and vessels; the latter running nearer to the surface than the former. The secondary papillæ of the fungiform papillæ are to be regarded as almost the exclusive seat of the termination of nerves distributed to these papillæ, and in the circumvallate papillæ every secondary papillæ contain not only vessels but a number of terminal branches of nerves.

2. M. Eckhard remarks that Bernard's statement is well known to the effect that the lingual ganglion possesses the important function of transferring in a reflex manner upon the fibres of the chorda tympani, excitations affecting the peripheric expansion of the lingual nerve, and thus inducing the secretion of saliva. This statement M. Eckhard believes to be erroneous, and he has performed certain experiments which lead him to the conclusion that the parotid glands of the sheep secrete fluid *uninterruptedly* as he has observed the process for hours continuously. In general, he collected from 3 to 5 centimètres every five minutes from each gland amounting to the considerable quantity of from 1728 to 2880 cubic centimètres in twenty-four hours. But in the case of the submaxillary gland the secretion is poured forth intermittingly, as occurs also in the dog. It is interesting, he observes, to inquire what cerebral nerve or nerves govern the secretory activity of the parotid glands in sheep, showing, as they obviously do, no sign of exhaustion. His observations tend to prove that the persistent activity of this gland is not under the influence of any cerebral nerve nor even of the sympathetic, since in one instance he carefully exposed and successively stimulated by means of an electrical current, 1. The branches of the fifth; 2.

The branches of the portio dura of the seventh; and 3, The sympathetic trunk in the highest part of the neck; and in every instance without producing any change in the secretion, nor did section of the sympathetic, nor extirpation of the superior cervical ganglion appear in any way to influence it. He then endeavoured to ascertain whether the excitation of the mucous membrane of the mouth in a healthy sheep exerted any influence, but found none. Thus, the quantity discharged being ascertained from previous experiments to be about 6·3 c. centimètres per five minutes, on brushing the animal's mouth over with vinegar, it not only did not increase the amount of saliva discharged, but actually diminished it to 5 c. centimètres in the same space of time. The cause of increase at different times he believes may, nevertheless, be the result of three factors, namely, 1. The direct reflectorial transference of the stimulus, whatever it may be, upon the secretory nerves. 2. The co-excitation which affects the secretory nerves and those distributed to the muscles affecting the movements of the jaw; and 3. The mechanical pressure to which the glands are subjected by the contraction of the platysma on the outer side and of the masseter on the inner side.

The parotid, however, seems not to be acted on by the first of these factors, that is, reflectorially.

He states that these experiments differ considerably in their results from the analogous ones that he himself made upon the horse in the following respects: first, that the secretion of the parotids in the sheep is not under the influence of any cerebral nerve, as can be clearly proved in the horse; and, secondly, because it cannot be shown that the secretory process in the sheep is commanded by the sympathetic. A secretion of a different quality is, no doubt, obtained when the sympathetic is stimulated from that poured forth on excitation of the trigeminus, which only shows satisfactorily that the secretion corresponds in its characters with the nerve stimulated. But he found that if the sympathetic were stimulated for a considerable period, there was a continuous flow of troubled saliva.

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

By Benjamin W. Richardson, M.D., F.R.S.,
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I. Toxicology.

Poisoning by Fluid Extract of Gelseminum.—Dr. R. P. Davis reports two instances of acute poisoning by the fluid extract of gelseminum. The symptoms unfortunately are to some extent obscured by others due to alcohol; but, notwithstanding this, the account is of great interest in toxicological science. The facts are as follow:

On the evening of October the 6th, 1866, Dr. Davis was called to see a young lawyer of Parkersburg, W. Va., who was reported as extremely ill. He was found lying upon his left side, with his face congested, and his pupils dilated but responding to the different degrees of light; his eyelids were half-closed, with apparent inability to move them; his lower jaw was drooping, and his tongue, to use his own expression, "was so thick he could hardly speak;" his skin was warm and moist; his pulse was small and feeble, and his respirations were somewhat diminished in number. He had neither purging nor vomiting.

Upon being questioned, he said that he and a friend had been enjoying themselves in a social way for three days, and that nothing was the matter with him now "but extreme nervous prostration." He also said he had not taken medicine of any kind. Thinking, as he did himself, that he was merely suffering from the prostration of dissipation, Dr. Davis ordered him "a brandy punch," and went to the drug store for some medicine. Whilst the prescription was being made up, the friend of the patient entered the store staggering and saying, "I am blind, I cannot see; what in the world is this I have taken?" (at the same time showing a bottle); "my friend is down town in the same fix." On examining the bottle, it was found to be labelled "Fluid extract of Gelseminum; and on inquiry it turned out that both the men had taken a dose of one tablespoonful. Thereupon Dr. Davis sent his assistant (Mr. White) immediately to the young lawyer to give him an emetic, he himself staying with the second patient, and administering to him, first, an emetic, which acted freely, and afterwards a dose of quinine.

A few minutes later Mr. White returned to report that the lawyer was dying, that it was with much difficulty he had got him to take the emetic, and that it had not acted. In company with Dr. Clark Dr. Davis returned to this patient, and found him really in the dying state; the pupils were widely dilated, the breathing was spasmodic, the surface of the body was cold and congested, the pulse was almost imperceptible, and there was total unconsciousness. Mustard was applied, the body was sponged with brandy, and artificial respiration was set up, but all to no effect. He died at 8.30 p.m., about two hours and a half after taking the poison.

On returning to his second patient, Dr. Davis found him inclined to sleep, with deep inspirations, and a numbness of the whole body. The quina was repeated, with some brandy, and he was kept walking about with the aid of two friends. At ten o'clock he could take food, and soon afterwards he was sent to bed, and slept soundly. In the morning he expressed himself as "quite well, but weak and dizzy." He recovered without any further difficulty.—*American Journal of the Medical Sciences for April*, 1867.

[The fluid extract of Gelseminum taken by these gentlemen was prepared by Tilden and Company of New Lebanon, N.Y. No facts are given as to the medicinal action of this extract, but we believe that in America it is used as a sedative.] Its action is considered to be uncertain.

Poisoning from Croton Oil.—Dr. Charles Shoyer relates a case of poisoning by croton oil. He had ordered as an embrocation three drachms of croton oil with an ounce of olive oil. By an accident the mixture was spilt over some pigeons and chickens which had been dressed for cooking, and were ready to be put to the fire. The birds, having been thoroughly washed in several waters, were thought fit to eat, and were prepared accordingly. Five persons in all partook of the meal, viz., the mother of the family, aged 54, and feeble; a young lady, 15; a girl aged 9; and two young sons of 17 and 19. Dr. Shoyer was sent for about an hour after the meal, and found them suffering from great anxiety, and with burning in the mouth and throat. He prescribed an opiate, milk diet, and quietude. The symptom manifested by all was purging, and in every case except one this symptom had passed off by the following day. In the case of the mother, after an interval of eighteen hours from the accident, she was again attacked with purging, and while at her work felt a burning in the throat and mouth; there were also some pustules to be seen in the mouth; she became faint, and commenced to vomit; there were muscular tremours, and great general prostration. The pulse was very feeble, there was slight tenderness of the abdomen, but none of the epigastrium; the tongue was white, and showed the impress of the teeth, the feet were cold, the skin was moist, the mind was clear. The vomiting continued an hour; the ejected matter was light and glairy, was not tinged with bile, and contained no food. During vomiting and for two hours after there was a constant tendency to faint, and a deathly indescribable feeling of prostration. The treatment consisted of application of flannels wrung out of hot water to the abdomen, a large sinapism to the epigastrium, and warmth to the feet. Ten drops of tincture of opium were given every half-hour in whiskey and water, and the fan was freely used. In four hours the patient was well, but very weak. Fifty-five hours after the accident she was up and about.—*Ibid.*

[The symptoms in this case closely resemble those of a case recorded by Christison, except that in the latter death was the result. The mistake of Dr. Shoyer's patients in considering that frequent washing would remove the poison was natural enough, although so serious. When animal substance, acted upon by croton oil, is well washed, all trace of the oil seems to disappear; but the volatile acid substance on which the action of the poison depends readily passes into the animal tissue and impregnates it.]

On Sublimation of the Alkaloïds.—Dr. Guy publishes a very interesting and valuable paper on the sublimation of the alkaloïds, a branch of toxicological research, which dates only from the year 1864, and which still admits of considerable advancement. To render the subject most intelligible, we will relate it in nearly the words of Dr. Guy. He commences by stating that Dr. A. Helwig, of Mayence, first proposed the sublimation of the alkaloïds as a test and diagnostic,[1] and more recently (in 1865[2]) he published a work,

[1] Fresenius's 'Vierteljahrschrift für analytische Chemie,' 1864, i.

[2] 'Das Mikroskop in der Toxikologie. Beiträge zur mikroskopischen und

in large 8vo, of which the greater part is devoted to the tests for the poisonous alkaloids—morphia, strychnia, brucia, veratria, atropia, aconitine, solanine, digitaline, conia, and nicotia. In the case of the fixed alkaloïds, the results of sublimation and the reactions of the sublimates are minutely described. The work contains carefully prepared tables of the reactions, and is enriched by no less than sixty-four micro-photographs, of which thirty-eight represent the crystalline forms of the alkaloïds and their salts with various reagents, and fourteen are devoted to sublimates and their reactions.

Dr. Helwig states that the idea of submitting the alkaloïds to sublimation first suggested itself to him as a natural extension of a method so successful in detecting and identifying minute quantities of arsenious acid and corrosive sublimate; and he lays claim to originality, inasmuch as he does not find the sublimation of the alkaloïds described in any handbook of chemistry or forensic medicine, even the most recent. This claim of originality is fully justified, though probably every manufacturing chemist must have recognised the fact that some at least of the alkaloïds are sublimed by heat, and experimenters on a small scale must have observed that the alkaloïds, as a class, after melting, and before depositing carbon, give out a more or less dense vapour or smoke, which, if allowed to settle on a cool surface, might possibly present, under the microscope, characteristic appearances.

This new application of the test of sublimation suggested itself to Dr. Helwig after becoming acquainted with the simple methods of obtaining sublimates of arsenic and mercury on flat surfaces, with a view to microscopic and chemical examination, which I proposed in the year 1858.[1] His own method of procedure with the alkaloïds is as follows:—In a piece of platinum-foil of moderate thickness, a small cup-like hollow is formed; in this a minute quantity of the alkaloïd is placed, and over it a microscopic slide (*Objectträger*). This simple apparatus being placed on a suitable support, the flame of a spirit-lamp is cautiously applied until the alkaloid melts, from which point of time the sublimate begins to form on the glass slide.

This mode of procedure is obviously open to objection. The successive changes that take place in the alkaloïd—the discoloration, the liquefaction, the deposit of carbon, either on the spot (as happens with some alkaloïds) or over a wide surface traversed by the liquid (as is the case with others)—cannot be distinctly seen, and some diagnostic marks of the alkaloïds as a class, and among themselves, are thus lost. Nor, again, can the formation of the sublimate

mikrochemischen Diagnostik der wichtigsten Metall- und Pflanzengifte, für Gerichtsärzte, gerichtliche Chemiker und Pharmaceuten, mit einem Atlas photographirter mikroskopischer Präparate. Von Dr. A. Helwig, pract. Arzte und Grossherzoglich-Hessischem Kreiswundarzte in Mainz. 1865.

[1] "On the Production and Identification of Crystals of Arsenious Acid and Crusts of Metallic Arsenic" (Beale's 'Archives of Medicine,' No. iii, 1858); also "On the Miscroscopic Characters of the Crystals of Arsenious Acid" ('Journal of the Microscopic Society,' 1861, and 'Principles of Forensic Medicine,' 2nd edit., 1861, p. 372).

itself be seen and watched, as it should be if we would obtain satisfactory results. There is also some risk in this sudden mode of applying heat, of causing the glass, which should receive the sublimate to break.

The method of procedure to which Dr. Guy gives the preference is the following:—Provide small crucible covers or slabs, or fragments of white procelain, a few microscopic cell-glasses, with a thickness of about one-eighth of an inch, and a diameter of circle of about two-thirds of an inch, and disks of window-glass about the size of a shilling. Place the porcelain slab on the ring of a retort-holder or other convenient support, then the glass cell, and upon the porcelain, in the centre of the cell, a minute portion of the alkaloïd or other white powder, or crystal reduced to powder. Then pass the clean glass disk through the flame of the spirit-lamp till the moisture is driven off, and adjust it with the forceps over the glass ring. Now apply the flame of the spirit-lamp to the porcelain, underneath the powder or crystal, and continue the heat till the powder undergoes its characteristic change and gives off vapour. Watch the deposit of this vapour on the glass disk, and remove the spirit-lamp either directly or after a short interval, as experience may determine.

The reasons for recommending this mode of procedure in preference to that advocated by Dr. Helwig are—By employing a flat white slab of porcelain, the heat of the lamp is applied gradually, and every change of consistence, colour, and position which the powder undergoes is easily observed. The ring of glass, as compared with a ring of metal, has the advantage of conducting the heat from the surface of the porcelain to the glass disk so slowly as to guard effectually against the danger of breaking, and if the powder after melting changes its place, the glass ring, with the disk upon it, is easily shifted. The disk of window-glass is very convenient both for the experiment itself and for the subsequent application of liquid tests. It will also bear a moderate heat if required. The disks, however, are not essential; their chief recommendation is the facility they afford of multiplying experiments. The common glass slide, or a slip of window-glass (as being less liable to scratch, and bearing heat better than plate-glass) may be substituted when only a few experiments are intended to be made.[1] An oblong slab of wood, somewhat larger than the microscopic slide, with a circular aperture and ledge to support the glass disk, enable us to examine the sublimate under the microscope, and a similar piece of thick cardboard, with a hole punched in the centre of it, serves for the mounting of the preparation.[2]

[1] As the disks of glass can only be conveniently cut by a revolving diamond, which few glaziers posess. it may be well to mention that they may be procured of Mr. Eade, 130, High Holborn, at a cost of two shillings the gross.

[2] When dealing with larger quantities (such as a grain or more) of the alkaloïds, the short specimen tube may be substituted for the porcelain and microscopic cell-glass. But the results are far from satisfactory; and it would certainly be better to sublime successive small portions of a hundredth of a grain or less, from the porcelain.

By this mode of procedure Dr. Guy has obtained sublimates of veratria and solanine, which correspond closely with the descriptions and photographs given by Helwig; but, in the case of strychnia and morphia, he got very distinct and extremely beautiful crystalline sublimates (not exceptional, but as the rule) where Helwig has failed;[1] and though he is not yet prepared to assert positively that the strychnia and morphia sublimates can always be distinguished by their crystalline forms alone, he is able to correct the statements contained in the following passages:

"It (the sublimate of morphia) consists of perfectly homogeneous spots of round, very sharply defined granules, closely packed together, which, when magnified a hundred and sixty times, are transparent, but among which no trace of a crystalline formation can be discovered (p. 9);" and "Examined microscopically, a sublimate of strychnia is not to be distinguished from a sublimate of morphia; precisely the same spots of round transparent granlues, without trace of crystalline formation (p. 21)."—*Pharmaceutical Journal*, June, 1867.

Action of Nitrite of Ethyl.—In a previous report we ventured to state our own researches on the action of certain of the bodies of the Amyl series. We have since then, at the instance of the British Association for the Advancement of Science, extended our inquiries to some of the ethyl compounds. The most potent poison amongst the ethyls is the compound known as the nitrite, a substance made by passing nitrous acid fumes through ordinary pure alcohol.

The fluid when pure is of light-amber colour; the specific gravity is 0·950, and the boiling-point 60° Fahr. To work with it, it must be mixed with absolute ether in fixed proportions, say of ten, twenty-five, or fifty per cent. It is so volatile that, without this precaution, it cannot be readily employed.

The action of nitrite of ethyl is closely analogous to the action of nitrite of amyl. Inhaled in quantities of not less than a grain, it induces the same sensation of fulness in the head, rapid action of the heart, and some suffusion of the skin. Animals subjected to it in the proportion of fifteen minums diffused as vapour through a cubic foot of air, die almost instantaneously from sudden failure of the heart; but even up to the moment of death they retain their consciousness and sensibility. The nitrite consequently is in no sense to be considered as an anæsthetic.

Precisely as with nitrite of amyl, nitrite of ethyl when it kills leaves the lungs entirely collapsed, and so perfectly white that one could assume they had been carefully washed free of blood. This effect is due perhaps to the rapid contraction of the pulmonary capillaries. The blood is changed in colour, the arterial blood being rendered very dark, and the venous of a deep chocolate tint; the coagulation of blood is not modified, the muscles are also all left blanched, as if the death had occurred from loss of blood.

[1] Dr. Guy has also obtained very fine crystalline sublimates from the new alkaloïd Cryptopia.

In describing the action of nitrite of amyl, we explained that on cold-blooded animals it suspended their animation, and that frogs which have been rendered powerless by it, and to common observation inanimate, would sometimes spontaneously recover, even so long as nine days after the administration. This same phenomenon we have observed with nitrite of ethyl, together with another even more singular. It is this: If a young animal, say a kitten, be subjected suddenly to the nitrite so as to fall senseless, and to appearance dead, in or within the minute, it will remain in the same state for six or even ten minutes, yielding no evidence of life. It will not breathe, and the most delicate auscultation will fail to detect motion of the heart. But after a period varying from six to eight minutes it will spontaneously recommence to breathe, and with every movement of expiration a breath sufficient to dull a mirror will pass from the nostril. As the breathing recommences the heart also will begin its work, making a series of distinct intermittent strokes. This condition, looking like an actual return of life, will last so long as half an hour, and will then cease gradually, the animal lapsing again into a state of actual inertia or death.

On the human subject the action of the nitrite of ethyl is most marked after inhalation. The nitrite causes extreme suffusion of the face, a sense of fulness of the vessels of the brain, a tendency to pitch forward, a rapid and painful respiration, and excessive overaction of the heart. The effects cease after the agent is withdrawn more quickly than when nitrite of amyl is employed; but for intensity of action, up to the period of full action, the ethyl is as powerful as the amyl nitrite.

On Poisoning by Compounds of Cadmium.—Dr. W. Marmé has collated a number of facts, chiefly experimental, bearing upon the toxicological action of the various compounds of cadmium. Our readers are aware that in its chemical relations cadmium resembles zinc more closely than any other metal, from which, however, it is distinguishable by the fact that it yields a yellow sulphide which is not dissolved by hydrochloric acid. The yellow sulphide of cadmium is used as a colouring agent by artists, and has been assumed but without much evidence to act injuriously. Dr. Marmé refers to the action of all the compounds; he states at the onset that the sulphide which Van Hasselt pronounced to be poisonous is not poisonous, but may be taken in small doses by animals for a week without danger. The insolubility of the salt in water, weak acids, weak alkaline solutions, and oil, is the reason why no injurious effects follow its employment. This is an important point, as it permits us to sanction the continued use of the sulphide as a pigment.

Those preparations of cadmium which are soluble in warm water have an analogous action as poisons, and they affect both men and the inferior animals. Experiments have been made with oxide of cadmium (Cd O), the oxyhydrate, the chloride, the iodide, and bromide, the potassio-chloride, the ammonio-chloride, the sodio-chloride, and other compounds. The local effects of the poisonous compounds of cadmium, when they are administered by the stomach in small doses, are

vomiting and purging,—gastro-enteritis, varying from the simple catarrhal form to the ulcerative; but in no case is there any perforation, not even when the chloride is given in the concentrate form. By hypodermic injection according to the strength of the solution, there follows a simple hyperæmia, or copious exudation, or free suppuration. The further or ultimate effects are fainting, sickness, purging, slowness of action of the circulation, and respiration, general prostration, loss of consciousness, and spasms of the limbs. In animals destroyed by the poisons, the action of the heart as a general rule continues longer than that of the respiration; the peristaltic action also continues after death for some time. When injected into the vascular system, the poison produces inflammatory action of the stomach and bowels, at times even hæmorrhage, erosion, and ulceration. The introduction of large quantities of dilute soda solutions, if quickly used, removes the poisonous effects perfectly. For acute poisoning, the alkaline carbonates with albumen form the best antidote.

Cadmium compounds which act as acute poisons in large doses produce in small doses distinct chronic symptoms. These symptoms are those of deranged digestion with emaciation, until death ensues. After death, from chronic poisoning, there is found evidence of gastro-enteritis, sometimes subpleural exudation, and injection of the lungs, together with fatty degeneration of the liver, and of the muscular structure of the heart, and diffuse inflammation of the structure of the kidney. When the animal suffering from a poisonous preparation of cadmium recovers, the poison is eliminated almost entirely, if not exclusively, by the kidney. The method for separating cadmium salts from the stomach and intestinal secretion recommended by Dr. Marmé, is the dialyzer of Graham three or four times renewed.—*Zeitschrift fur Rat. Med.*, xxiv, 1867, *and Schmidt's Jahrbücher*. Band 133. No. 3, 1867.

Case of Acute Poisoning by Alcohol.—Dr. A. Mitscherlich relates the case of a soldier twenty-nine years of age, who, on the 24th of August, took at one draught a quart of rum. He fell on his left side, with his left hand under his body, and in this condition he laid for thirty hours before medical aid could be rendered him. On the 26th of August, at 9 p.m., he laid in a deep coma, and when disturbed only gave vent to a suppressed groan. The sensibility was greatly decreased; there were no active convulsions, but now and then slight movement of the extremities. The pupils were dilated and yielded scarcely any reaction in the light: the pulse was weak, easily compressible, and extremely variable, being at one time 140 beats in the minute, and at another not countable. The skin of the whole body was of dark blue livid colour, in patches œdematous and swollen: some places on the left side of the body on which the patient had rested, looked as if they had been burnt. In some parts there was formation of blisters.

Various remedies internal and external were applied to the patient, and from the 22nd to the 25th the man was restless and excitable, but unconscious; the œdema had passed away; the skin had resumed

its whiteness, and the pulse sank to 120, but rose again in the evening to 140 beats, on which the congestion of the head increased. The breathing was difficult, and pressure on the region of the stomach elicited signs of sensibility. On the 26th the sensorium regained function, the tongue was clean, but the pulse was still irregular. The patient complained now of acute pains in the head, great thirst, and entire loss of appetite. In the afternoon of the 26th at 4 p.m., the patient became suddenly restless and delirious; the pulse was irregular, quick, and scarcely perceptible. Two hours later there was spasm of the muscles of the face and jaw: he lapsed again into unconsciousness with intervals of lucidity. At 8.30 p.m. he died. The post-mortem revealed congestion of the longitudinal and transverse sinus with fluid, injection of the pia mater and of the choroid plexus. In the dura mater there was evidence of serous exudation, and there was also exudation at the base of the brain. The brain-substance was firm and very full of blood, and there was a little fluid in the lateral ventricles. No smell of alcohol was perceptible. The pericardium contained from three to four ounces of dark serous fluid; the left ventricle contained a little and the right a good quantity of dark gaseous partially decomposed blood. The muscular structure was flabby and dark. The lungs, especially at their depending parts, were congested, and contained little air. The veins were filled with dark half-fluid blood. The mucous membrane of the bronchi and trachea was strongly infiltrated. In the chest cavity there was no smell of alcohol. The stomach was much distended with gas, and contained about six ounces of a thin, yellow, frothy fluid. The mucous membrane was strongly injected, and at the cardiac orifice it was spotted red and was raised from emphysema; the large vessels were distended. The spleen was dark and soft, the kidneys were large and strongly injected, with the capsule slightly separated. The liver was greatly enlarged, soft, and darker than natural. There was no smell of alcohol even in the cavity of the abdomen. There were no fibrinous deposits either in arteries or veins.—*Virchow's Archives*, No. xxxviii, 1867.

III. Hygiene.

Reports on Cholera.—We notice three very able reports on cholera, during the past six months: One is by Drs. Hayden and Cruise; another by Dr. Gordon K. Hardie of the 73rd regiment; and the third by Dr. Robert Bartholow, Consulting Physician of the Mercy Hospital, Cincinnati. We regret we can only supply the conclusions of these authors.

Drs. Hayden and Cruise come to the following conclusions:

1. The diarrhœa so prevalent amongst the inhabitants of localities actually suffering from cholera, is a premonition and the earliest manifestation of cholera.

2. Cholera is a disease strictly prevented by sanitary and hygienic measures, and in nearly all cases curable in the stages preceding collapse.

3. The treatment we (the authors) have found most successful in

the stage of choleraic diarrhœa is sulphuric acid and opium, with sinapisms, external heat, and creosote water.

4. In collapse we (the authors) have had more success with calomel given in large doses than with any other medicinal agent. In several cases besides those in which recovery took place, reaction set in under the calomel treatment, but death occurred in the consecutive fever.

5. We (the authors) believe the cholera to be contagious, but in a much less degree than the principal endemic contagious diseases of this country, viz. typhus, scarlatina, measles, and smallpox.

6. As regards individuals exposed to the contagion of cholera, a state of good health, with sanitary and dietetic precautions, afford a strong assurance of immunity from attack.

7. Immediate attention should be given to derangement of the stomach or laxity of the bowels during a cholera epidemic; and to ensure this amongst the poor, house to house visitation seems indispensable.—*Dublin Quarterly Journal of Medical Science*, May, 1867.

Dr. Hardie comes to the next series of conclusions:

1. That cholera is not due to atmospheric or telluric influences, nor is it susceptible of spontaneous origin from bad local sanitary conditions.

2. But it is due to a contagion transportable from place to place, namely, by human intercourse, or by clothing, or other articles infected by persons ill of the disease.

3. That when the disease appears in a vessel some time after leaving port, it is probable that the contagion has arisen from exposing some articles of clothing which had previously been kept close from the air.

4. That personal infection takes place under favourable conditions of overcrowding, and with a very short period of latency, and may be presumed to be the most common mode when the disease appears on embarkation or immediately after.

5. That running water seems to be a powerful agent in carrying the contagion of the disease.

6. That drought following cholera may, by contaminating the subsoil water, give rise to secondary epidemics.

7. That previous exposure to cholera influence seems to diminish susceptibility to the disease.

8. That race, under otherwise similar conditions, shows a varying susceptibility, which may, perhaps, be the result of No. 7.—*Ibid.*, February, 1867.

Dr. Bartholow, in his most able report, dwells specially on the nature of the poison of cholera. He argues that if the cholera poison is contained in the rice-water matter, it probably acts locally upon the intestinal mucous surface, and all the other phenomena of the disease are secondary to the changes induced in the blood by the outward diffusion current. It becomes, then, a matter of prime importance to determine this point, and without designing it he became subjected himself to experiment.

A wound in his left hand bleeding freely was immersed in the

various fluids of the body of a patient on the post-mortem examination; no result followed.

Some fresh rice-water excretion was injected subcutaneously into a dog, and some into the rectum. There was no result except local inflammation from the injection.

The experiment was repeated on the same dog, some of the secretion being also poured into the stomach. He continued unaffected.

Some of the dried matter was made into a solution with water, and a portion injected into the thigh, the rest being poured into the stomach. In fifteen minutes the animal had free watery evacuation, which was frothy, but no subsequent symptom resulted.

As the gastric juice of the dog is powerfully acid and the stomach digestion very active, it was necessary to bring the cholera matter into contact with some other mucous surface. Bartholow, therefore, performed tracheotomy on the same dog and injected cholera matter into the trachea, into the nasal passages, and freely into the stomach. The dog recovered promptly even from this rough treatment, ate well, and showed no sign of cholera. The experiment was repeated with the same results. Bartholow finally remarks, "The results of these experiments are thus far nearly negative. They indicate, however, that the fresh rice-water matter, and the other fluids of the body in the recent state are perfectly innocuous. The dried matter appeared to have more effect. Future experiments must be made with the rice-water discharges in a state of change if anything is to be accomplished in this way. The dejections are poisonous if at all under some as yet undetermined condition."

Part III. Summary.

The Organic Impurities of Water. By Dr. W. M. Proctor.—An excellent paper. The properties of a good water are thus rendered:—Water for drinking purposes should be clear, colourless, inodorous, tasteless; it should be well aërated, and especially cool. It is difficult to fix the exact amount of dissolved constituents. As a general rule, subject to limitation, it should not contain more than:—of organic matter, one grain; of carbonate of lime, sixteen grains; of sulphate of lime, three grains; of chloride of sodium, ten grains; of carbonate of soda, twenty grains per gallon.—*The Lancet*, September 8th, 1866.

Effects of the Bite of the Scolopendra Heros (centipede). By Dr. H. C. Wood, jun.—Dr. Wood details the experiences of Dr. G. Linceicum, of Texas, who attended a fatal case of bite from the centipede, and knew of five other serious cases. The symptoms occur instantly, and are those of great oppression, vomiting of pale yellow glairy matter, livid spots on the skin, and, in extreme cases, an "elastic puffiness" of the whole surface of the body. The Mexicans have an antidote for the poison, viz., the bruised root of the Tephrosia Virginiana, boiled in sweet milk, and taken freely at short intervals.—*American Journal of the Medical Sciences*, October, 1866.

The Land of Death. By W. J. Moore, L.R.C.P.—This paper is

a good history of a district in India to the east of Scinde, and called Marwar—The Land of Death. The commercial city of Marwar is Palee. The narrative of Dr. Moore bears strong evidence of the dependence of endemic diseases, especially fever, on impure drinking water.—*The Indian Annals of Medical Science*, August, 1866.

Morphia and Caryophyllum compared by the Nitric Acid Test. By A. F. Haselden.—Mr. Haselden points out that when nitric acid is added to morphia or its salts, the colour produced, which is first red, turns to orange yellow. When the same test is applied to oil of cloves, the red colour is developed; but it does *not* change to yellow. Further, if we add to the different solutions of morphia and nitric acid, and cloves and nitric acid, some solution of chlorinated lime, and expose the solutions to the light for some hours; the clove solution will become colourless, while the morphia solution will retain a pale straw colour. These, Mr. Haselden thinks, are distinctive tests.—*The Pharmaceutical Journal*, October, 1866.

What effect has the Flesh or Milk of diseased Animals upon the Public Health? By Samuel R. Percy, M.D.—Dr. Percy on this subject writes temperately and instructively. His most novel point is a description of the disease of animals called "milk sickness," or the "trembles." The disease exists only in newly-settled districts, and disappears as the country becomes well cultivated. The author agrees with Dr. Drake in believing the malady to be produced by the cattle feeding on the leaves and tender twigs of the *Rhus toxicodendron*. He states that the milk, butter, or flesh of animals suffering from this sickness, either in the acute or severely chronic form, causes a prostrating sickness if eaten by man; and if he recovers from its first effects, he is left in a sickly condition, and will frequently for years, or for the balance of his life, complain that he has never fairly recovered.—*New York Medical Journal*, August, 1866.

Successful Treatment of a Case of Poisoning by Morphia; Belladonna the Antidote. By Dr. Mitchell.—The great therapeutical fact that belladonna acts as a direct antidote to morphia becomes every day more and more fully verified. The case by Mitchell is of striking importance. The patient had swallowed the large dose of ten grains of the sulphate of morphia, and was not subjected to any treatment for several hours. Dr. Mitchell treated the case at a late stage by first throwing a drachm of tincture of belladonna into the rectum, and afterwards injecting a quarter of a grain of atropia under the skin. The body was kept warm, and the breathing was regulated by the galvanic battery. The recovery was complete.—*New York Medical Journal*, November, 1866.

[Our readers will find in our previous reports several cases similar in result to the case here recorded. They will also find some most valuable information on the same subject in a work on injuries to the nervous system by Drs. Weir, Mitchell, Morehouse, and Keen, of Philadelphia, published in the year 1865. The experiments re-

corded in this volume appear to remove all and every doubt as to the antagonism which exists between opium and belladonna. It is, however, fair to mention, that the theory of such an antagonism has its opponents, who argue on their side with considerable power. The latest of these is Dr. Fraigniaud, who records his researches at length in the 'Gazette des Hôpitaux' for May 1 of the present year. Fraigniaud injected subcutaneously opium and belladonna in full doses, and the conclusion he arrives at is, that belladonna exerts no effect as an antidote to morphia, nor morphia to belladonna. His paper is laboured, and his arguments are set forth in fair and clear style.]

Anæsthesia considered in a Medico-legal point of view. By G. Tourdes.—In this elaborate paper the author considers chloroform and other anæsthetics in relation to their application for the purposes of the homicide and the suicide.—*Gazette Hebdom.*, No. 2, Series III, 14, 21, 22, 1866.

CHRONICLE OF MICROLOGY.

By J. F. Streatfeild, F.R.C.S.,

Assistant-Surgeon to the Royal London Ophthalmic Hospital, and Ophthalmic Surgeon to University College Hospital, &c.

Part I.—Physiological Micrology.

Nucleation of Blood-cells.—Professor Rolleston, investigating the truth of the statement of Dr. Kuhne, "to the effect that only some mammals (the sloth and the camel) possessed nucleated blood corpuscles," and employing a twelfth of an inch object-glass of Powell and Lealand's, has been convinced that, though a certain number of the dried blood corpuscles of the sloth do contain one or more nuclei more or less roughly hewn and irregularly and eccentrically placed, still the immense majority of them have the non-nucleated character ordinarily assigned to the mammalian red blood cell. In the smaller corpuscles of the camel he could not detect the presence of nuclei in the coloured blood-cells. He continues: "Bearing in mind Nasse's observation as to the comparative frequency of the presence of a large colourless nucleus, or, in the place of it, of an area of fainter coloration, in the coloured blood-cells of the pregnant human subject, and also of the pregnant bitch, I examined the blood from the uterine veins of a cow which had been killed, in ignorance, as I was told, towards the end of the period of gestation. But I was unable to discover any nucleated red corpuscles in the blood from this source." The author has since examined the blood of an elephant: "In this blood very many nucleated red blood-cells were visible; but in all observed, with

perhaps one exception, *the coloured factor was internally placed,* whilst the colourless formed the envelope."—*Quarterly Journal of Microscopical Science*, April, 1867, pp. 127, et seq.

Networks or Plexuses of dark-bordered Nerve-fibres —Dr. Lionel S. Beale gives two new drawings illustrating the manner in which networks of dark-bordered nerve-fibres are arranged in voluntary muscle, and upon the deep surface of a highly sensitive mucous membrane.

In the first, the long elementary muscular fibres are seen partly separated from one another, so that, under high powers, the very fine nerve-fibres in the intervals between them can be traced and followed to and from bundles of dark-bordered nerve-fibres.

In the second are seen the division and subdivision of complex nerve-trunks and the formation of nerve-trunks by the union of the resulting bundles. The author adds—I have already advanced these broad facts as favourable to the view that nerve-fibres do not terminate in free ends, but form uninterrupted circuits.—*Archives of Medicine*, April, 1867, p. 325.

Fibrous and Fibro-cartilaginous Structures.—M. Sappey has presented to the Academy of Sciences of Paris the results of his various observations on the inter-articular fibro-cartilages. His preparations show the existence of vessels and nerves in their thickness; but in the so-called inter-articular fibro-cartilages, and, again, in the peri-articular fibro-cartilages, the disposition is different. The former only have vessels and nerves in their peripheral parts. Those of the knee are distinguished by the multiplicity of vessels and nerves which they receive; the vascular and nervous ramifications advance to their centre, or even a little beyond, but never reach their extreme edge. The minute arteries and veins, dividing and subdividing, traverse the interstices of the bundles of fibres of which the tissue is made up; both show frequent anastomoses enclosing the fibrous bundles. In the first part of their course both kinds of vessels have still their three coats. In the so-called periarticular fibro-cartilages the vessels are more abundantly distributed than in the preceding, and extend through all their thickness. These vessels, which enter by their outer aspect, resemble otherwise the distribution shown in the interarticular fibro-cartilages. The nerves in both follow generally the course of the minute arteries and veins; they are numerous and frequently anastomose. Their size, in some parts, exceeds that of the vessels; their exact termination is not described by the author. Thus, the observations of M. Sappey establish the fact that fibro-cartilages are provided with nervous ramifications, and that they anastomose extensively. Also that these structures have numerous arterial and venous subdivisions.

In the structure of ligaments the author has found all the elements of which collectively the fibro-cartilages are composed, but in different proportion and arrangement. They are remarkable for the multiplicity of the vessels ramifying in their thickness, which, penetrating the intervals of the fibrous bundles, subdivide and anastomose, so as to form a network to include each bundle. In the

capsular ligaments they form, even in the deepest layers, a capillary plexus but little less rich than that of the superficial layer of the dermis. All the ligaments have nerves, the divisions and subdivisions of which may be readily followed in the preparations of the author. These nerves generally accompany the arteries and veins, but some nervous divisions advance alone, followed only by vessels which represent their proper capillaries. The nerves in their course give off a long series of branches and twigs continually anastomosing with each other, so that in the midst of the vascular plexus, the network of nerves may be easily seen to be intermixed.

The structure of tendons shows exactly the same disposition of vessels and nerves as in the ligaments, but their number is less, and they are also less in size.

In the aponeuroses M. Sappey's preparations allow of no doubt that they are as rich in vessels as the tendons, and, like them, traversed by nerves.—*Robin's Journal de l'Anatomie*, etc., No. 2, 1867, pp. 212—215.

On the Doctrine of the Minute Structure of the Lung parenchyma in Mammals.—The investigations of M. Hirschmann concern the question whether, in the lung vesicles of mammals, flat muscular fibres are found or not. Moleschott and Gerlach say that it is so; other microscopists, on the other hand, deny it. One method of investigation consists in this, that the pulmonary artery of a fresh inflated lung should be injected with a blue mass, the lung then hardened in spirit, and fine superficial sections made. By this method it must stand for twenty-four to forty-eight hours in a solution of carmine, then for some time in 1 or 2 per cent. acetic acid, and after this in an acid glycerine of the same strength. In preparations so made one finds in the walls of the lung-vesicles evident nuclei, generally in pairs and parallel to the borders of the alveolæ and infundibula, which, by their placing and their form, can only be considered as flat muscular fibres. Near to them three kinds of nuclei may be distinguished in the lung vesicles: *a.* Nuclei of the capillary vessels, which are only one half, or even only one third, as large, and thicker in proportion; at the same time, a nucleolus may also well be distinguished. *b.* Connective tissue nuclei, which are smaller than the former, and at the same time appear of a rounded shape. *c.* Epithelial nuclei, which are larger than the connective tissue nuclei, and also are round and have granular contents.

By a second method of examination the author sought to bring into view, even in some degree isolated, the muscular fibres. The superficial sections of the injected and hardened lung were laid for eighteen or twenty-four hours in about 20 per cent. hydrochloric acid, then treated for a couple of hours with distilled water, and left in acid glycerine. If the investigation of these preparations be undertaken at a suitable time before the whole piece is saturated one finds the epithelium completely destroyed, the connective tissue changed into a fine granular detritus mass, and by the side of fine elastic tissue one recognises muscular fibres with muscle nuclei. These fibres are all clearly related to the walls of the lung vesicles;

the longish spindle-like shape and central nucleus characterising them.

By the first-mentioned method of investigation the author has arrived at the determination that in the lung vesicles a complete epithelium exists, and indeed a pavement epithelium, which completely uncovers the meshes of the capillaries, and sometimes covers even the capillaries also.—*Virchow's Archiv*, xxxvi, 3 p. 335, etc. *Schmidt's Jahrbücher*, No. 2, 1867.

Liver of the Vertebrata.—In two communications to the Academy of Sciences of Vienna Professor Ewald Hering has detailed the results of his investigations into the comparative anatomy of the organ, he says, "For some time past it has been felt to be necessary to trace the analogy of the minute anatomy of the mammalian liver with the other special secreting organs. All the different conceptions which aimed at this have nothing in common with mine but the explanation of the cells of the liver, which also I must consider as analogous to the epithelial cells of other glands, *e. g.* with the salivary cells. *If one speaks of the liver of the vertebrata in general* (as I have shown in reptiles already, and yet expect to show in fish and birds) *one must, of course, denote them as a reticularly arranged tubular gland; of the mammalian liver, however, in particular nothing at all of a proper tubular structure is to be seen. All the often-repeated accounts of a tubular structure of the mammalian liver I must point out as erroneous.* For instance, Beale's familiar representation, which is intended to demonstrate the tubular structure of the pig's liver, shows me plainly that a completely ruined preparation was the foundation of it. The injection mass is extravasated out of the gall-ducts, the liver-cells are distorted from their natural position, and to such an extent destroyed. Beale has also investigated the liver of cold-blooded vertebrata, and this may have misled the distinguished microscopist in supposing analogous circumstances for the mammalia.

The analogy between the structure of the liver and other secreting glands consists in this, that there, as here, peculiar gland-cells surround the opening of the gland-ducts, so that the latter are everywhere separated from the blood capillaries by intervening gland-cells. The liver is distinguished from the other glands in a remarkable way by the relatively great contact layer between blood vessels and gland epithelium. Even in the lower classes of the vertebrata we find each liver-cell in contact with the course of the circulation. In the mammalian liver each cell on several sides is in contact with the capillary system; here, also, the whole contact layer between the blood vessels and the parenchyma of the liver is incomparably more important. Similarly, also the number of the gall-ducts relatively to the number of the liver-cells is much greater, since each cell, not only on one side, but on many sides, is surrounded by the gall-ducts."—*Archiv für Mikroskopische Anatomie*, vol. iii, part i, p. 112.

Part II.—Pathological Micrology.

General Amyloid Degeneration, extending even to the Vessels of the Muscles.—E. Wagner, of Leipzig, notes a case disproving positively the primitive existence of amyloïd degeneration. All the symptoms of Bright's disease were present. At the autopsy, as a primitive affection, there was found a chronic and extensive, but non-tubercular, ulcer of the cœcum and the first portion of the colon. Microscopic examination showed an extensive amyloïd degeneration of the vessels of the kidney and of the parenchyma of the spleen; amyloïd degeneration of small arteries and numerous capillaries of the right rectus abdominis muscle, of small arteries and capiillaries of the meninges, the brain, the pancreas, the supra-renal capsules, of the intestinal mucous membrane, and of the lymphatic ganglia of the cæcum. The other muscles were not examined; the cardiac vessels were not degenerated. The stroma of the cortical substance of the kidney was over-abundant and strewed with little round and brilliant nuclei. The epithelium in great part had undergone fatty degeneration. In the canaliculi of the kidney were found cylinders, thin and hyaline. The muscular tissue of the heart had undergone a slight fatty degeneration. The liver showed, besides a little marked amyloïd degeneration of most of the arteries (the capillaries remained normal), a very marked fatty infiltration of its cells, and partially a considerable proliferation of nuclei in the interlobular connective tissue and along the peripheral capillaries.—*Archives Générales de Médecine*, March, 1867, p. 342, et seq.

Tubercle.—MM. Hérard and Cornil, in their published work on pulmonary phthisis have entered upon the study of the morbid anatomy of tubercular matter. They have established miscroscopically, and in a manner which they believe to be incontestable, that the yellow caseous masses, which were considered by Laënnec and those of his school, to be tubercular, are only lobular pneumonias in which the exudation products have undergone a granular fatty degeneration; and that there is no other tubercle than the miliary semi-transparent or opaque granular matter, a primordial lesion, specific and truly characteristic of the diathesis.—*Journal de Médecine et de Chirurgie pratiques*, March, 1867, p. 141.

Tubercular Meningitis.—Dr. H. C. Bastian, concerning "the so-called *grey granulations*," says, "I have fully satisfied myself that not only a few of the granulations of the pia mater have a vessel running through, or in direct connection with them, but that, save for occasional exceptions, a vessel may be shown to be so related to every one of them. It only requires that one should be familiar with the ordinary appearance and disposition of the perivascular sheaths around the arteries of the brain and pia mater, to convince oneself that these granulations are due to local circumscribed dilatations of the vascular sheath, which is irregularly distended by an excessive growth of the small nuclei or cells, that are usually met with in small quantities in the space between the sheath and the outer wall of the vessel."

"As to the nature of the nuclei or cells which are found so abundantly within the sheaths of the vessels," the author says, "they are round or oval in shape, and vary in size between 1-10,000″ and 1-3000″, the average diameter being 1-5000″. They are but slightly affected by dilute acetic acid, and usually present no nucleolus. They do, however, contain a number of minute granules, and their boundary-wall is proportionately thick and well defined."

The paper is of considerable length, and contains two illustrative cases.—*Edinburgh Medical Journal*, April, 1867, p. 1875, et seq.

Lymphatic Tumour of Bone.—Dr. Ranvier communicates to the Micrographic Society of Paris a case thus entitled (the tumour composed of the adenoïd tissue of His), and differing altogether from tubercular or syphilitic tumour of bone or cancer. He says he has never before met with such a case in his own large experience or in print, but that perhaps they are not rare, as the bones of leucemic patients have not been generally examined hitherto. The patient was a pale and thin girl of ten years old. She had symptoms of coxalgia on the right side. But the internal and external iliac fossæ of that side were found to be filled by excessively hard tumours. She got weaker, and her feet and legs became œdematous. She died in a year after she first became ill (previously she had had no illness). A careful post-mortem examination was made. The right iliac veins, of increased size, were comprised in a lardaceous tissue, which occupied the fossa and neighbouring parts: it could not be separated from the bone unless portions of the bone were at the same time removed. The external iliac fossa was similarly occupied. The mass was hard to cut, of fibro-cartilaginous appearance, and yellowish in parts. (Abnormal appearances, at first supposed to be tubercle, were found in the lungs, pleura, liver, &c.). The ilium was in some parts enormously increased in size. A yellowish viscous fluid escaped from it when cut into. It was so soft that the fluid could be squeezed out of it with the fingers in great quantity. The fluid contained cells and nuclei, such as are found in lymph. Colourless spherical cells of 0·012 to 0·02, and containing many nuclei; others similar, but smaller, one or many nucleated, little free nuclei of 0·005 to 0·007. The fluid did not give the characteristic reaction of mucine. (The head of the femur was similarly changed, but in a less degree; the new formation being in rounded islands in the bone. The pathological productions of the liver were characteristically leucemic, and suggested a correct diagnosis of those of the bones.) Portions of the iliac bone were placed in a solution of chromic acid, of $\frac{5}{1000}$, and after some days fine sections were made with a razor, and washed with a camel's-hair pencil. This was done to remove the greater number of the cells in the medullary spaces. Then there could be seen in them a reticulated stroma, the spaces of 0·05 to 0·2, some cells yet remaining in the spaces. The stroma fibres were cylindrical, of 0·004 to 0·003 in diameter; at their junction were evident knot-like thickenings. This pathological production was defined in its nature, especially by the existence in it of a special tissue, lymphatic (adenoïd of His), essentially made up of a

reticulated stroma filled with lymph elements. A special interest attaches to these leucemic productions in bone—in the white portions not yet opaque the lymphatic tissue was found in all its perfection. On one side there were red parts, and on the other a white and opaque tissue. In the red, or rose-coloured parts, the areolæ of the spongy tissue were augmented in size by gradual absorption of the bone structure, and were filled by lymphatic tissue in course of formation. This new formation was made up at the expense of the embryo cells accruing either from bone cells set free or from proliferated medullary cells. Some of these cells became fusiform or stellate, and anastomosing together formed a network of cells. The anastomotic prolongations of these at first appeared flattened or granular, their diameter soon diminished, and then they became cylindrical and homogeneous; at the same time the cells atrophied, and soon no more remained than the fibrils with the knots at their junction points. From their origin, the spaces of the stroma were filled with embryo cells, which differed very little from lymphatic cells and with some fatty cells.

The capillary vessels which furrowed the new tissue were themselves enveloped in a second reticulated tunic, whose fibrils were in connection with those of the stroma. Some of these vessels were filled with white globules, which were very readily distinguishable, even after the addition of chromic acid.

In the white and opaque portions, the brush did not remove the cells. But one found some of these atrophied, compressed together, and containing fine fatty granulations. In the midst of the semi-caseous mass, the reticulated stroma could be yet made out, but only in the very thinnest sections. The bone cells of the neighbouring trabeculæ did not contain fatty granulations.

Neither the other bones, nor yet the tumour, found in the lungs, were examined.—*Robin's Journal de l'Anatomie*, No. 2, 1867, p. 215—221.

Secondary Degenerations of the Spinal Marrow.—M. Bouchard characterises and distinguishes from primary lesions secondary degenerations which are, he says, consequent to all those nerve-tubes which have lost relation with those parts from which they derive their origin, and which so much influence their nutrition. They are not easily recognised. They are only found in the white substance. The alteration extends from the primary lesion upwards or downwards to a central or peripheral extremity. The consequences resemble those following section of nerves. The white tissue of the spinal marrow is much more easily compressed and injured than that of the nerves, but the work of destruction is more rapidly completed in the latter, the granular state exists longer, and if, as he believes, regeneration of medullary cords is possible, it proceeds more slowly than in the nerves. The author differs from L. Türck, and considers the change not of tissue, at first, at least, but of elements. In two cases of recent compression of the spinal marrow (in one two, and in the other six weeks), in examining freshly the degenerate fasciculi, he has found that a certain number of tubes showed manifest

segmentation; spaces more or less deep, divided the cylinder of myeline in the direction of its thickness. In some parts of the preparation, these fragments were infiltrated with fatty granular matter, others had been already partly destroyed by the retrogression, giving to the tubes a divided and, as it were, a mangled appearance. Independently of the fatty granular matter contained in the changed tubes, a great quantity was free among the elements, and, at certain points, it was collected into a mass, so as to constitute what is known as the granular bodies of Gluge.

In these cases, a thin section made perpendicularly to the axis, after maceration for some hours in alcohol, showed a great number of granular bodies interspersed as black bodies in the diseased fasciculi. All around these masses the tissue had a normal appearance, but the vessels showed on their surface a series of fatty molecular granules, or even a complete envelope, which rendered them black and opaque under the microscope. In older cases, in those of descending changes, in consequence of cerebral softening dating from six months, one or two years, he found in tearing up the tissue in the fresh state, that the nerve-tubes were healthy or slightly varicose, that they were separated from each other by a scanty amorphous matter, generally soft, transparent, as if gelatinous, containing abundant fatty granular matter, more or less numerous granular bodies, and also nuclei analogous to those which normally exist in the grey substance of nerve-centres, and which M. Robin has decribed as *myelocytes*. These nuclei were never very numerous, but they were so much the more abundant as the change was older. Sections made after maceration, for some hours, in alcohol, resembled those described. But those of the same specimens, after some weeks' maceration in a solution of chromic acid, showed a great difference between the healthy and diseased parts. Examined with a low power, the preparation in the changed parts showed clear striæ or transparent points dispersed among the tubes, which by their opacity, contrasted with the spaces occupied by the amorphous substance above described, and which alone freely transmitted light. In the normal parts the mutually compressed tubes everywhere prevented the passage of light, and gave to the section a sombre uniform tint.

Finally, in a case of compression of the spinal marrow, of thirteen years' duration, the examination in the fresh state showed a soft, transparent, abundant, amorphous matter, interspersed with *myelocytes*, in the diseased fasciculi; and in its thickness, few tubes, not granular, but varicose. The fatty molecular granules were not numerous, the granular bodies were only here and there, the vessels were almost normal. Fine sections made after maceration in chromic acid, and examined with a low power, showed at once the diseased parts, which contrasted very distinctly by their transparency with the healthy part, which preserved their uniform sombre tint. Besides, in the changed parts, black points showing the tubes, which had not been destroyed, in section, were seen disseminated in the thickness of the transparent amorphous substance. The connective substance which thus was substituted for the nerve-tubes had, by

its contractility, brought about a peculiar deformity of the marrow, consisting in a longitudinal depression of the surface at points the nearest to the altered portion of the fasciculus.

The author deduces three principal facts: 1st, the atheromatous appearance of the capillaries, and the formation of granular bodies in the degenerating tissue; 2nd, the change, and then the disappearance of a greater or less number of nerve-tubes; 3rd, the formation of a connective tissue which is substituted for the tubes.—*Archives Générales de Médecine*, March, 1866, pp. 272, 292.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

On some Errors in Military Surgery.—Under this title Professor Roser has contributed a series of critical observations on various points in military surgery.

1. *Extracting Balls.*—He takes Pirogoff's observations concerning Garibaldi—that had the presence of the ball been diagnosed more early he would not have preserved his foot—as a statement that is applicable to thousands of analagous cases. The routine practice of at once searching for balls he regards as highly mischievous. Their presence does not add to the danger, while the efforts so often fruitlessly made, consume most precious time and create great irritation of the parts, the ball, in fact, being far more easily detected and removed after the suppurative process has been set up around it. Interference is, however, called for when portions of clothing and other foreign bodies likely to excite putrefactive suppuration are carried in.

2. *Ligature of wounded Arteries.*—Dr. Roser protests against continuing the practice of tying the trunks of wounded arteries, notwithstanding that Guthrie so long since demonstrated its inefficacy, and that the correctness of his views has been admitted since by Nélaton, Stromeyer, Pirogoff, Legouest, Macleod, &c. The old objection to tying the ends of the vessel at the seat of injury, founded upon its supposed friability has, indeed, been abandoned. But Neudörfer opposes to the practice the difficulty of finding the vessel amidst the swollen and suffused parts, and the great irritation produced by the attempt, at all events when this is made some time after the injury. To this statement Dr. Roser opposes his own practice, having on several occasions applied the ligature in this manner with little difficulty and excellent results. A good knowledge of surgical anatomy is requisite, and as little cutting as possible should be resorted to. The further division of the fasciæ may be effected with a blunt hook; and a thorough clearing out of all coagula and blood, and tying of any bleeding branch will enable the ends of the vessel to be found.

3. *Débridement.*—The exaggerated frequency with which this was at one time resorted to under the influence of the French doctrine of inflammatory strangulation, has in its reaction led to the practice

being neglected in cases where it is urgently called for, as when collections of matter are confined behind aponeuroses, especially when they are in a putrefactive condition. The importance of liberating such putrid pus must be admitted in removing a source of surgical fever due to its absorption. When necessary, therefore, aponeuroses or muscles must be divided, or the apertures of the wound suitably enlarged. Another end gained by this procedure is relieving the deep-seated veins from intrafascial compression, which gives rise to œdema and stagnation of the local circulation. It is often a matter of difficulty to determine the proper moment for interference, but if we wait until the fascia is spontaneously perforated we shall often be too late. It is of great importance, when intrafascial compression of the deep-seated veins exists, to avoid applying gypsum or other firm bandages which compress the superficial veins, as otherwise gangrene and various ill consequences will follow this complete obstruction of the circulation. The opening once made, it must be kept patent by tents of *laminaria*, &c., and its disposition to become valvular guarded against by the introduction of the finger, or a catheter, &c. When the contents of the abscess are very foul antiseptic injections may be sparingly resorted to. As evacuating the abscess, not *débridement*, is our object, the large incisions formerly employed are not required.

4. *Conservative Treatment of Gunshot Fractures.*—Dr. Roser is of opinion that the publication of statistics of amputations by Malgaigne gave an immense impulse to improvement, and led to the foundation of conservative surgery. Opposed as this long was by distinguished surgeons in its application to gunshot fractures, all the experience of the wars during the last ten years has been recorded in its favour. The substitution of improved bandages for the complicated and tedious apparatus of Scultetus has been of great aid in this direction. The gypsum and other contentive bandages must not, however, be indiscriminately applied; for where the vessels have suffered much, or where there is effusion of blood, or subfascial collections of matter, the excessive compression from such bandages may prove very dangerous. One point in the use of apparatus of any kind has not been enough insisted upon, viz. the necessity, in the case of all bad fractures, of including the nearest joint within the sphere of protection offered by the apparatus in question. From want of attention to this many cases have done badly.

5. *Extraction of Splinters.*—The position laid down by the French surgeons that the splinters in gunshot fractures must be immediately extracted, is alike wrong in theory and practice. The asserted simplification of a wound by this procedure is entirely fallacious, far more irritation resulting from the interference than would be caused by the fragments removed. The difficulty of the extraction, and the great meddling with the parts necessary for its execution, only increase the disposition to decomposition from which so much mischief in these cases results. When suppuration has been set up the splinters become loosened in their connections with the soft parts, and also excite an irritation which they did not at first cause, and

which indicates the necessity of their removal. This must, however, not be done too hastily, for their presence is rather detrimental to the healing of the wound than dangerous to life; but, on the other hand, needless delay must be guarded against, or the wounds may be kept open for months or years. For the removal of deep-seated splinters, when vessels or nerves may be endangered, Dr. Roser is in the habit of only dividing the skin and, if necessary, the fasciæ with the knife, and then passing in a long, strong, crane-bill forceps. With this he dilates the parts forcibly, until the necessary space for the passage of the finger is secured.—*Berlin Klinische Wochenschrift*, 1867, Nos. 14, 16, 17, 18.

Subcutaneous Herniotomy.—In the number of this 'Review' for April, 1864 (p. 540), we described a new mode of performing the operation for strangulated hernia devised by Professor Max Langenbeck. In the present paper, he states that his three years additional experience has only served to convince him more of the great value and superiority of this which he calls the subcutaneous mode of operating. The forefinger is the chief agent, and by passing it in through an aperture in the skin, made at the side of the hernia, he finds that he can very frequently remove all obstacle to the reposition of the hernia, without any further use of the knife. Indeed, the latter is never required for removing the stricture, as he effects any division that may be required by means of the sharp edge of a ring attached to the finger, or, more lately, by the introduction of a convex director, having a broadish ridge sufficiently sharp to divide the fibrous, cartilaginous, and muscular tissues, the finger still performing the chief part in dilatation and reposition. Where such division is not required, that is in the majority of cases, the method is really a subcutaneous taxis. In three or four days the patient is on his legs again, and can bear his truss even before the aperture in the skin is healed. Even in complicated cases in which the old mode of operating may have to be resorted to, the introduction of the finger is a useful preliminary. Taken altogether, Professor Langenbeck is convinced that his mode of operating is well deserving the attention of the profession, by reason of the far less danger that attends it and the speed with which recovery takes place.—*Allg. Wien. Med. Zeit.*, 1867, No. 10.

Suture of the Flap after Extraction of Cataract.—Dr. Henry Williams, speaking from the experience of twenty-four cases, thinks very highly of this procedure. "It renders etherisation more applicable, as it obviates the danger of loss of the vitreous humour after the operation, in case emesis should occur; and the patient being thus impassive, the operator is enabled to do with deliberation and care whatever may be requisite in removing complications which may arise in the course of an operation, without feeling that he runs the risk of contusing the iris or losing a portion of the vitreous humour during sudden involuntary movements of the eye. The edges of the wound being retained in close apposition, union by primary intention is rendered much more certain. The puffy, swollen state of the margin of the flap, which renders the healing process difficult and uncertain, is thus avoided, and the eye at once

resumes almost its normal condition. It nearly obviates all risk of spontaneous prolapsus of the iris; and by affecting a speedy re-establishment of the anterior chamber, it admits of the free use of atropia without fear that prolapsus may ensue, thus allowing continued dilatation of the pupil to be kept up, and lessening the risk of irritation of the iris from unremoved fragments of lens or torn edges of capsule, or from proliferous degeneration of the intra-capsular cells. It permits of early and frequent inspection of the eye, and the prompt discovery of any morbid phenomenon, so that timely recourse may be had to appropriate remedies. It much abbreviates the time of rigorous confinement, and shortens the entire period of convalescence."

Dr. Williams places a single point of suture at the apex of the flap of the cornea, while the patient is still under choloroform, employing a straight needle, less than a fourth of an inch long, with a flat cutting point. Threaded with the finest silk, this is seized with strong forceps and passed through the edges of the wound, which are held with very delicate toothed forceps. In most cases the suture has been left to come away of itself; and, though usually becoming detached within a few days, it has in some instances remained *in situ* for seven weeks without giving rise to more than trivial irritation. The author believes, however, that the best plan is to remove it, under anæsthesia, within a week.—*New York Medical Journal*, February.

Treatment of Varicose Veins by the Galvanic Current.—Dr. Jasche, of Nishni-Nowgorod states that having employed the various injections for varicose veins recommended of late with unsatisfactory results, he determined upon resorting to galvanism, in order to effect the necessary coagulation. By means of from four to six elements of Daniell's battery, he was enabled to produce a pretty thick coagulum, which firmly adhered to the walls of the vessel acted upon. A needle connected with the positive pole was passed through the vein when distended by pressure, either in the longitudinal, but better in the transverse direction, the negative pole being usually brought into connection with the tongue. The current was kept up for from five to fifteen minutes. On account of the numerous anastomoses the operation had to be repeated at different spots; and excellent results were often obtained when the dilated vein was closed by coagula at both its upper and lower ends, or two such obstructions were produced at a short distance from each other. The punctures may also be advantageously made at the spot where the greatest dilatation has taken place, as a larger coagulum is produced than in the channel of the vessel, and it is not easily carried away. The little inflammation that results from the procedure is easily subdued; and in the course of a few weeks the author's ten patients were able to enter upon military duties.—*St. Petersburg Med. Zeitsch*, 1867, No. 1.

On the Spontaneous Rupture of Veins.—Professor Legouest observes that the rupture of veins under the influence of violence,

or of a diseased or varicose state of the vessels, is a fact familiar to surgeons, but another form of rupture has not attracted any special attention, and is not described in our treatises. Of this last he has met with three cases in his own practice, and refers to another described by Moulinié, of Bordeaux, under the name of "sanguineous œdema." In his first case, effusion of blood, occurring without obvious cause in a man under 50, in robust health, doubled the size of the leg from the knee downwards, and the patient sank next day. All the organs were found to be sound. The leg was completely infiltrated with blood, but there was no collection of this, and the deep-seated veins were intact. The superficial veins were lost amidst infiltration. 2. A military surgeon observed an ecchymotic tumefaction take possession of, first, a finger of his right hand, and then, eight or ten days afterwards, the same occurrence took place in a finger of the left hand. The swelling in this last was considerable, but gradually abated in a few days, and no cause for its production could be discovered. 3. A young soldier, without obvious cause, found his hand the seat of a considerable ecchymotic tumefaction, accompanied by much pain. A fortnight elapsed before the hand recovered its normal condition. These cases do not resemble scorbutic and spontaneous ecchymosis, in which there is neither the same pain nor amount of blood observed. Still these accidents may be regarded as phenomena of the same nature as spontaneous ecchymosis, with the difference that the vessels giving rise to the effusion of blood are of a larger calibre than that of the capillaries.—*Archives Générales*, May.

On Stricture at the Anterior Portion of the Urethra.—The object of M. Folet's interesting paper is to demonstrate the accuracy of the statement recently made by M. Verneuil, that the general opinion that the ordinary seat of organic stricture is at the bulbous portion of the urethra is erroneous, the obstacle in the great bulk of cases really existing at six or seven centimetres distance from the meatus, a second spasmodic stricture, symptomatic only of the former, also existing in the membranous portion. Ten cases, observed in the wards of M. Verneuil, supply the material for the present essay, the author having also assembled together the opinions of the chief writers on stricture. The following are his conclusions:

"1. Spontaneous, fibrous, organic stricture is of frequent occurrence in the spongy portion of the anterior part of the urethra, and is frequently misunderstood. 2. Organic strictures of the bulbo-membraneous region, stated to be so frequent, are rare. 3. In *all* cases of anterior stricture, a second obstacle exists at thirteen centimetres distance from the meatus, at the commencement of the muscular region at the entrance of the urethro-vesical neck. It is this deeper-seated obstruction which has given rise to the classic opinion that the bulbo-membranous portion is the usual seat of stricture. 4. The calibre of the anterior stricture is invariable, or is only susceptible of slow and gradual dilatation. But the diameter of the deeper obstacle undergoes the most sudden and capricious variations;

for while in the morning it may be traversed with facility, by the evening some source of irritation may have rendered it completely impassible. 5. This second obstacle is due to muscular spasm, the anterior stricture, of which it is symptomatic, often being but slightly narrow, and of itself incapable of notably impeding the passage of the urine. It is the secondary spasm which is the efficient cause of the dysuria, constituting a serious obstacle, which is sometimes invincible by catheterism. 6. In the rare cases in which the fibrous stricture is located at the bulb, the secondary spasmodic stricture is none the less present, situated immediately behind the former. The fibrous coarctation is often broad enough to allow the entrance of the point of the bougie, it being always the spasm which effectually closes the entrance of the bladder.

"If the ideas now advanced are accepted, it is obvious that the mode of treatment of stricture must be modified. The employment of agents capable of relieving urethral spasm must be more largely resorted to; and divulsion, or the forced dilatation of Rigaud, Pereyre, and Holt becomes a logical procedure."—*Archives Générales*, April.

Osteomyelitis.—Dr. Lidell of the United States Volunteers has just published a highly interesting essay upon this subject, which he read at the New York Academy. We have only space to notice what he says upon the consequences and complications of the disease.

1. Osteomyelitis, even when acute, not unfrequently terminates in resolution. But the acute variety much more frequently degenerates into the chronic form, which is, for the most part, susceptible of relief by surgical means. 2. It often occasions central necrosis, the laminæ of the shafts of the bone lying next the marrow suffering from an inadequate supply of blood, owing to the morbid condition or destruction of the medullary tissue. 3. The inflammatory process may spread from the marrow to the osseous system and periosteum, and thus occasion necrosis of the whole thickness of the bone. 4. Osteomyelitis not unfrequently produces pyarthrosis. Inflammation of the medullary tissue exhibits a strong tendency to travel upwards from one bone to another towards the root of the limb, and in this way invades the intervening articulations. It may occur simultaneously in more than one bone. 6. It very often causes the development of new osseous tissue. 7. It frequently induces abscesses in the neighbouring parts. 8. It not unfrequently causes the blood to coagulate in the veins leading from the affected parts, producing thrombi. 9. It often causes pyæmia. This is, indeed, by far the most frequent cause of death in fatal cases, eighteen out of thirty deaths being due to it. Hence it is believed that pyæmia is the form of systemic intoxication which is produced by suppurative inflammation of the marrow. Clinical observation has also abundantly shown that purulent infection occurs but seldom in surgical cases, comparatively speaking, unless the osseous tissue is injured. 10. Osteomyelitis is sometimes complicated with leucæmia. 11. It not unfrequently causes death by exhaustion.

"But the closeness of the relationship which exists between sup-

purative inflammation of the medullary tissue of bone and pyæmia is very remarkable. No such relationship obtains between suppurative inflammation of any other tissue in the whole body and that disease. It is but seldom that we meet with pyæmia in a severe form, in military practice, unless it has followed a traumatic lesion of the osseous tissue, or a surgical operation rendered necessary by such lesion. . . . . In answer to this question it has been suggested that the patulous condition of the veins in the osseous tissue, and in the marrow itself, occasions the difference."—*New York Medical Journal*, March.

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Abdomen.—Lidell. Rupture of the Abdominal and Pelvic Viscera, especially the Bladder. (Amer. Journ. Med. Science, April.)—Flechsig. Case of Recovery after Penetrating Wound of Abdomen, with Protrusion of the whole of the Intestines. (Zeit. für Med. und Chir., 1867, No. 1.)

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outgrowth from its outer surface, and which he proposes to term *myringitis villosa.*)—Duplay. Critical Account of Recent Publications on the Ear. (Archives Générales, April.)

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THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

OCTOBER, 1867.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *London Pauperism amongst Jews and Christians. An Inquiry into the Principles and Practice of Out-door Relief in the Metropolis, and the Result upon the Moral and Physicial Condition of the Pauper Class.* By J. H. Stallard, M.B. Lond. London. 1867. Pp. 527.
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12. *The Management of Workhouses.* By Samuel W. North, M.R.C.S. A Paper read at the Meeting of the National Association for the Promotion of Social Science, 1866.
13. *Report of the Committee for the Prevention of Venereal Diseases.* Harveian Medical Society of London, 1867.

It is difficult to conceive of a community arrogating to itself the title of a Christian community in which the claims of its destitute members to the means of existence are altogether ignored. Religion in its primary and most comprehensive sense as "*quod religit*" must bind to something, and in so far as it does so it destroys the freedom of the individual who professes it to do or not to do as he chooses. And among all the obligations of the Christian system there is none more characteristic than that which, uniting all its adherents into a brotherhood, recognises the necessities of one as a claim upon the sympathy of all. It adopts the heathen system of hospitality, invests it with a sanction peculiarly its own, and, prescribing no definite limit to its exercise, inculcates instead the cultivation of a Divine and universal love. Its teaching is, "Bear ye one another's burthens, and so fulfil the law of Christ."

It is thus that the Christian religion binds the individual. How does it bind a Christian state?—for surely its profession must bind a Christian state; surely it is the duty of a Christian community to see that no individual laxity of practice, no individual lack of Christian principle, shall interfere with the performance of a duty which would still remain a duty were Christianity banished from the earth. But in virtue of its profession it is bound to do something more than merely to feed the hungry, to clothe the naked, and to visit and to cure the sick; it is bound also to carry into this common and national work something at least of the peculiar motives and principles by which it declares itself to be guided, and to exhibit towards the necessitous, in an obvious degree, a sympathy with the sufferings of our common nature.

From a very remote period of our history the support of the poor as a religious duty was acknowledged by the State. Horne, in his "Mirror of Justice," records among the ordinances of our kings prior to the Norman Conquest the provision that "the poor should be sustained by parsons, rectors, and the parishioners, so that none of them should die for want of sustenance," and a portion—a third part—of the tithes of the Church was devoted to this object. As monastic institutions increased and the rectorial tithes became absorbed by the religious orders, the latter accepted with them the burden of providing for the poor, and they, together with various institutions endowed for purposes of charity, constituted the chief resource of those who were unable to sustain themselves. This arrangement continued up to the period of the dissolution of monasteries in the 27th year of Henry VIII. No other or different national system of relief was attempted—no other seemed to be necessary. Nolan[1] remarks –

"Such abstinence from regulation on the part of our civil government is no slight testimony that the clergy devoted a sufficient portion of their immense property to maintain the poor. * * * But a more direct proof of the fidelity with which the clergy administered the trust reposed in them arises from observing that the first important legislative attempt to provide for the impotent poor was made in the same year when the property of so many religious houses was vested in the crown."

We, in this England of the nineteenth century, blinded as we often are by party zeal, are very apt to forget that the monastic system with all its faults had some excellent points about it, that, amidst the wars and political tumults of less settled times, it was the rallying point of very much that was good, the salt which preserved our social organisation from total corruption. But Henry made no attempt to dissociate the relief of the poor from the obligations of religion. Begging was restricted, but it was ordered (27 Henry VIII, c. xxv) that alms should be collected after Divine Service on Sundays, and on one Sunday in the year the names of persons willing to give for the ensuing year were taken down in writing, the minister being enjoined to exhort the people to liberal contributions. Refusals to give were reported to the bishop, and on his requisition being of no avail, the Justices in Sessions were empowered to assess the obstinate individuals what they thought reasonable. And so it went on until the 43rd year of the reign of Elizabeth, when a poor-*rate* was first established, and the payment of it made compulsory by statute-law. The ecclesiastical

[1] 'Treatise on the Laws for the Relief and Settlement of the Poor,' 1825, vol. i, p. 4.

division of the kingdom into parishes was retained, and the state, holding to the old principle that each parish ought to maintain its own poor, relieved the Church of the duty of collecting the necessary sum as alms, and also of the distribution of it, vesting both offices in the hands of laymen, having still, however, a nominal association with the ecclesiastical establishment. It was for the churchwardens and overseers to make the needful rate and to see to the support of the poor out of it. Since this time every change that has been made has been further and further in the direction of secularising the relief of the poor, as a national system, until at last it has come to this, that the administration of the laws to this end throughout the kingdom is vested in a secular board of commissioners nominated by the crown, and the distribution of relief itself in the hands of lay "Guardians of the Poor" selected by the ratepayers in their several parishes or unions of parishes. The ancient association of the relief of the poor with the notion of religious obligation, so far as the State is concerned, has been at an end for very many years, and unhappily, whatever other advantage may have been derived from the alteration, it has borne bitter fruit to those most concerned. If the monks erred on the side of too profuse liberality, as it is probable they did, it was an error which the excellence of their motives might well condone: the same fault certainly cannot be laid at the door of their lay substitutes in the present day, whose grand object appears to be to render the tax upon the ratepayers as light as possible. Paid agency has taken the place almost wholly of individual voluntary labour in the investigation of the circumstances of applicants for relief, and this too has borne its fruit in the loss of that individual sympathy which is certain to be excited in a well-constituted mind by the unaccustomed sight of a brother in distress. The poor-rate is claimed as a right and paid as a tax, is regarded on all hands as a disagreeable impost, and its distribution is effected with little of the sympathy which ought to accompany the relief of material necessity, and with little, if any, variation of method in accordance with the peculiarities of individual cases of distress. It has been attempted to do by the paid agency of a few hirelings what is strictly due as the voluntary labour of many, who compound both for duty and labour, as they imagine, by the money payment demanded from them; and the attempt has miserably failed. Bad indeed would be the case of the poor had not the Church as an establishment, and nonconformist congregations everywhere, still held it their duty as Christian communities to seek out the distressed in their own neighbourhoods, and to minister to their necessities by their own voluntary alms. From these

sympathy has always been obtainable as a concomitant of material relief; to say nothing of those noble foundations for the relief of the sick and destitute which the charity and munificence of our own and former ages have spread widely over our land.

It has been necessary to premise this brief historical outline in order to define the relative position in which the state and voluntary organisation strictly stand to each other as respects the relief of destitution. The state long ago took upon itself a certain work, formerly, and we say naturally, devolving upon the Church, and substituted a compulsory tax for a voluntary offering. It undertook to collect the necessary revenue in its own way, and it was bound to take care that the poor were no losers by the change. We assume that, under the circumcumstances of the time, this alteration of system was right and wise and prudent. The state undertook the Church's duty; all of it as regards the poor, except the administration of purely spiritual assistance, and we say it was bound to perform it all. It had no right to consider itself from that time forth as merely supplementing the voluntary efforts of Christian people. How has the system worked? How was it working only a year or two ago? Has the relief of the poor been sufficient in amount? Has it been properly discriminating in its character? Has it been accompanied by any manifestation of sympathy with distress, or with that consideration for the feelings of the sufferers that ought to be associated with material relief?

Some of the books and pamphlets at the head of the article will enable us to give an answer to these questions as regards the metropolis.

1. Has the relief been adequate in amount? Dr. Stallard supplies us with a table (p. 245), showing the number of persons relieved on the 1st of January, 1858 and 1865, with the weekly relief per head, the average of which was 1*s.* 2½*d.* The only two unions, namely, the City of London, where the average, in 1865, was 2*s.* 4*d.*, and Hampstead, where it was 2*s.* 2*d.*, was the relief per head in excess of two shillings. He gives a group of twelve parishes, where it varied from 1*s.* 11½*d.* to 1*s.* 6*d.*, the average being 1*s.* 8¼*d.*; of eleven where it varied from 1*s.* 5*d.* to 1*s.*, the average being 1*s.* 3*d.*; and of fifteen where it varied from 11½*d.* to 8*d.*, the average being 10¾*d.* The last includes Whitechapel, St. George the Martyr, Mile End, Bethnal Green, and others where pauperism is most deep and congregates most. No one who knows anything of London—of the cost of lodging, food, and clothing—can regard the highest of these amounts as a sufficient sum to provide all that is requisite. But it may be said that it was not intended to provide all that is requisite, that, as relief to distress, it was sufficient as an aid in all cases

relieved out of doors, that other means of subsistence were available in most instances, and that these, with the very proper contributions of friends and relations, was sufficient for the purpose. Looking at the inequalities of relief given in different parishes, one would say that *primâ facie* this is an improbable assumption; for in the wealthiest community the relief is largest in amount, and smallest in the poorest community. Let us take, then, one of the poorest parishes (St. George the Martyr), and test the question by what took place there. Dr. Stallard tells us that—

"During the week ending the 23rd June, 1866, 80 families were relieved on account of the sickness of the male parent, who was under sixty years of age in every case. On these 80 males depended 58 wives and 209 children for support. Every one of these families was, of course, in a state of what is called destitution, the only income, other than that supplied by the parish, being the earnings of the children, the wife being required to act as nurse. The amount of relief was nothing in money, and £5 9*s.* 2½*d.* in meat, bread, and groceries, or 3¾*d.* per head. In the same week 19 families were relieved on account of the sickness of the wife, or of one or more of the children. In these cases the father is also in a state of destitution, for if in work he would not be relieved, and the amount given to the 104 individuals of which the family consisted was £2 13*s.* 3*d.*, all in bread, meat, grocery, or wine, or three farthings per day for each individual."

Even if in these cases the highest average sum per head given in any parish had been allowed, namely, 2*s.* 4*d.* per week, it would only have amounted to 4*d.* per day. But in extenuation of such niggardliness, it may be replied that, had the amount been insufficient, the doors of the workhouse were open, and that there these poor people would have been better off, the parish supplying all their wants liberally, lodging, food, raiment, and medical care and comforts. We shall see presently if this is true.

2. Has the relief been of a sufficiently discriminating character? That is to say, Have cases as a rule been investigated so thoroughly as to permit of the discovery of the mode and character of relief best fitted to the circumstances of each, and best calculated not only to meet the existent emergency, but to prevent the recurrence of the event in the future? Persons applying for parochial relief when in destitution are no doubt classified. They are classified according to the standard set up by the Poor Law Board. Of course, in dealing with a large mass of poverty, some classification is needed; and that adopted cannot be said to be essentially vicious in its character, so far as it goes. It separates the able-bodied from those who

are not able-bodied, as two classes of persons for whom a different species of relief is demanded.

Dr. Stallard has made an attempt to exhibit for the entire metropolis something like the proportion of persons of different classes who were in the receipt of parochial outdoor relief on the 1st of January, 1866. Out of 74,014 persons estimated to be in receipt of such relief, the adult males relieved on account of want of work, or as cases of urgent necessity, amount to only 530 persons, while those relieved on account of their own sickness, or the sickness of some of their family, amount to 2869 persons. Those relieved on account of sickness then were more than five times as numerous as those relieved because they were unable to obtain work. The wives and children of these classes, relieved at the same time of course as the father whose labour supported them, amounted to 12,173 persons. All these together give a total of 15,572 persons, of whom at least five sixths were relieved on account of sickness in some member of the family. Then we find 23,150 persons classed as not able-bodied, of whom 5700 were males, and 17,450 females, and with them 3320 children who were dependent upon them. Another large class is formed of widows, of whom there are estimated to have been 7450, with 18,300 children dependent upon them; together, 25,750 persons in a position of the utmost difficulty: for, however strong such poor widows may be, they constitute a class quite unable to maintain their offspring by their own efforts, still less to afford them a proper education. The widows and children thus form one third of the whole mass of pauperism. The remaining classes are constituted by 740 orphans, mothers with their illegitimate children, single women, deserted women and their children, and the wives and children of soldiers, sailors, or men in prison. The result of the whole classification seems to be this: that, out of the 74,014 relieved, 39,503 persons were destitute on account of sickness, or the death (mostly the result of sickness) of the father of the family. We put out of the question the 23,150 classed as not able-bodied and their children, although these, being persons over sixty years of age, must mostly be believed to be more or less infirm, and still we have more than 53 per cent. destitute as the result of sickness alone—destitute from this single cause. Adding the not able-bodied, we get a total of 62,653, or six-sevenths of the whole number relieved, who are more or less disabled from providing for themselves from sickness or infirmity. Now, we ask, are the special circumstances of each of these cases properly inquired into, and is the relief given such as is most appropriate to each case? About those able to work there is and can be no doubt. Work is offered them, and the Christian rule enforced, that,

"if a man will not work, neither shall he eat;" at any rate the relief is very properly afforded in the most repulsive form. But we specially ask our question as to those disabled from obtaining their livelihood on account of sickness or its results; and we say that, as things are managed now, it is a physical impossibility that the cases should be properly inquired into, and the cases relieved in the most appropriate manner.

Dr. Stallard informs us that 100,000 out-door paupers in London to be found under relief (the number is not quite so large as this) are practically left in the charge of 100 relieving officers, whose duty it is to investigate the cases and to keep the accounts. This would give 1000 cases (say rather between 700 and 800 cases) in charge of one man. In some parishes it would be a larger, in others a smaller number. The board of guardians rarely meet anywhere more than once a week, and is necessarily therefore guided by the opinion of the relieving officer; the members of the board do not visit and inquire into cases themselves: all they do is to determine the amount and kind of the relief to be given.

Now, Dr. Stallard is quite correct when he says that under such a system a satisfactory inquiry is impossible, and that the relief given cannot be discriminating. Nowhere throughout London is a continuous history of an indigent family to be seen, nowhere can we see and trace the gradual descent in the social scale which over and over again occurs where sickness or other misfortune overtakes an honest family. At the Jewish Board of Relief, although each member does his own share of the work of private inquiry, and two investigating officers are further employed, no more than twenty cases can be considered properly, and relieved within the course of one hour, notwithstanding that the majority only require a renewal of relief. He mentions that, at a board meeting at Bethnal Green, 390 applicants for relief were dismissed in two hours and a half, and publishes a letter from a guardian of St. Paul's, Covent Garden, to show that this poor parish does not stand alone in such a summary mode of procedure.

At a meeting of the guardians of the Strand Union, which this gentleman attended, during exactly thirty-eight minutes, 43 cases of applicants were read out only to be confirmed—a mere formal proceeding; but, in addition, 34 cases appeared personally, were questioned and adjudicated on by the board through their chairman. The results of this offhand method of dealing with applicants in individual cases may be observed in some of the instances detailed at length by Dr. Stallard, who places in contrast with them the results arrived at by the more careful plan adopted in the relief of the Jewish poor of the metropolis.

In the one case we see the sickness of the head of a respectable family followed by distress, and then, as one article of clothing and furniture after another is parted with, by each gradation of destitution, until nothing remains but the breaking up of all home associations, and the degradation inseparable from the necessity of taking refuge, as a last resource, in the workhouse; in the other case we see the temporary difficulty tided over, the ties of family and home preserved intact, and even a condition produced by the industrial education of elder children superior to that existent prior to the advent of trouble. And let us add, although this is not a point we are engaged in considering, at a less cost in the end.

3. Has the administration of relief been given accompanied by the exhibition of sympathy? We shall say very little on this head, leaving facts such as we shall have to detail presently to speak for themselves. No doubt there are to be found, among guardians of the poor and among relieving officers, men with feelings as delicate as among other people, kind-hearted, good, and considerate men, when acting in a private capacity; men who would treat an honest, poor man with the respect he deserves, and the kindness which suffering ever claims as its due. But, in the office and the board room, even these men seem to undergo a metamorphosis. It is not the *poor* who claim kindness then, it is the *pauper* who asks assistance from the public rates. A distinction is made where there is really no difference; and, as Dr. Stallard says—

"Pauperism is regarded as a species of moral sore, incurable social disease, aggravated by kindness and fostered by generosity, and from its nature, only susceptible of palliation by harsh and repressive measures. The true pauper is held up by Poor-law officials as a contemptible animal, endowed with the ignorance and passion of savage life united with the meanness and vices of modern civilisation; he makes no effort to raise himself or his children from the position in which they are contented to exist, and his delight is to live in idleness upon the rater. Upon the pauper sympathy is thrown away; he gets more than he deserves already. The evils under which he suffers are self-imposed; it is no use trying to help those who will not help themselves. This is the excuse for Guardians of the Poor." —p. 291.

And in a very small minority of cases it is, perhaps, a good and valid excuse, but even in this the question may be honestly asked, whose fault it is that, as a body, the poorer classes of the community are thus degraded; whether the community at large is not in fault rather than the poor themselves.

"The low standard of public ethics as regards the poor is the main

cause of scanty relief; it is that which calls upon the widow for superhuman effort; leaves sickness to pine away in neglect; drives age to despair and suicide, and claims its victims of starvation in the metropolis week by week. It is that which makes the poor that which all represent them to be; for if no respect be paid them by the rich, how shall it be hoped that they will respect themselves? If there be so little interest in their welfare, or care for their material comfort, no attempt made to improve their education, or raise them in the scale of humanity,—if they are reared in sickness and fed on garbage,—if, broken by debility, they are turned helplessly on the world without a trade or the means of getting bread,—if there be no guarantee that genuine distress shall be fairly and adequately relieved,—if, in fact, the rich shirk the duty of helping to bear the burdens of the poor and so force them to lying as the best means of obtaining scanty help, can we wonder that the poor impose, and that their moral and physical condition is degenerating day by day? * * As individuals we ought to be ashamed of our apathy; and as a nation we are utterly and hopelessly disgraced until the standard of public humanity be greatly raised."—p. 297.

This is strong language, but we very much fear there is more or less warrant for it.

We may now quit, for the present, the subject of relief of the out-door poor, and follow the pauper into the parish workhouse. Is he much better off there? The home is broken up, the husband and wife drafted off into separate wards, the children sent to the district school, and the members of the family become units in the mass of persons to which they are severally added.

In this country it is no uncommon thing for great abuses to exist until some startling event happens to bring them prominently under public observation; and even then matters soon fall back into the customary routine, unless some public-spirited individuals make it their business to raise and keep on foot some agitation upon the subject. At the end of the year 1864 a navvy, named Timothy Daly, died in St. Bartholomew's Hospital the day after his admission from a poor residence in the Holborn Union. At the time of his admission he had large bed sores, pericarditis, and pleuropneumonia. It appears that these bed sores had formed while he was resident for six weeks in the Holborn Union Workhouse, and, as a matter of course, first the surgeon and then the Poor Law guardians were blamed. 'The Times' and other newspapers took up the case, and were profuse in their condemnation. An official inquiry, however, absolved the surgeon from blame, and terminated by recommending that his salary should be increased, that drugs should be provided at the expense of the board of guardians, and that salaried nurses should be appointed to the infirmary. The pub-

licity given to this case roused public attention, and it was kept from flagging by the report of another case of manifest neglect of a poor man of the name of Richard Gibson, at the St. Giles's Workhouse. It is to the credit of our own profession that we were the first to institute an inquiry into the truth of the allegations put forward of neglect and improper management of the poor, and especially the sick poor in the metropolitan workhouses. Dr. Stallard informs us that he took the matter up very shortly after the occurrence of Daly's case, and in May, having gathered a considerable amount of information, proceeded to put it into form for the consideration of Mr. Villiers. In April, it appears that the editor of the 'Lancet' also conceived the plan of instituting a full inquiry into the same matter, and a commission was organised, consisting of Mr. E. Hart, Dr. Carr, and Dr. Austin, whose reports were commenced in that journal on the 1st of July. In the meanwhile, Dr. Stallard had placed himself in communication with the Ladies' Workhouse Visiting Society, and a deputation went to Mr. Villiers, being introduced by Earl Devon, and attended by a number of peers and members of parliament, to lay before him the main evils of the workhouse infirmaries, and to urge upon him the importance of providing for their proper medical inspection. The work was now fairly inaugurated. On the 1st of February, 1866, the "Association for the Improvement of the Infirmaries of Workhouses" was constituted, several well-known peers and members of parliament, medical men and philanthropic laymen, being united together as its executive committee, and a public meeting was held in Willis's Rooms, on the 3rd of March, at which the Archbishop of York took the chair. Resolutions were passed condemnatory of the present system of managing the sick in the workhouse infirmaries, proposing their consolidation, and the placing of them under an uniform system of management, and an influential deputation was appointed with a view to induce the president of the Poor Law Board to bring a bill into Parliament for the attainment of these objects. This movement, thus nobly supported, was such as no government department could resist; nor do we believe there was the least wish to resist it. Our own impression is that the government was only too happy that the faults of the present system had been thus brought before them, and that the opportunity afforded by so important an expression of public opinion had presented itself for the carrying out of very necessary amendments. On the 3rd of April, then, Mr. Farnall and Dr. Edward Smith were directed to institute a searching inquiry into the existing arrangements for the care of the sick, and they presented their reports separately towards the middle

of the month of June. On the whole, these reports confirm what had previously been brought to light by the independent medical investigation alluded to, and in considering them we shall not separate the one from the other. In the month of July last year another association was formed. It was felt by the medical officers acting in the relief of the sick in the metropolitan unions that matters relating not only to the treatment of the sick poor within the parish infirmaries, but to the out-door medical arrangements also had long been in an unsatisfactory condition, and that any legislation would be imperfect that did not embrace medical relief both within and outside the workhouse doors. These gentlemen have long been urging upon the state the grievances to which they are subject, in the execution of their very onerous duties, and the opportunity now offered of expressing them again was not to be lost. The objects of the "Metropolitan Poor Law Medical Officers' Association" are mainly the following:—

1. To obtain for the sick poor chargeable to the state the advantages, in respect of nursing, dietaries, supply of medicines, medical and surgical appliances, cubic and floor space, ventilation, light, and general treatment, enjoyed by the sick poor in hospitals and medical institutions supported by voluntary contributions. In this respect, then, the objects are the same as those of the Workhouse Infirmary Association. 2. To obtain life appointments for all Poor Law medical officers, and entire instead of partial payment of salaries from the Consolidated Fund; and thus to secure, at once, due recognition of their position as state servants, and greater freedom and independence of action. 3. To provide a basis for consultation and united action, in all cases of difficulty arising out of the nature and in the discharge of their duties. 4. To obtain an authoritative decision upon all disputed questions relating to duties and extra medical fees. 5. To obtain from the local authorities the provision of all medicine and appliances prescribed for the sick poor, and the employment and payment by the same of qualified dispensers.

The agitation originating in Daly's and Gibson's cases has then, it will be seen, resulted in opening out a vast field of investigation and of complaints calling for remedy; and these complaints have mostly to do with the management of the sick. The latter fact cannot be wondered at. We have already shown that, the relief demanded from the parish rates arises mainly out of the attacks of sickness, or the advent of infirmity. Among the families seeking out-door relief a very large proportion has sickness in some member, and the cases are under the care of the out-door medical officers of the several unions,

who, in the majority of instances, are paid a most incommensurate salary, and commonly have, besides the labour of visting the sick, to find at their own cost all the medicine they prescribe. In the workhouses also, as out of doors, the greater part by far of the inmates are either sick or infirm. Among the 23,500 inmates at the time of the official inspection by Mr. Farnall and Dr. E. Smith there were only 1850 able-bodied persons. Three thousand were children, all the rest were sick persons, old and infirm persons, or imbeciles and idiots. Putting the children aside, not one tenth of the inmates were able-bodied persons. It is important to keep this in mind, because it was not always so. Dr. Smith says—

"In the earlier history of Poor-law administration a very large proportion of the inmates of workhouses were persons under sixty years of age, and without such infirmity as might have disabled them from earning their living, whilst the aged and the sick were more commonly allowed to remain at their own homes. Hence the greater part of the arrangements had reference to the able-bodied class, and the aim was to make the workhouse deterrent, with a view to exclude these as much as possible. This idea ran through the whole system of administration, including the diet, the bedding and furniture, the discipline and the labour; and even to this day it is predominant in the minds of many guardians, whether in town or country. * * * The essential part of the establishment was therefore the main building with its day rooms and bed rooms, kitchen and apartments for the officers, and the incidental part was the sick ward for the treatment of occasional cases. Hence the main building was large and the infirmary small; and since it was desirable that the inmates generally should be lodged in large wards, whilst the occasional cases of sickness should be kept separate, no more provision was made for sick cases than the occasion required, and the rooms were small. Moreover, as the occupation of the sick-room would be temporary, and the occupants few, scarcely any attention was given to such conveniences as lavatories, baths, water-closets, and kitchens." —p. 33.

At the present time—

"The workhouses are the homes for the aged and infirm, a last refuge for destitute sick persons, places where a great number of women give birth to illegitimate children, and a temporary shelter for tramps and very destitute persons suffering from some disease. They are not on the one hand workhouses in the sense of institutions where the inmates are able to work or where labour is exacted; neither, on the other, are they hospitals where all are sick and temporarily received and treated, but asylums where the aged and infirm are provided for during life, and where consumptive and other chronic cases, most of whom have been in-patients or out-patients of

hospitals, and being entirely without resources, end their days."—p. 6.

It is no matter of astonishment, then, that where, as is mostly the case, the change of character in the inmates has not attracted the attention of guardians, the traditions of thirty years and more ago should still be observed, nor yet that the structural arrangement in the older buildings should be found wholly unfitted for the use to which they have come to be applied. Regarding the metropolitan workhouses, then, as refuges for the destitute sick and infirm, let us see in what respect they are deficient in arrangements, either structural or domestic, for the proper care and maintenance of persons of this class.

1. *As to site and general character of the buildings.*—There is not much to be said against the site of the greater number of the metropolitan workhouses, except that since their first erection they have generally been greatly hemmed in by buildings usually of a low class. Out of forty inspected seventeen occupy, however, suburban sites, and are built upon sufficiently large plots of land—some are new or comparatively new buildings, but the majority are old, and quite unfitted for the purpose of lodging sick persons. These older houses present all the evils of cramped narrow staircases, low-pitched wards lighted and ventilated only by windows on one side, and when these are closed in winter, receiving their supply of fresh air only through the passages which lead to them. In the case of St. George's the Martyr the workhouse is surrounded by premises where various noxious trades are carried on; and in that of the Strand Union a carpet-beating business, bringing in about £600 per annum to the parish, is stated to have been carried on for years immediately below the windows of the sick wards, and upon premises belonging to the parish. Indeed, very few of these old houses are capable even of being adapted to the requirements of the sick without complete reconstruction. Even in the newer workhouses there are vices of construction which should never have been permitted, such, for instance, as the construction of two wards on a floor separated by a central corridor, sometimes very narrow, long, and dark, and quite incapable of being used as a medium for introducing air into the wards. A great and common defect noticed by Dr. Edw. Smith is the great height of the window-sills from the floor, so that the inmates even when standing cannot look out upon the ground, by which a prison-like character and gloominess are imparted to the wards; nor, as a rule, are any pains taken to render the wards more cheerful by tinting or any other kind of ornamentation.

2. *Classification.*—At the time of the visit of the Lancet Commission the classification of the workhouse inmates is said to have been most inefficient and improper in most of those visited; the fact mainly dwelt upon being that the insane were not always separated from the body of the house.

"At most there are insane wards, in and out of which, however, the patients pass and mingle freely with the general patients. At Clerkenwell, indeed, it is true that the male and female insane are guarded apart in two dismal wards, where, as far as can be seen, they have no earthly occupation except that of moping; but even here the imbeciles pass freely into the body of the house. Now, it is true that the insane patients in workhouses are for the most part chronic cases, and that dangerous lunatics are sent away as soon as possible, but as a matter of fact we have ascertained that a considerable number of cases of acute mania are retained in the houses from four to ten days owing to difficulties about the forms of removal. This was especially noticed at Chelsea, where there is no padded room, nor any provision (beyond the receiving room) for the temporary seclusion of even the most violent lunatic, and where there are also about thirty chronic insane patients, who wander in a melancholy objectless manner about the house and the yards."—p. 16.

All the workhouses are not alike bad, however, in this respect; the Lancet Commissioners give special commendation to the guardians of Marylebone and Newington for their arrangements for the insane. Fever and smallpox cases are not willingly admitted as a rule into the workhouses, but are transmitted to the Fever and Smallpox Hospitals respectively; but, when these special hospitals are full (and they have been filled to repletion of late), it has sometimes happened that a patient has been sent back again to the workhouse, and then temporary arrangements have had to be made. Still at several workhouses cases of fever are occasionally retained, and, where there are no separate fever wards, such cases are necessarily placed in ordinary wards with the other cases. We are told by Dr. Smith that venereal cases are mostly sent to the Lock Hospital, only doubtful and milder cases being retained; sometimes they are placed in separate wards, but in other houses they are mixed with the general inmates, or placed with itch and other diseases in foul wards. Except in the disgust which itch and some venereal cases are calculated to inspire, we cannot see why these two classes should be associated together. Certain it is not because their treatment is similar, nor yet because any moral stain is common to the sufferers from scabies and syphilis. Twelve of the workhouses visited by Dr. Smith had no separate itch ward for one or both sexes. "The sick children in the metropolitan workhouses are not always treated apart from the adults;

for in some workhouses, as St. Giles' and St. Luke's, there is no such nursery. The absence of a sick nursery is a too general defect."

3. *Infirmary Ward arrangements.*—It is not to be supposed that the sick in all the metropolitan workhouses are to be found only in the "sick wards." In some instances these wards are too limited in number to permit of the separation of the sick even from the infirm, much less from the healthy; and hence, sometimes, not only is there no separate infirmary building, but the sick are to be found scattered in the wards all over the house. This was the case in Clerkenwell, both at the time of the visit of the Lancet Commission in 1865, and of Dr. Smith in 1866. In the new houses there is little to complain of as respects the method of *ventilation* of the infirmary wards, except where the vice of construction before referred to exists. Mr. Farnall says that very few of the *water-closets* for the sick wards are well placed; some of them are placed in the wards themselves, some on the landing-places of the staircases; and when, as in some old houses, they are placed within the wards, the defective arrangements for flushing and the imperfect ventilation render them a dangerous nuisance. The Lancet Commissioners detail several instances of this fault, and among the rest they mention the existence at the Bethnal Green Infirmary of an anteroom—practically the same room with an untrapped closet—in which were three beds, occupied by a paralytic, an epileptic, and a wards-man.

"The two unfortunates were lying on low beds about six inches from the floor, in close contiguity to the closets, which were in fact merely parted off by a partition not rising more than half way to the ceiling. The three seats were part of a common latrine, not parted off from each other, and stank abominably. They were used by some sixty-five persons, who not only passed through the 'ward,' but might be said to evacuate within the ward, for ward and closet constituted practically but one small room."—p. 151.

At this workhouse they found that, except across an open yard, no water-closet was provided at all for the female wards, the rule being to use a night chair without even a screen to put round it. As regards the *bed* and *bedding*, both the Lancet Commissioners, Mr. Farnall and Dr. Smith, report great defects. It is true most of the bedsteads are of iron and of proper length, but the beds were frequently found to be too short, and, being made of flock, were generally lumpy, and being moreover imperfectly filled, were rendered still more uncomfortable by the absence of any mattress between them and the bands of iron at the bottom. Neither was the *furniture* found to be com-

monly such as it ought to be for sick persons; forms without backs, or with backs too straight and upright, an absence of easy chairs, of head rests, cupboards for cups and saucers, plates, and food, and even of proper means of maintaining personal cleanliness were not unfrequently observed. In the matter of towels especially great fault is found by all our reporters. At one infirmary the nurse of the syphilitic women's ward distinctly stated to the Lancet Commission that there was but one round towel a week for the use of eight inmates; and Dr. Smith mentions wards where only two are allowed per week to a ward, being not changed more than twice a week. In fact, the supply of towels varies in almost every workhouse, and there are very few where each sick inmate has a separate towel. The instances, too, were very rare in which hair-brushes were provided for adults, and only one or two combs, as a rule, were supplied for the use of all the inmates of a ward. The filthy practice of using the chamber utensil for washing purposes is alluded to as existent in some of the workhouses as habitual among the bed-ridden patients.

We mention these points as instances illustrating the general faultiness of the ward arrangements, not as the only faults observable. Some wards, unexceptionable in other respects, presented flagrant deficiencies not noticed in others; some few gave little cause for complaint in any of their internal appliances, while others had scarcely a redeeming point about them, so bad were they in every respect.

4. *Crowding in the Wards. Cubical Space.*—The importance of abundant breathing space in sick wards occupied by cases of all kinds, night and day, is now universally recognised, and a sufficient floor space around each bed is also admitted as an important desideratum. The Poor Law Commissioners have required, in all such wards, that a minimum cubic space of 500 feet per bed should be afforded; a very low estimate, if the requirements of health be considered, and the universal practice of modern institutions for the sick. Mr. Farnall gives a table illustrative of the irregular manner in which even this rule is observed in the metropolitan infirmaries, and in some of the newer as well as in the older workhouses. There were only eight workhouses in which, in any wards, the cubical space per patient exceeded 1000 feet, and in all of these there were wards where from 590 to 233 feet per head only were provided. On the other hand, there were found to exist in all but one or two of the workhouses wards so crowded as to allow from 490 to as little as 206 cubic feet per head to their inmates, while the amount of floor space allotted to each bed was very rarely above 100 feet, and sometimes as small as from 18 to 30 feet. For

the most part a separate bed is allowed to each sick person, but still as many as 97 double beds, occupied day and night by sick or bedridden persons, were counted by Mr. Farnall.

5. *Nursing.*—The system of nursing seems to have been generally very defective, in consequence of the practice commonly adopted of employing paupers in the performance of this important duty. Even where paid nurses have been appointed, they have commonly been inadequate as to number, and sometimes other duties beside that of nursing have been assigned to them. Most of the scandals that have recently shocked the public mind have arisen from the inefficiency of the nursing arrangements. Mr. Farnall gives it as his opinion that pauper nursing should be wholly abolished, for the following reasons, viz. because he finds that a great many of the pauper nurses cannot read either printed or written labels on the bottles of medicine which they are entrusted to administer; because, as a rule, they are feeble old women, who know nothing about nursing; because their previous careers have been in many instances vicious; because their love of drink (fostered sometimes by a reward in the way of gin being given by the guardians for extra services) often drives them to beg or buy or rob the sick of the stimulants which they should give them, and because their treatment of the poor is, generally speaking, not characterised either by judgment or by gentleness. In May, 1865, the Poor Law Board issued a circular condemnatory of the employment of pauper nurses, but at the time of the visit of Mr. Farnall and Dr. Smith in April and May, 1866, there were four workhouses where the pauper nursing was still in force, no paid nurses being appointed. And where they had been appointed, in nominal compliance with the recommendation of the Poor Law Board, they were too small in number for the patients that they had to attend to; and even at the commencement of the present year arrangements for night nursing were quite exceptional. Every medical man knows what this means in many acute cases, and in the majority of old, infirm persons. It is more than a mere unjustifiable neglect; it is an absolute and positive cruelty. There can be no doubt of this, that under such a system many an old and debilitated patient dies from the lack of that attention and nourishment in the night hours, which would at all risks have been given in their own miserable homes by some kind and watchful relative. Removed from their family surroundings, by what right do guardians refuse to supply to the poor the necessary care which, had they not been thus removed—had not the wife, for instance, been separated from the husband—one member of the family would have bestowed upon the other?

6. *Dietaries.* — The unlimited power accorded to medical officers to order any special diet for the sick and infirm, a power which is fully exercised, removes much of the evil which might otherwise be anticipated from the use of the ordinary house-diet by the old and infirm. Still the latter must be put upon the sick list to be thus privileged, otherwise they must use the pea-soup, the tough boiled beef and dumplings which are only capable of being masticated and digested by the more robust inmates. But still there remains the fault of bad cooking for the sick, which is almost inseparable from a system in which no special infirmary kitchen is provided, and where the cook is some pauper who knows little or nothing about the preparation of food. This at the commencement of the present year was the case in half the metropolitan workhouses, only 22 of which had a paid cook who was not a pauper. In many instances, too, no provision was made for the serving of the food warm to the sick, and so in a palatable and enticing form. Dr. Smith noticed that the usual manner of serving food to the infirmary was to put the several messes upon plates, and to pile them one upon another on a tray itself entirely uncovered. Altogether, it appears that there is too much monotony in the diet.

7. *The Supply of Medical Attendance and Medicines.*—There were last year only three workhouses in which there was, at the time of Mr. Farnall's inquiry, a resident medical officer. Otherwise the surgeon of the establishment was non-resident, visiting the sick once or twice daily, and at other times on emergency. Mr. Farnall gives the following testimony to their services:

"The medical officers of the metropolitan workhouses appear to me to do their duty to the best of their ability; but I am obliged to add that in many instances their duties are very arduous and their salaries inadequate. An examination of the duties prescribed by your Board to be performed by a medical officer of a workhouse is the best evidence of the very responsible post which he occupies; he has to attend punctually upon all persons in the workhouse who require his aid; to examine every pauper who enters the workhouse, and direct the warding of such pauper; to give directions for the diet, classification, and treatment of all sick paupers, and of children, and women suckling children; to report to the guardians all defects in diet, drainage, ventilation, and warmth of the workhouse; to report defects of the infirmary, and of the mode in which the nurses do their duty; and to keep the medical books, together with all the various details prescribed by such books."—p. 6.

We may add that for the most part he has to dispense all the medicines, and in many instances to find the drugs at his own expense. The following are the observations of the Lancet Commissioners upon the position of the medical officer:

"In the first place we must declare our opinion that under the present system the medical officers are habitually placed in an entirely false position by having twice or three times as many persons under their nominal charge as they can possibly do justice to. There can be no doubt that they strive to do their best, and they amply earn their moderate and indeed stingy salaries; but there are limits to human capacity; and when we are gravely told that one surgeon assisted by one resident junior is expected to look after 300 acutely sick and 600 chronic cases; or that (in another instance) one medical officer can attend to 130 acute and about 300 chronic cases in the house in the intervals of private practice, we know very well what these things mean. It is time that the ratepayers were accustomed to look at this matter honestly; at present they are engaged in what would be a farce if the matter were not so deeply serious. We challenge the whole profession to say whether the experience of our hospitals does not show that the above or any similar proportion of medical attention is utterly and hopelessly insufficient for the adequate treatment of so many patients; yet the instances to which we have referred are not those of comparatively ill but of comparatively well managed infirmaries. We regret to say also that the enormous overwork thrown upon the medical men renders it in most cases impossible for them to give their attention to many details of hospital management which every medical officer should study. In particular we have been struck with the fact that the doctors not unfrequently know less of the qualifications of their nurses than an outsider, with the requisite tact, may soon discover by a little patient cross-examination. We were particularly struck with this in one instance where a workhouse surgeon had been arguing for the continuance of pauper nursing from the supposed excellence of the pauper nurses employed in his own infirmary."

"Taken as a body, the medical officers of the metropolitan workhouse infirmaries apply themselves with a zeal and an amount of success to their disproportioned tasks which are surprising; and it must not be forgotten that they have in most cases not only to perform most arduous professional duties and a large amount of desk work, but that they have to fight the battle of the poor, with terrible earnestness, against the prejudices and the gross material interests of the worst members of their board of guardians. All honour to the more enlightened and disinterested guardians whose ears are open to truth and justice and humanity. But let us try to picture to ourselves a board of guardians who allowed stone-breaking to be carried on in their workhouse yards, who could with difficulty be restrained from dressing the unfortunate prostitutes in this house in a quasi-convict dress of violently contrasted colours, and from setting up beds for their young unfallen girls in close proximity to these very fallen creatures (so degraded that it seems there was no harm in insulting them); and whose willful neglect to build a properly isolated dead-house for their parish has long exposed the poor of a crowded district to frightful sufferings and risks of disease. . . .

"With regard to the powers entrusted to the surgeon we are of

opinion that great need for reform exists. At present, owing to his nominal inferiority of rank to the master, an official who is nevertheless (save in exceptional instances) socially below him, an antagonism is often set up, and in many cases leads to the most vexatious and mischievous interference with the purely medical orders of the surgeon."—p. 29.

We have thus very imperfectly exhibited the picture drawn by the authors of the several works and official reports placed at the head of our article. Our space would not permit of our exhibiting the picture in its entirety; we have been compelled to content ourselves with unveiling portions of it here and there. Most of all, we feel that we have failed in imparting to our narration the graphic interest which pervades the originals. Some of the latter have been denounced as "sensational." If in stating truthfully the facts they observed the writers have roused the better feelings of the community, this cannot be imputed to them as a fault. Far better is it that the truth should be fully and honestly told, even with burning words, than that grievances such as are laid bare should be tenderly overlaid with conventional phrases lest the sensibilities of the easy-going, contented public should receive too rude a shock. For what do these grievances amount to? They amount to this, that the State years ago undertook the entire charge of the destitute whom the Church in former times consoled with its sympathy, and relieved liberally, and that the poor are now no longer relieved (at all events in the metropolis) either in a considerate or liberal manner; that poverty, once held in honour and never necessarily a disgrace, has come to be accounted a disgrace, the deeper, the greater the destitution, its very designation altered to suit the changed views of the times in which we live; that, with this, the poor, formerly sought out, and even now sought out by Christian congregations with the object of assisting them, have come to be regarded by the local administrators of the law as a nuisance, and to be treated as if they had brought all their troubles upon themselves, and by their own fault alone; to be dismissed with a mere pretence of relief, or when sick, and admitted into the parish workhouse, to be deprived of the solace and tender care of their family connections, and handed over to the unsympathetic attentions of ignorant officials, lodged in places sometimes even less wholesome than their own miserable dwellings, and never assured in resorting to the refuge provided for them by the state that they will not ever after be immured in what is little better than a crowded, gloomy, and unwholesome prison house, with associates such as in better times they may have always striven to avoid. If there be disgrace, at whose door does the disgrace lie? If

there be fault, who is the culprit? Are the poor ignorant and brutal? Who has made, and left them so? Are they destitute of all self-respect? Who shows them the consideration which leads up to it? Have they no notion of the decencies of life? Who puts the laws in force which would even ensure to them a decent and wholesome dwelling? Are they indolent? What pains are taken that the children should all be taught to get their own living, and be brought up in industrious habits? Are they cast down, degraded, and broken-spirited? Where is the "guardian" of the poor, who has ever gone one step out of his way from his own comfortable dwelling to the board-room, to discover the little trouble of his neighbour which a single well-directed contribution would have overcome, but which is left to be succeeded by others, each more crushing than the preceding, until the degradation of the "pauper" is at length attained?

Still we do not lay all the blame of this shameful state of affairs upon the shoulders of present or past guardians—the legislature has itself something to answer for. Its object in repealing the statute of Elizabeth and enacting the "new" Poor Law was a praiseworthy one. Its end has been attained. A new condition of affairs has resulted, and the change has passed unnoticed.

"At the time when the new Poor Law came into operation sturdy paupers and valiant beggars infested the country—men who would not work when they could, frequently ruling the parish to which they belonged, and dictating their own terms of relief. In many districts the labouring population were demoralised, and rapidly becoming a burthen to the country instead of a source of prosperity; under the new law, hedged round as it is by safeguards which prevent the violation of its spirit through the laxity or prejudice of local authorities, a vigorous system of repression was inaugurated, the result of which has been that able-bodied pauperism is all but extinct, and thousands earn their own living who under the former system would have been pensioners on the public purse.

"To the spirit of repression which everywhere pervades, and I think justly, the administration of the Poor Law, we may look for the origin of many, if not all, the defects which exist in our workhouse management; not that they are a natural consequence of the law, or that they are in any way chargeable to it, but rather to the fact that the energy and attention of the administration has been too exclusively directed to the repression of pauperism, and has to a great extent overlooked the mass of mental and physical misery which everywhere pervades the pauper population."—*North*, p. 3.

We thoroughly believe that the department of the state entrusted with the control of the system established under the pre-

sent Poor Laws has done its very best to carry out the benevolent intentions of Parliament; but what could it do in London, hampered as it has been, and its recommendations set at nought by local boards armed with special privileges of independent action; how could it direct a more liberal scale of relief, and more convenient accommodation for the sick on the breaking up of their own homes, when the funds for the purpose had to be extorted from a limited community, often itself little better off than the poor who sought their assistance?

But it is to be hoped that, thanks to the "sensational" publications and the untiring efforts of those of our own profession who have taken the matter in hand, a better day is dawning upon the poor in London. And the first instalment of the debt which we owe them is about to be paid under the auspices of Parliament, which in its present session has passed the "act for the establishment in the metropolis of asylums for the sick, insane, and other classes of the poor, and of dispensaries; and for the distribution over the metropolis of portions of the charge for poor relief; and for other purposes relating to poor relief in the metropolis." We have now, therefore, to consider the leading provisions of this statute, which must ever be associated honorably with the name of Mr. Gathorne Hardy, and to point out how, in our opinion, it will be most advisable that they should be carried into effect.

In the construction of his bill, Mr. Hardy availed himself fully of the advice tendered him from non-official quarters, whenever based upon accurate observation, and tempered by soundness of judgment, as well as of the recommendations of the inspectors of the Poor Law Board. In adopting this course, as well as in the speediness of his procedure, and the courage which he showed in introducing his bill into Parliament when it was occupied with so absorbing a question as "popular representation," he deserves great praise. His success shows that he judged rightly in not under-estimating the breadth of meaning with which members of all parties in the House of Commons accepted the position in which they were placed.

One of the principal features of the new act is, that it increases the jurisdiction as well as the powers of the Poor Law Board, as respects the metropolis; it does away with all those local acts which have hitherto rendered some parishes independent of their control, and gives to the board the power of fixing the salaries, and prescribing the number and the duties of all officers which they think necessary to be appointed. They are left in charge of the administration of the law as regards the poor, with power to vary local arrangements of all kinds

at their pleasure, while the local authority is rendered merely executive; the duty of the guardians being little more than to carry out the directions they receive, to collect the funds, and to keep accounts of local expenditure. Another good point is the limitation which they are empowered to impose, where they see fit, in the number of guardians of the poor, and the power of nominating as they see occasion for it additional guardians to those elected by the ratepayers from a class of parishioners who, from their social position, may be expected to be less influenced than elective guardians would be by motives of an unworthy character. Justices of the peace are *ex officio* guardians, and in parishes hitherto governed by local acts may be expected to exercise a beneficial influence in the interests of the poor.

The act provides for the redistribution of the various parishes and unions, and their combination for certain purposes into districts, the number of which is left to the discretion of the central authority. In each district, asylums are to be established for such classes of the sick, infirm, or insane as the board may from time to time see the propriety of treating separately, and they may further direct what persons from among the sick and infirm poor shall be removed into them. In each district the asylums are to be placed under the management of a Board of Managers, partly chosen by the guardians of the several parishes of the district, and partly nominated by the Poor Law Board. Where an existent workhouse can be wholly or in part adapted to such a purpose, it may be made use of; under other circumstances buildings more appropriate may be erected or hired, but in no case are the asylum managers at liberty either to select or to determine in what way existing buildings shall be altered, or on what plan new buildings shall be erected. It is, therefore, to be earnestly hoped that the faulty arrangements now in force will not be perpetuated, but that as these asylums will be, in fact, district hospitals, whether for the acute or chronically sick, or for the insane, all the requirements of modern sanitary science will be observed, and especially that the full amount of breathing space and floor room, with good and natural ventilation from without, will be combined with cheerfulness of appearance in their construction. There is one clause in this act which especially calls for commendation, namely, that which directs that the asylums may be used for the purpose of medical instruction, and for the training of nurses. Now, we hold that nothing is so well calculated to keep a public medical officer up to his work, to render him careful in his diagnosis, and in the management of the cases under his charge, as the feeling that he is every day subjected to the sharp and prying eyes, and to the comments, of students.

Better supervisors and more uncompromising critics are not to be found anywhere. Besides, the arrangement opens to them a field of practical study of chronic diseases such as only exists to a slight extent in our largest hospitals, and does not exist at all in the smaller. It also will give them facilities for the study of mental diseases, which are very much needed in the present day. We think we can see in all this, too, looming in the future, the very desirable emancipation of students from the hospital bondage in which they are at present held. It will be for the medical council, in the interests of freedom of study, to take advantage of the new arrangements. The plan of making the new asylums training schools for nurses not only tends to surmount the difficulty of substituting educated for pauper nursing, but affords a new field of work to which the elder girls in the district schools may be brought up, and promises at no very distant date to provide for the middle classes of society nurses in sickness such as can be depended upon, and at a rate of remuneration which their circumstances will permit them to pay. There can be little doubt that the first step taken by the Poor Law Board will be to provide asylums for the reception of fever and smallpox cases. The special hospitals supported by voluntary contributions, towards the support of which the parishes now pay per case that they send, have long been inadequate to the wants of London. They might suffice for a northern district, but that is all, supposing that the governors were willing to dispose of them, or engage to keep a definite number of beds at all times open for the reception of pauper patients. Now, inconveniences arise, not only from the occasional repletion of these hospitals, but from the long distances that patients have to be carried to them. We hope also that the Poor Law Board will consider it within their powers to direct that each parish shall be provided with appropriate conveyances for the carriage of such patients. As the law now stands such provision is permissive only, and but few parishes have taken advantage of the permission.

The Venereal Committee of the Harveian Society suggest that it might be possible under the provisions of the new Act to make arrangements for the better care of poor persons suffering from venereal diseases. The need is a pressing one. From inquiries which they instituted, it appears that nearly half the surgical out-patients of some of the London hospitals are persons suffering from one or other of these affections; in others, these persons constitute from a fifth to a third of the number daily seen. At St. Bartholomew's Hospital alone as many as 174 venereal cases, on an average, are seen daily. This will give some idea of the amount of venereal disease among the poorer

classes of London. And it is for the interest, not only of these miserable people alone, but of the entire population, that adequate provision should be made for their reception into hospital. Yet, putting all the London hospitals together, there are but 150 beds available for diseased prostitutes. In 1865 there were in London close upon 6000 prostitutes known to the police, probably not more than one third of those who gain their living by prostitution, and a large proportion of these are diseased. The London Lock Hospital has no more than fifteen beds for males, and never more than thirty beds available for the prostitutes of London. Applicants even with large ulcerations of the genitals are constantly compelled to be sent away for want of room. Now, we have already stated that venereal cases are not, as a rule, kept in the London workhouses; the parishes mostly subscribe to the Lock Hospital; and the only result can be that probably some of these prostitutes, and certainly the rejected and out-patients, are forced to go on with their trade in order to live. Mr. Acton calculates that 1500 beds at least would be needed in London for the reception of venereal cases in males and females. However this may be, and the Committee of the Harveian Society consider it not an exaggerated estimate, it is perfectly clear that the contemplated establishment of asylums would be very incomplete without some provision for the reception of diseased prostitutes, either into special hospitals or into special wards set apart for such cases in the workhouse hospitals. The committee, indeed, go further than this, and recommend the extension of the operation of the Contagious Diseases Act to the civil population of London. But this is a matter which we cannot discuss now.

The removal of all contagious cases and of lunatics and imbeciles from the workhouses of London will to a moderate extent only relieve their overcrowded condition. The mention of imbeciles, however, reminds us of another grievous want in the metropolis, and that is, of a proper separate establishment for the reception of imbeciles and idiots. It is cruel to herd them in ordinary sick wards or in ordinary lunatic asylums, both because such poor creatures require a special kind of management, and because modern science has demonstrated that many are so far capable of improvement as to become useful members of society. The grand institution at Earlswood should have its counterpart for the poor in the vicinity of London. There are plenty of neglected creatures ready to fill it.

The act also makes provision for the better administration of out-door medical relief. District medical out-door officers have hitherto, for the most part, not only been most miserably paid for very hard service, but they have generally been required to find their own drugs. The Poor Law Board, indeed, have re-

commended that in all cases the most expensive medicines, such as cod-liver oil and quinine, shall be found by the guardians, but this recommendation has not in all cases been carried into effect. But in addition to this, the very dwelling-house of the officer has been used by the guardians for the reception of out-door patients, to the great detriment of the private practice of the medical man; for it is not likely that well-to-do patients would call at a residence or surgery the door of which is every morning blocked up with disreputable-looking and not over clean persons, often carrying in their arms children suffering with a variety of contagious disorders. In future this abuse will be done away with. Dispensaries will be established at which the medicines and other requisites for the cure of the sick will be supplied at the cost of the guardians on the written order of the medical officer, and houses or rooms in convenient localities will be built or hired at which the out-patients may attend for the advice that they need.

Another step which it is proposed to take seems to be the classification of the inmates of workhouses, and the distribution of each class more or less distinctly in separate buildings. We hope that this will remove a stigma from the present method of accommodating the destitute poor, which has lain upon it ever since the present Poor Law has been in operation. We refer to the separation of aged couples, whose mutual association is essential to their happiness and comfort, and when infirmity is not extreme on both sides, is calculated to relieve the authorities of a part of the nursing now left in the hands of not very considerate attendants. We can conceive of nothing more cruel, of no blow more severe that could be inflicted under the name of benevolence, than the breaking of the marriage tie under such circumstances. Many, we know, would nearly have preferred death to subjection to so unholy an interference. Mere humanity demands that this shall be one of the earliest amendments brought about under the operation of the new law.

The inequalities and insufficiency of the present out-door relief of poor persons, due in part to the poverty of some parishes and the low class of persons often elected as guardians, will, we believe, be in great part obviated by the improvement of the local boards and by the provisions made in the Act for the expenses incurred in giving appropriate relief. Considering that the sick constitute a large proportion of the persons applying for material aid, we may expect that under the new *régime* the medical officers will find that no small part of their duty will consist in giving written orders for food, clothing, and bedding. We hope that if this is the case they will co-operate with the medical officers of health and sanitary au-

thorities of the metropolis by aiding them in substituting new articles for such as the health officers may on sanitary grounds think it right to destroy. But this is not the only mode in which the Poor Law authorities ought to strengthen the hands of those engaged in arresting the spread of contagious diseases. It is among the persons who seek medical relief from the parish that fever and smallpox, to say nothing of some other contagious diseases, commonly originate in a district, and the removal of the sick to a hospital, and the disinfection of the rooms and clothing, &c., immediately afterwards are the means most likely to arrest their spread. Early notice of the occurrence of such new outbreaks ought to be forwarded as a matter of course to the health officer, and he should be provided with the means of carrying into effect the disinfecting processes for the performance of which permission is now given by the Sanitary Act of last session. The medical officers of the parishes will now be independent of their local boards, and there can be no reason why some co-operation should not be set on foot by which the arrest of the spread as well as the cure of disease may be more effectually accomplished. There are existent at the present time two associations—the Metropolitan Association of Medical Officers of Health, which has already shown itself a power in the state, and that of the Poor Law Medical Officers—who have their honours and power to win. Both are interested in the diminution of disease. Why should they not confer so as to establish a system of co-operation and mutual assistance? The time is favorable. Never was time more so. Let not the present opportunity be lost.

Lastly—and this in the working of the Act is not the least important point for us to direct attention to—we come to the method directed for the payment of expenses incurred in its administration. The district asylums are to be built or hired, furnished and supplied with medicines, and all medical and surgical appliances, at the expense of the entire district in which they are situated, and the salaries and maintenance of the officers are provided for in a similar manner. The expenses of the food, clothing, relief, or burial of the inmates, are to be borne by the parishes or unions from which they severally come. In a similar manner should the Poor Law Board direct a removal of the poor from any workhouse into another in a different parish, as a result of any plan of classification they may adopt, the expenses of lodging and maintaining such poor persons will become a charge upon the parish from which they have been removed. But as respects all other expenses incurred in the relief of the poor a Common Poor Fund is established, to which all the parishes and unions in the metropolis will be called upon to contribute in

proportion to the annual rateable value of the property in each, or on such other basis as the Poor Law Board may from time to time direct. This latter provision was necessary in order to avoid any unfair advantage being taken by any particular parish, in the way of any alteration in the assessments of property which each local authority may be disposed to make, and in order to relieve the poorer parishes of an unfair amount of the general burden which it is intended to distribute equitably over the entire metropolis. Under the new statute, also, the Masters of the Bench, and the governors or treasurers in other places where now there is no poor rate at all, will be assessed to this common fund. The expenses to be paid out of the Common Poor Fund are those enumerated in the 69th section of the statute—the maintenance of lunatics (except those expenses chargeable upon the County Rate), the maintenance of patients in any asylum specially provided under the Act for patients suffering from Fever or Smallpox, the expenses incurred for all medicines and surgical appliances supplied generally in the course of the medical relief of the poor, the salaries of all officers, compensation to officers deprived of their office, fees for the registration of births and deaths, fees and other expenses of vaccination, the maintenance of pauper children in district or other schools, and, lastly, the expenses of all the out-door relief throughout the metropolis.

We cordially congratulate the medical officers of the metropolitan parishes on the emancipation effected by the operation of this act, and both them and the philanthropic persons who have agitated the subject before us, on the successful issue of their self-denying labour. We are no enemies to the principle of local self-government. By all means let it be tried wherever considered applicable; but where it fails, either from the incompetence of the local governing body, or from the antagonistic operation of self-interest, or from any other irremovable cause, we hold that no rigid and unbending compliance with an abstract principle should be permitted to hinder its being practically put into abeyance. Under the operation of this principle the management of poor relief in London had become a scandal too glaring to be much longer overlooked; it was rendering us contemptible in the eyes of foreign nations, and bringing our national system of government into disrepute. How long will a similar scandal be permitted in respect of our sanitary laws? Surely it is high time some similar modifications were introduced into the hygienic administration of London. Last year a great deal of trouble was taken to carry through Parliament the Sanitary Act, the execution of which was left as a permissive proceeding in the hands of local

vestries and district boards, very little higher in their constitution than the guardians of the poor. Even now, with the honorable exception of one or two parishes, no action whatever has been taken to carry it into effect. The medical officers of health may write reports, may urge the necessity of its adoption, may repeat the same recommendation month after month, but their advice and suggestions are as thoroughly ignored as those of the Poor Law Board had hitherto been wherever that board was powerless to compel their adoption. The next thing for the present government to do for the benefit of the metropolis is to centralise its sanitary administration by placing it in the hands of a department of the state, leaving local boards no option as to carrying out necessary amendments, and giving to the medical officers of health a position, as respects salary and removal from office, independent of the petty corporations who now so commonly underrate and insult them.

Review II.

1. *Principles and Practice of Medical Jurisprudence.* By Alfred Swaine Taylor, M.D., F.R.S. London: Churchill. 1865. 8vo. pp. 1186.
2. *On Poisons, in Relation to Medical Jurisprudence and Medicine.* By Dr. Taylor. 2nd Edit. London. 1859. pp. 863.
3. *A Treatise on Poisons.* By Dr. R. Christison. Edinburgh: Adam and Charles Black. 4th Edit. 1845. pp. 987.
4. *Etude Medico-Légal et Clinique sur l'Empoisonnement.* Par Ambroise Tardieu, Professeur de Médecine Légale à la Faculté de Médecine de Paris, &c., avec la collaboration de Z. Roussin, Professeur Agrégé à l'Ecole Impériale de Médecine Militaire du Val de Grâce. Paris. Baillière.

Medico-Legal and Clinical Treatise on Poisoning. By A. Tardieu, Professor of Medical Jurisprudence to the Faculty of Medecine, Paris, and Z. Roussin, Vice-Professor at the Imperial School of Military Médecine Val de Grâce. pp. 1572.

5. *Handbuch der Giftlehre.* Von Dr. A. W. M. Van Hasselt, nach der Zweiten Auflage aus dem Holländischen bearbeitet von Dr. J. B. Henkel, zu Tübingen. Braunschweig: Vieweg und Sohn. 1862.

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6. *Handbuch der Toxicologie im Anschlusse an die Zweite Auflage* von A. W. M. Van Hasselt's Hanleidung tot de Vergiftleer von Dr. Th. und Dr. A. Husemann. Berlin: Reimer. 1862.

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8. *Die Gerichtliche Chemie fur Gerichtsärzte und Juristen.* Bearbeitet von Dr. F. C. Schneider. Wien: Braumüller. 1852.

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9. *Anleitung zur Ausmittelung der Gifte* von Prof. Fr. Jul. Otto, zweite durch einen Nachtrag vermehrte Auflage. Braunschweig: Vieweg und Sohn.

A Manual of the Detection of Poisons. By Dr. F. J. Otto. Translated, with Notes and Additions, by William Elderhorst, M.D. New York: Ballière. 1857. pp. 178.

10. *Lehrbuch der Gerichtlichen Medizin.* Von Dr. Adolf Schauenstein. Wien: Braumüller. 1862.

Text-Book of Forensic Medicine. By Dr. A. Schauenstein. Vienna. pp. 512.

11. *Manuel Complet de Médecine Légale.* Par J. Briand et E. Chaudé. 7th Edition. Paris: Ballière. 1863.

Complete Manual of Legal Medicine. By J. Briand and E. Chaudé. 7th Edition. Paris: Ballière. pp. 1048.

12. *A Handbook of the Practice of Forensic Medicine.* By J. L. Casper. Translated for the New Sydenham Society. London, 1861, et sq. 4 vols. pp. 1439.

13. *Das Mikroscop in der Toxicologie.* Von Dr. A. Helwig. Mainz: Von Zabern. 1865.

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Noxious and Poisonous Gases. By Dr. H. Eulenberg. Braunswick: Vieweg and Son. pp. 550.

15. *The Principles of Forensic Medicine.* By W. A. Guy, M.B. London: Renshaw. 1861. pp. 534.

16. *Practical Chemistry for Students.* By W. Odling, M.B., F.R.S. London: Longman, Green, and Co. 1865. pp. 241.

17. *Medical Jurisprudence for India.* By NORMAN CHEVERS, M.D., &c. Calcutta: Carbery. pp. 608.
18. *Annales d'Hygiène, Henke's Zeitschrift, Casper's* (now *Horn's*) *Vierteljahrschrift, Virchow's Archiv, Reichert and Du Bois' Archiv, Henle and Pfeuffer's Zeitschrift. Moleschott's Untersuchungen, Hoppe-Seyler's Medicinisch-Chemische Untersuchungen, Canstatt's Jahresberichte, Schmidt's Jahrbucher, &c. &c.*

RETURNING to our consideration of recent toxicology, and taking, as before, Dr. Taylor's work on the 'Principles and Practice of Medical Jurisprudence' as our guide, we shall this time begin with vegetable poisons. We have already alluded to the great difficulty of classifying poisons satisfactorily; but if this stumbling-block was appreciable when we spoke of mineral poisons, it is three times more apparent when vegetable poisons have to be discussed. The great obstacles in the way of grouping vegetable poisons are, in the first place, our incertitude as to their exact influence on the animal economy; in the second, their variable effects on different species, and even on different individuals of the same species; and in the third, the difficulty of obtaining any very well-marked bond of union between many of their number. Here also the boasted accuracy of our modes of analysis is brought to a rude test, without, we are sorry to say, in any degree justifying the high encomiums occasionally passed on them. But it is something to be aware of our own ignorance, for, knowing how much remains to be done, we are more likely to advance than if we believed that nothing more in this direction was attainable, or even desirable, if within our reach.

Thus, to select one or two examples of the difficulty of grouping vegetable poisons, we might mention the subdivision Narcotics, the type and in very truth the sole constituent of the group being opium. But search all through the vegetable kingdom, and when will another substance be found having an identical or even similar effect upon the system? Look, again, at the awkward lumber-room into which every inconvenient substance is thrown—we mean the narcotico-acrids—what common bond of union, except, it may be, their antagonism, is there between such diverse substances as the Calabar bean and belladonna, or between hemlock and strychnine? In short, the unsatisfactory nature of our present grouping only becomes the more apparent, the more we become alive to the difficulty of obtaining a better. For we cannot look upon Dr. Taylor's arrangement of them into groups according as they affect the brain, the spinal cord, or both, as being any great practical advance on the one originally adopted by Foderé.

But putting aside all such vain regrets, let us see what has been done in a practical way towards improving our knowledge of vegetable poisons, and let us ascertain how far Dr. Taylor is to be taken as a thoroughly satisfactory guide, the evidence coming from his 'Principles and Practice.' Commencing with the narcotic or cerebral poisons, Dr. Taylor first directs attention to opium.

There are many points of interest connected with the subject of opium; to all of them we cannot refer; but there is one of special interest to all practising medical men—we refer to the diagnosis between the effects of opium, alcohol, and extravasation of blood within the cavity of the skull. Now, it may at once be said that there is in reality no satisfactory means of distinguishing the one from the other, the diagnostics usually given being of such a kind as to admit of no reliable determination either way. Thus, it is commonly said that opium contracts the pupils. Well, this is not absolutely invariable; one pupil may, the other may not, be diminished in size under its influence. Again, it is said that the pupils become dilated by the effects of pressure on the brain, as in apoplexy; but exactly the same anomalous condition, already referred to as occasionally occurring in poisoning by opium, may be seen in cases of apoplexy; or, as very properly pointed out by Dr. Taylor, should blood be extravasated into the pons Varolii, both pupils will become contracted. In intoxication, again, the condition of the pupils vary with the amount of alcohol imbibed, and with the time taken in its imbibition; it may be from other influences also; at all events, nothing is more certain than that in acute alcoholism the pupils are sometimes contracted, though more frequently dilated. We are inclined to think that the former condition is most frequently connected with the effects of a large quantity of strong alcohol rapidly swallowed, the latter by the imbibition of a considerable quantity of alcohol over a rather longer period; certainly the other appearances distinguishing these two forms of poisoning are distinctly marked, but, beyond the valuable investigations of Dr. Ogston on this subject, we have not many trustworthy researches. The means of diagnosing, by the pupils alone, being thus obviously incomplete and uncertain, we find that the other characteristics relied on are equally so. Thus, the degree of insensibility has been brought forward as of some value, but is really useless, inasmuch as it will not enable any one to distinguish between a slight degree of, say, poisoning by opium, and a higher one of poisoning by alcohol. The smell of the breath has also been employed more especially to distinguish opiate and alcoholic poisoning, but in reality the two may be combined, and, in fact, often are so;

under such circumstances no means exist of ascertaining which has the most important share in the production of the toxic symptoms. Neither is it unusual for a drunken man to become apoplectic from the effects of falls or otherwise, nor to pass almost directly into a condition of uræmic poisoning. We have refrained from speaking of those diagnostics founded upon the history of the case, for where the history is known all is known, the great difficulty existing in dealing with cases where men are picked up in the streets in an insensible state. One more diagnostic and we have done with the subject; it has been proposed to examine the urine, which can and generally ought to be drawn off by a catheter, to ascertain the existence of alcohol. Now, in the first place, the detection of alcohol in the urine proves nothing; in the second, we know that in the worst cases of acute alcoholism it can only exist there in very small quantity, the patient sinking so rapidly under its influence; and in the third, we do not know how it is to be detected save by its physical properties, which imply as a matter of course that a very large quantity exists in the urine. When we hear of men relying upon the production of the green oxide of chromium by means of sulphuric acid and bichromate of potash along with the supposed alcoholic urine as a proof of the presence of alcohol, we can only hold up our hands in silent horror. Not but that, as performed in accordance with Dr. Anstie's suggestion—that is, with a graduated solution of chromic acid—it may not be usefully employed by skilled hands; so also when a small quantity of alcohol has been distilled from the urine the test may to *a certain extent* be relied upon, for alcohol has indeed pre-eminently the power of deoxidizing chromic acid, but so has ether, wood spirit, and many other substances. The only value which can be placed upon the test is derived from the fact that alcohol is more likely to be found in the urine than any other of these substances; and if present, there is a *probability* that the patient is insensible from its effects, but nothing beyond this. Thus far we have no objections to the test, if it can, indeed, be called such; but for any one to rely upon it absolutely is sheer folly—folly as great as that which a worthy surgeon evinced when in a case of poisoning by aconite he made himself confident of its presence in the stomach because the contents of that viscus gave a green reaction with the chromic acid test; this, too, when the substance had been swallowed as tincture.

But if the theoretical part of the question be unsatisfactory, it is some comfort to know that what has to be done in medical practice is more distinctly settled. If a patient be under the influence of alcohol or opium, by all means empty his stomach, and this, should there unfortunately prove to be cerebral mischief, will have

no very prejudicial, if, indeed, it would not have the opposite, effect on the lesion. Now, to waste time in introducing emetics, supposing the patient can swallow them, into a stomach already rendered well-nigh powerless by the paralysing effects of the poison, is useless, and the exhibition of nauseating remedies like tartar emetic worse than useless. The stomach-pump should be promptly and effectually employed. Thus, for instance, a very common cause of death in intoxicated persons is from the stomach attempting to get rid of the offending substance, and not being powerful enough to raise it higher than the fauces, it is dragged into the air-passages, and the patient suffocated. By the stomach-pump the viscus can also be washed out, an advantage which emetics do not present. So much for this part of the subject.

Of common poisons, with the exception of arsenic, there are none which produces such diverse effects as opium, nor can this be always accounted for by difference of dose or peculiarity of constitution. It may be that the great differences which exist in the constitution of the opium itself may have something to do with these peculiarities, for if opium is such a complex substance, and its different constituents have such diverse actions (what little we know of them shows us this), then any great variation of composition would be likely to lead to considerable diversity of effect. Still, we doubt if we have the whole truth here, and it is not yet certain that we know the whole range of variation in the symptoms. It would be needless to dwell upon the well-known susceptibility of infants to the effects of this poison, nor to the enormous doses which confirmed opium eaters can swallow; these are variations known to every one. So also the different effects produced by large and small doses of the drug are within the cognizance of all, but these are not the variations to which we refer. Thus, Dr. Taylor speaks of a *remittent* form of poisoning by opium, in which all symptoms may have disappeared, yet the patient may suffer a relapse, and die from the effects of the original dose of the poison. All cases may not be so marked as this, yet several instances are on record where, after the indications of poisoning by opium have apparently yielded so far as to render the patient conscious, and even intelligent, still a return of the torpor may, as in the case of the Honorable Mrs. Anson, carry the patient off. Again, the effects of the different preparations of opium vary, especially those of quack remedies, which are nowadays so common. The symptoms may therefore vary according to the preparation exhibited, but even this difference is not so noticeable as that which exists between the various alkaloïds extracted from crude opium. It is ordinarily assumed that poisoning by opium and poisoning by morphia possess identical

characters; in the main this is true, though not exactly so. Dr. Taylor hints that convulsions are more frequently observed in poisoning with this alkaloïd than after a large dose of opium, but we do not think that this has been sufficiently insisted on. Most men associate with the idea of opium poisoning something so different from convulsions, that they can scarcely bring themselves to believe in the possibility of their occurrence, yet they are not very infrequently encountered, especially in poisoning with the acetate of morphia; indeed, with some the idea is a favorite one, even now that Palmer killed Cooke with this substance; in the case of Castaing, this convulsive action of acetate of morphia appears to have been still more marked. If now we come to examine the effects of the other alkaloïds obtained from opium, we find that their actions are still more diverse, and here we have to thank Claude-Bernard for the information he has given us on this subject. Dr. Taylor in his work on toxicology does, indeed, allude to this diversity, but does not discuss it at any length. Bernard's experiments were chiefly made on dogs and other animals, mostly too by subcutaneous injection, and the substances employed were morphia, narceine, codeia, narcotine, papaverine, and thebaia; of these, three possess well-marked narcotic properties—morphia, codeia, and narceine—the others having a diverse or even antagonistic, and what Bernard terms *atoxic*, action. The former three also differ between themselves in certain respects; of them, *morphia* appears to put the animals in the densest sleep, yet, says Bernard, they are liable to be roused by a loud noise, when they will attempt to run away, but immediately sink down again. On coming to themselves the hind quarters of the animals experimented on are more or less paralysed, and they present an excessively stupid appearance, from which they do not recover for some hours. *Codeia*, again, whilst rapidly inducing sleep, does not seem to cause the lethargy seen after a dose of morphia; the animal can always be readily roused, and on coming to himself of his own accord does not suffer from the prostration which follows the effects of morphia. *Narceine* Bernard characterises as the most soporific of all the products of opium, for, although the animals by its influence are not rendered insensible and do not possess the same hebetude to pain which follows the use of morphia, yet they cannot be roused by a sudden noise, as when morphia has been exhibited. With regard to narcotine, thebaia, and papaverine, they seem to exercise no sleep-producing powers, and although the three soporific alkaloïds are capable of causing death in large doses, they are not nearly so dangerous as some of those of which we now speak. *Thebaia*, or, as it is sometimes termed, *paramorphia*, is the most poisonous of all, infinitely more so than morphia itself; thus,

one decigramme of the substance injected into the cellular tissue in any part of the body will kill a dog, while two grammes (upwards of thirty grains) of morphia will not destroy life. Again, with regard to *narcotine*, although not so poisonous as thebaia, it exercises another action, common to all opium alkaloïds except narceine, that is, the power of producing convulsions, in a higher degree than any of the others. M. Bernard thus makes out three sets of actions possessed by the alkaloïds of opium. First, their soporific action, seen in its purest form, if not in its greatest intensity, in narceine; next, their toxical effect on the heart, which is most strongly marked in the case of thebaia; and finally, their convulsive action, in which case narcotine may be selected as the best illustration, although as a purely toxic agent it is comparatively powerless. These investigations of Claude-Bernard enable us to have some slight appreciation of the causes which influence the peculiar effects of opium and its preparations. It is well known that the acetate of morphia varies more in its effects than any other of the opiate preparations ordinarily employed, and it is also well known that, as a general rule, except it be prepared from the hydrochlorate, it is an extremely impure salt, and one unusually liable to decomposition. Claude-Bernard's researches enable us to understand that this impurity is the very cause of its variable action, seeing that it sometimes contains an unusual quantity of narcotine, and sometimes an excess of the other alkaloïds, but rarely, except when prepared with extreme caution, is it a salt on which implicit reliance can be placed.

We do not now propose to discuss the mode in which opium acts upon the system; most men are of opinion that it acts through the nervous centres, although certain experiments by Kölliker would seem to contradict this, nor shall we do more than allude to the period, after the fatal dose has been taken, at which death takes place. Dr. Christison's law, that in most fatal cases death occurs within twelve hours, and that it seldom, if ever, supervenes after twenty-fours have elapsed, is one of great practical importance, and so well known that we only mention it to cite one notable exception which occurred in the person of a child aged three months, who died *fifty-six hours* after swallowing the poison. Again, with regard to the post-mortem appearances, Dr. Taylor very properly remarks that beyond congestion of the brain and lungs nothing unusual is ordinarily encountered. Tardieu would make him place great reliance on the fluidity of the blood, and cites in opposition cases where decolorized clots have been encountered, but in this he does not do Professor Taylor justice, for we confess to not being able to find the passage referred to. In addition to the signs above narrated, Tardieu specifies occasional spots of capillary apoplexy in the brain,

more rarely with similar apoplectic kernels in the lungs. But the most notable appearance was encountered by Professor Tourdes in a case wherein the mucous membrane of the alimentary canal was coloured yellow, mixed at some parts with the darker tint of injected vessels, from the mouth to below the pylorus. The appearance very closely resembled the coloration produced by nitric acid, but there was no breach of surface, and on examination it turned out that the colour was due to the saffron, which in considerable proportion is present in a favorite Continental preparation, Sydenham's laudanum, and is also contained in the ammoniated tincture of opium. Tardieu also refers to congestion of the sexual organs, but the frequency of this occurrence has been greatly overrated, especially by Barbier.

As the post-mortem indications of death by opium are so faintly marked, it is a matter for regret that in chemistry we have so weak an auxiliary, for we think it cannot be denied that of all the poisons in common use opium is the one most likely to escape detection. We have known chemists of undoubted skill and world-wide reputation fail in proving the existence of opium in a body, which had, but a short time before, been deprived of life by a large dose of the substance referred to. Except its odour, opium presents by itself no other distinctive character, which might enable the chemist to detect it; accordingly, the demonstration of its constituent bases and acid is chiefly relied on for proof of its presence. Morphia, as being the most abundant alkaloïd, as well as being frequently employed by itself, is that which is usually sought; and with regard to the tests for it, one or two precautionary remarks may be of service. Of course it is well known that nitric acid is one of the great reagents for morphia, but it is not so well known that the reaction produced is extremely variable. When strong nitric acid is poured on solid morphia an intensly red colour is produced, gradually, however, fading into yellow; if weak nitric acid be poured into a solution of morphia the latter tint alone will appear, and this may be almost entirely destroyed by heat. These facts, which are alluded to by Dr. Taylor, will serve to explain the discrepancies encountered in books as to the reactions of morphia. Dr. Taylor in his larger work only enumerates the tests for morphia and meconic acid, but in his special work on toxicology he refers, though but cursorily, to those of some of the other constituents. We think, however, that those of narcotine are of sufficient importance to demand attention, the more especially as it is much more soluble in ether than is morphia, and accordingly admits of far readier separation from a watery solution. With cold concentrated sulphuric acid narcotine forms a greenish-yellow solution, but on the addition of the smallest drop of nitric acid it immediately assumes an intense blood-red

colour, gradually passing into a brownish tint. Care must be taken that the solution of narcotine in sulphuric acid is not heated nor left for any length of time exposed to the atmosphere, or the change in colour may be gradually brought about. Curiously enough, Tardieu tells us that narcotine presents no very characteristic reaction. The other substances contained in opium, with the exception of codeia, are tolerably easily distinguished.

With regard to the processes in use for the separation of the constitutents of opium, we find that Dr. Taylor still adheres to the old plan of precipitating with the acetate of lead. This scheme we cannot regard favorably, and consider that to a great extent it is at the root of the evil complained of above, with regard to the difficulty of detecting opium in the body of one who has died from its effects. True, Dr. Taylor has to a certain extent improved on it by introducing an initial process, but the result is never to be relied on; besides he tells us that meconic acid may be detected in solution by a persalt of iron when morphia is present in too small quantity to be thrown down by acetate of lead. And what, after all, does the process amount to?—the morphia is supposed to combine with a small quantity of acetic acid, which might quite as well, and far better, be added in another shape, and is in this imperfect state of combination left in the original mass. To purify it some have recourse to passing the soluble portion through animal charcoal, which, if there be not much morphia present, will probably retain what there is, leaving only pure water or little more. In addition to these two fallacies we have heard of men attempting to dissolve up the morphia by ether, after which we need not be surprised that they have experienced some difficulty in demonstrating the presence of the alkaloïd. The process given by Tardieu and Roussin is infinitely superior to this; unfortunately, it is far too long to quote. Suffice it to say that it is in essentials the process advocated in these papers long antecedent to the publication of the work in question, viz. rendering the bases soluble by the addition of a weak acid, filtering, and evaporating until the alkaloid remains perfectly pure. As by these means the quantity of the alkaloïd, although obtained in a state of remarkable purity, may be to some extent diminished, owing to a small proportion being left behind in each filter, some recent researches as to the management of minute quantites of the poisonous and other alkaloïds assume a great importance. We have already had occasion to notice Dr. Guy's researches as to the sublimation of arsenious acid, mercury, and other mineral substances, and we have also referred to Helwig's discovery that many alkaloïds could be treated in the same manner. We have now to state that Dr. Guy has to a certain extent perfected Helwig's process, and obtained results which promise to be of the very utmost impor-

tance. Instead of employing a strip of platinum foil with a small depression in its surface, Dr. Guy makes use of a porcelain slab, on which the substance to be operated on is deposited; there it is surrounded by a ring of glass, on whose surface a glass slip is laid, and heat applied by a spirit lamp. In this manner Dr. Guy has obtained results far more striking than those of Helwig; he can produce, not only distinctly crystalline crusts of morphia and strychnine, in which Helwig failed, but can produce crystals of the latter with from the $\frac{1}{3000}$th to the $\frac{1}{5000}$th of a grain. It is also stated that if a quantity larger than the $\frac{1}{500}$th of a grain be employed the result is marred. Further, not only can these crystals be produced from the perfectly pure and dry substance, they can also be formed by deposition from solutions, as, say, from benzole, and they can be sublimed from salts as well as from the base itself. We consider these researches of the very highest value, and would earnestly invite the attention of our readers to Dr. Guy's original papers.[1]

The next poison discussed by Dr. Taylor is prussic acid and the substances containing it, thus associating it with opium, with which we cannot see that it possesses any affinity. Be that as it may, we will in the mean time proceed to examine Dr. Taylor's account of the symptoms produced by the pure acid, and these we think might be promptly summarised in the word "death," for the rapidity with which this results after a full dose of the poison is certainly its most striking peculiarity; and even in cases where death is not so speedy, insensibility is almost instantaneous. Connected with this are some questions of great medico-legal importance, as those of determining the amount of exertion possible after a full dose of the poison. Certain cases have occurred in which the disposition of articles has led to the belief that death had been the result of poison given by another rather than voluntarily swallowed, and on one occasion an innocent person was nearly hanged for a crime he had not committed. Dr. Taylor gives several invaluable illustrations of the retention of motion after taking a large dose, but we do not think that any can better illustrate the necessity of caution in this respect than one which occurred not very long ago in one of our west-end dispensaries, and is recorded in the columns of the 'Lancet.' The porter of the establishment had been ailing, and had prepared some medicine for himself, which he kept in a cupboard along with a number of other medicines. One day, going to help himself to a dose, he took the wrong bottle, put it to his lips, and swallowed a considerable quantity of prussic acid. He discovered his mistake and ran to the house-surgeon's room up two flights of stairs, told him of it, again descended to the dispensary followed by the surgeon, but became insensible before

[1] 'Pharmaceutical Journal,' June, 1867, et seq.

any remedies could be exhibited. Death followed almost instantaneously. The case shows the folly of trusting to such fallacious guides as smooth bed-clothes as a means of distinguishing suicide from homicide. In one of the most important trials for poisoning by this substance, that of Tawell the Quaker, a good deal of use was made of a somewhat sensational feature which was supposed to attend all cases of death from this substance. This was poetically termed the "death shriek," but we need hardly inform our readers that the phenomenon in question is of more than doubtful authenticity. In his work 'On Poisons' Dr. Taylor casts some doubt on the occurrence of convulsions in death by prussic acid in large doses; but the case we have mentioned above puts this beyond a doubt, as the symptoms were observed throughout by competent witnesses.

An inquiry of some importance from the rapidly fatal effects of this poison readily suggests itself, viz. as to the way in which it acts on the system. The rapidity of its effects has to a certain extent fostered the idea of its sympathetic action, but Bergmann's experiments, whereby it was shown that death followed more rapidly when it was introduced into the jugular vein, than when injected into the carotid artery completely contradicts the notion. Liebig's hypothesis, of its attacking the blood, through the iron contained in its corpuscles, is equally untenable. Kölliker's researches would seem to indicate that it attacked the muscular tissue and motor nerves at the same time, thus producing rapid and complete paralysis (Coze, again, says cramp) of the heart, but the subject has not yet been completely and satisfactorily examined. The speedy death which follows its exhibition is a powerful obstacle in the way of a complete knowledge of its action.

As might be expected, the post-mortem appearances following death by prussic acid are not particularly well marked, but it may be well to bear in mind that reddening of the stomach is not unusual among them. Extravasation into the walls of the viscus has also been noticed, and a case recently occurred where this led to an awkward mistake. Our readers may remember the case of Madame Mertens, who was found dead in Fontainebleau forest. Madame Frigard was tried and condemned for murdering her, which it was supposed she had done by kneeling on her stomach, and at the same time compressing her trachea, or in some other way arresting respiration. The condition of the lungs was said to have aided the experts in coming to this conclusion, but the weather being warm the body was partially decomposed before it was discovered, and it is likely that the putrefactive gases below the pleura had been mistaken for the result of ruptured lung cells, a mistake we have seen committed before now. From this and the existence of an ecchymosis in

the front of the stomach, the mode of death above alluded to was assumed. After her trial Madame Frigard confessed to having poisoned her victim with prussic acid. The case is one of great importance. The other abdominal organs besides the stomach usually suffer from venous congestion. Considerable reliance has been placed on the odour exhaled by the corpse as a means of detecting prussic acid, but we cannot decide from this that prussic acid is present either by itself or as oil of almonds, for, supposing the smell to be absent, the acid may have been fixed by antidotes given, or may have been exhibited as cyanide of potassium, whilst, on the other hand, the smell of nitro-benzole very closely resembles that of prussic acid, and, in fact, is nowadays extensively used instead of it in perfumery. As to the detection of prussic acid for medico-legal purposes, one or two points ought to be noted. In the first place, owing to its great volatility, it may not be detected in the body of one who has undoubtedly died from its effects; and in the second, owing to its presence in certain articles of food and drink, it may sometimes be detected when no prussic acid has been given with murderous intent. Thus, it is present, though in small quantity, in the kernels of several of our stone fruits, as the apple and the peach, in almond flavour used for cooking, as well as in certain liqueurs, as noyeau, maraschino, and notably in kirschwasser. As to the processes by which it may be separated from foreign substances, its volatility supplies a ready clue to their nature, but it is in most cases necessary, after ascertaining whether any free acid exists, to add to the suspected substances some body capable of separating the acid from the bases with which it may be combined. Strangely enough, Dr. Taylor says nothing on this head. A remark made by Tardieu is of some importance; he tells us that strong sulphuric and hydrochloric acids, especially with the aid of heat, rapidly transform hydrocyanic acid into formiate of ammonia, but that phosphoric acid, which he consequently recommends, has not this property. He also recommends nitrate of silver instead of caustic potash for the absorption of the volatilised acid. The silver, sulphur, and Prussian blue tests are too well known to require any special notice. Poisoning by cyanide of potassium and by the essential oil of bitter almonds Dr. Taylor discusses apart from prussic acid, although they have much in common; in fact, death from the former usually differs from the ordinary form of poisoning by hydrocyanic acid, inasmuch as insensibility and death come on rather more slowly, and there is, consequently, more time for the exhibition of antidotes. There is, however, one form of poisoning by cyanide of potassium which is of the very utmost consequence, from the readiness with which it may occur. Among

photographers this substance is extensively employed for dissolving up an excess of precipitated silver, or removing the stains of lunar caustic, and the following case shows what evil results may follow its use in this way. Davanne mentions the case of a photographer who, in washing his hands with cyanide of potassium, allowed a small morsel to slip under his thumb-nail. At first he paid no attention to the circumstance, but by and by the part became painful and he became giddy. To get rid of the offending body he, unfortunately, betook himself to some vinegar, which set the prussic acid free from its combination with the potassium; rigors and intense prostration, loss of speech, cold extremities, and double vision followed, but he ultimately recovered. The frequent use made of the substance in our time renders it important that people should know of this liability to accident, as well as that it tends to soften and abrade the cuticle from its mere alkalinity, thus rendering absorption of the poison through the skin more likely to take place.

The following chapter is devoted by Dr. Taylor to a consideration of the toxical actions of such substances as naphtha, fusel oil, benzole, nitro-benzole, and aniline. The two last are by far the most important, several instances of death or injury from their effects being already on record. We need not, however, pause to consider the subject, as it has already received ample attention at the hands of Dr. Letheby. Sonnenkalb has also written a work on the subject of aniline and the aniline colours, from which Dr. Taylor's facts have to a considerable extent been derived. The actions of the substancess which next follow are of greater importance—these are alcohol, ether, and chloroform. The confusion which is apt to arise between the effects of alcohol and those of opium has already been noticed. The only other points which we consider of sufficient importance to be referred to here are the appearance of intense congestion usually encountered in the stomach, and the existence of alcohol in the ventricles of the brain, after death from its immediate effects. In connection with chloroform, again, it is too often forgotten that death may follow its internal exhibition, as well as the inhalation of its vapours. The latter is, unfortunately, only too common, but the former has not been very frequently observed. Dr. Taylor has mentioned one or two instances of this form of poisoning, and another which he has not given has been reported by Dr. Darling from Australia. The process which Dr. Taylor gives for the detection of chloroform is extremely simple and sufficiently delicate for all practical purposes, but he seems to think that chloroform cannot be readily separated from the blood or tissues of one who has died from its vapour. We think we can recall one case where this was done, and that among the very first deaths brought about by its agency.

With these last Dr. Taylor classifies the somewhat diverse substances, camphor, tobacco, cocculus, &c. The fact that camphor is a poison is apt to be overlooked, for the instances in which it has produced fatal or even dangerous effects are not very numerous, whilst it is so often prescribed and swallowed that people have almost come to regard it as innocuous. Knowledge to the contrary is all the more important because during the last cholera epidemic a nostrum termed "Ruspini's specific," consisting of a saturated solution of camphor in alcohol, was extensively used as a remedy for the disease. It was also at one time used as preventive to contagion from fevers and such like disorders, not always with impunity, as a case narrated by Leroy d'Etoiles shows, its use in this way being followed on one occasion by paralytic symptoms. Curiously enough, Dr. Taylor commences his description of its effects by narrating *a fatal case ending in convalescence*. What he means we cannot pretend to say; at all events, in this curious case, dangerous results seem to have followed the exhibition of twenty grains *in solution*. This Dr. Taylor thinks the smallest dose which has produced such an effect; but there is an instance recorded by Mr. Fenerley where death followed after three days the exhibition of *twelve* grains. On the other hand, ten drachms have been given in the form of enema without causing death. It always acts more powerfully when in solution than in the solid form, it being but slowly absorbed when in the latter condition; in this latter form it may cause death by gastro-enteritis alone, without any other symptoms. Why Dr. Taylor should place it among his cerebral poisons we cannot comprehend, for most writers affirm that its principal action is on the spinal cord, Majendie classifying it as a tetanic poison. It is one of those substances which seem to affect the lower animals more than it does men, whom, as we have seen, it affects most unequally. It seems to be principally evolved by the breath and perspiration; in one case the smell was noticeable in the latter secretion three weeks after it had been swallowed.

Poisoning by tobacco is a subject of very great interest, as it was for the purpose of detecting its active principle, nicotine, in the famous Bocarmé case that Stas invented his celebrated process, completely revolutionising the chemical detection of alkaloids. The case itself is also of unusual interest. Again we have to object to Dr. Taylor classifying this substance with opium; their actions are totally dissimilar. And here it may not be out of place to say a few words on the subject of Tardieu's classification, which is quite a new one. As before stated, however, he deals with poisoning, not with poisons; and, consequently, his groups are—

1.	Poisoning by irritant and corrosive substances .	Acids, alkalies, iodine, drastic purgatives, arsenic, mercury, or copper.
2.	„ depressing (*hyposthenisant*) substances	Antimony, oxalic acid digitalis, &c.
3.	„ stupefying substances . . .	Lead, carbonic oxide, chloroform, belladonna, tobacco, fungi, &c.
4.	„ narcotic substances . . .	Opium.
5.	„ neurosthenic substances . . .	Strychnine, prussic acid, aconite, cantharides, camphor, alcohol, &c.

Much may be said in favour of this arrangement as a step in advance, but as a final resting-place we cannot accept it. The whole group of the Solanaceæ are to be looked on with suspicion, but tobacco is probably the most virulent of them all; and considering the readiness with which it can be obtained, it is really wonderful that it is not used more frequently for suicidal or homicidal purposes. Its strong and characteristic taste in most instances unfit it for the purposes of the murderer, but there is reason to believe that it is not infrequently used for *hocussing* half-drunken men. One fatal case of this nature occurred in the practice of Dr. Ogston (*vide* Christison).

One of the best-marked cases of poisoning by tobacco occurred in the case of a lunatic sailor, narrated by Skae (see also Taylor 'On Poisons,' p. 746). In this instance well-marked vomiting and purging were observed. The old tobacco enema was not unattended with danger, as is shown by a case narrated by Eade, where death followed the exhibition of a clyster containing the product of nearly half an ounce of shag tobacco. A somewhat curious instance of the effects of tobacco smoking are given by Tardieu from an old journal. A man who had been a soldier and who smoked, but not to excess, made a wager he would smoke twenty-five pipes in immediate succession. He did so, but soon became insensible, and it was not till eighteen months had elapsed that he completely recovered from the effects of his foolish conduct. Of the less common alkaloïds, nicotine has been more frequently employed for destruction than any other, probably on account of the prominence it acquired in the trial of Bocarmé. Dr. Taylor gives an instance of its having been employed for suicidal purposes, and Fonssagrives another, but the symptoms antecedent to death were not observed in the latter case. Dr. Taylor says that this poison, like prussic acid, produces no convulsions. We have already shown good grounds for rejecting this dogma as to prussic acid, and we believe that the same may be said of nicotine. All experiments confirm this view, whilst opposed to it is the solitary case cited by Dr.

Taylor. Indeed, Kölliker states from the result of his experiments that, whilst it paralyses the brain, and through the motor nerves the voluntary organs, it excites the spinal cord and gives rise to tetanic spasms. The sensory nerves and the muscles themselves are unaffected. A most interesting class of cases are mentioned by Tardieu, but not by Dr. Taylor; these are certain instances where tobacco applied to sound skin has produced toxical effects. This occurrence has mostly happened in the case of men trying to carry a large quantity of tobacco without paying duty on it. There are besides many instances of bad effects having followed its use when applied to a denuded surface, as is not uncommonly done by ignorant persons, especially sailors. The empyreumatic oil of tobacco which collects in the stems of tobacco pipes contains a certain proportion of nicotine; but its peculiar effects are the intense irritation which is produces when applied to any surface. One case is worthy of narration. A sailor had long suffered from toothache, one of his teeth being carious and hollow; to relieve it he introduced into the cavity a quantity of oil from his pipe, which set up such intense inflammation that destruction of one half of his jaw was the result. Another fact worthy of notice has been elicited, viz., that the great majority of cases of idiopathic amaurosis, that is to say, without preceding neuritis, are ordinarily met with in men who have been great smokers, whilst many maintain that the stimulation leading to epithelioma of the lip or tongue is produced by the empyreumatic oil or the pipe-stem in the case of smokers.

With regard to the detection of nicotine we view Dr. Taylor's observations in a mixed light; of his process for separating it from organic matters we can speak well. It is excellently adapted for getting rid of that most obnoxious element—animal fat—the bugbear of all practical toxicologists, and is much simpler than that recommended by Tardieu. It must be remembered that nicotine is for an alkaloïd unusually soluble in water, totally differing in this respect from conia, and, being volatile heat must be applied to all liquids containing it with great caution, except it has been first neutralised by an acid. Herapath was, if we mistake not, the first to point out the process for separating the substance which rests at the basis of Dr. Taylor's plan. If any substance containing nicotine be treated with caustic potash, the alkaloïd is set free and dissolved in the water, from which it can in turn be readily separated by agitation with ether or chloroform, and on the volatilisation of these is left in a state of purity. One very important test for distinguishing nicotine from ammonia is given, but not very clearly, by Dr. Taylor. Iodine dissolved in water by means of iodide

of potassium gives with nicotine a brown precipitate, becoming crystalline; he says ammonia does not give such a precipitate. We have, however, seen a precipitate so produced; whether from impurities or not, we will not say; certainly however, a precipitate, but not crystalline. Another test he overlooks. If gaseous chlorine be passed through a cold solution of nicotine it produces an intense red, with ammonia it gives off nitrogen. This is a test of great importance, but the other tests given are of no great moment.

Closely allied in many respects to tobacco is hemlock, although Dr. Taylor places them in totally different groups. We shall, however, here bring them into closer proximity, for the sake of bringing out peculiarities which the other method does not so effectually accomplish.

Murder by means of umbelliferous poisons, notwithstanding their ready accessibility, is far from common, most of the deaths which have followed their use having been the result of accident, from one plant having been mistaken for another. The plants which have thus proved fatal have been *Conium maculatum, Cicuta virosa, Æthusa cynapium,* and *Œnanthe crocata,* the last being probably the most dangerous of all. A case given by Bennet of Edinburgh, and transcribed by Taylor, affords probably the best specimen we possess of the effects of conium. The paralysing effects of the poison were in this case extremely well marked, but the patient appeared to be conscious up to the last. Convulsions are commonly observed when hemlock is given to animals, and they have been seen in men, but only for a short space of time. Death always follows rapidly. Dr. Taylor tells us that this poison acts chiefly on the spinal marrow. With this we both agree and disagree, for the poison seems to vent its intensity on the motor system of nerves, leaving the sensory nerves and the muscles themselves comparatively unharmed (Kölliker). Ihmsen and Schroff have carefully analysed the effects of conia, and the latter has contrasted them with those of other substances. He finds that it most closely approximates to nicotine in its effects; it differs from the alkaloïds of the Solanaceæ in giving rise to intense muscular weakness, and resembles veratria in stimulating the spinal cord to convulsions. The nature and properties of the active principle of conium are from these researches tolerably well known, but with regard to the others enumerated above this is not the case. That they do contain some powerful principle is evident, and that this has many characters in common with conium is also plain. Indeed, Tardieu speaks as if conia was the active principle common to them all. Schroff has shown that hemlock contains an irritant substance as well as a sedative one, and it is just possible that the slight variety in the symptoms ob-

served in connection with each poisonous umbellifer depends on the relative amount of these two. The family likeness observable in their effects is unmistakable. Dr. Taylor mentions that he has been unable to obtain any precipitate in an infusion of *Œnanthe crocata* by means of his favorite reagent, the chlor-iodide of mercury and potassium, an objection which may, however, be got over on closer investigation. Owing to the resemblance which exists between the root of this last vegetable and that of the parsnip, accidents of a fatal character have not infrequently occurred from mistaking the one for the other. Thus, in 1843 a number of convicts died from its effects at Woolwich, in another instance as many as thirty-six soldiers perished in the same way. Dr. Taylor tells of the death from it of two men in Durham; and some of the seamen of H.M.S. Wellington were poisoned by the same substance. The rapidity with which death follows the exhibition of this vegetable is quite appalling. Nicol relates a case in the 'Association Journal' where a person died in an hour after eating of its root. Vomiting and convulsions were observed in this case. There is probably more of an irritant character associated with œnanthe than with conium, for fingering the roots is enough to set up inflammation in the hand. Mishaps have also arisen from confounding *Æthusa cynapium* with ordinary parsley, notwithstanding their dissimilarity when closely contrasted.

With regard to the detection of conia little need be said, for it comes but seldom before a court of law; indeed, there has been but one trial (resulting in acquittal) for its exhibition. Hemlock has, however, been used; when taken in substance, the leaves and the strong smell it evolves, when treated with caustic potash, would suffice for its detection. Dr. Taylor recommends taking advantage of its volatility in separating conia from any organic mixture; probably, however, the process he recommends for nicotine would be preferable. It must not be forgotten that, although conia is almost insoluble in water, yet along with a small quantity of alcohol it forms a clear mixture with that fluid. Connected with the reactions of conia, Dr. Taylor tells us one or two things which rather astonish us. In the first place, he states that neither of the three ordinary mineral acids affect conia; in this we think he is mistaken. Gaseous hydrochloric acid produces a deep purple colour, gradually passing into blue (probably the best test for the substance); nitric acid gives a bluish-red, and sulphuric acid, with heat, a purple red, but (and probably this accounts for Dr. Taylor's mistake) the acids must be very strong. Chlorine produces a white cloud in it, unlike the red colour produced with nicotine. Another test of considerable importance is the chromic acid solution, which, when heated

with conia, gives rise to butyric acid, readily recognisable by its strong and disagreeable odour.

Besides tobacco, other members of the family of the Solanaceæ, as is well known, possess dangerous toxical properties; all of them, however, seem to act more on the brain than the two we have last discussed. Hyoscyamus probably approximates more closely to opium in its effects than any other substance we know, if, indeed, we except the weakly essence, but belladonna differs in its effects from both. As a rule, hyoscyamus does not cause death; and although there have been a good many instances of its giving rise to symptoms of acute intoxication, not many deaths have been recorded. Among the most recent instances of its effects are certain cases recorded by Leistner, Möller, and by Zamboni, in which evil consequences followed the use of the seeds and of the roots. Schroff has carefully studied the properties of this plant, and holds that in most respects it comes nearest belladonna, but does not so powerfully affect the sphincters; it acts more as a soporific. When locally applied, its active principle affects the pupil more speedily than even atropia itself, whilst the scarlet eruption which sometimes accompanies the use of belladonna but seldom follows the exhibition of hyoscyamus.

Belladonna and its active principle, atropine, are of considerable importance from a toxicological point of view; not that either is often employed with murderous intent, or even for suicidal purposes, to any great extent, but, like some of which we have had occasion to speak, frequently giving rise to dangerous effects, whether taken by mistake or in excessive quantity. The frequency with which they are employed for medicinal purposes renders accidents of this nature all the more likely; indeed, there is an instance on record where the application of a belladonna plaster to the spine, the surface being highly abraded, produced symptoms of poisoning. A somewhat similar, though not so striking case, is recorded by Dr. Plors, of Leipsic; in this instance atropia was applied to a blistered surface for the relief of sore throat; symptoms came on in a few minutes, and the patient was dead in two hours. The face became swollen, respiration was quick, then intermittent, and the pulse rose to 150. The power of speech was lost, and swallowing soon became difficult; choreic convulsions were noticed. Probably the most common mode of poisoning by belladonna is from children mistaking its tempting berries for wholesome fruit; in fact, during the summer of 1846 wholesale poisoning from these effects took place in London, the berries being hawked about in the streets. Dr. Taylor narrates the occurrence, and describes the appearances observed. One of these should not be forgotten—the

dying of the lips and mouth of a dark colour by the juice of the fruit. Kürmer narrates a similar case, and mentions that the heart in this instance was empty and the vagi reddened; no congestion of the stomach was observed. Mr. Leach has narrated a case of poisoning by atropia, but it presents no point of special importance. The supposed antagonistic effects of opium and belladonna have already been alluded to. Since that notice a case has been reported in an American journal in which a very large dose of sulphate of morphia was taken, yet the patient recovered by the use of atropine. A case of poisoning by a combination of atropine with morphia is, however, reported, so that we cannot accept this antagonism as a thing absolutely proved. There are one or two points still to be noticed in connection with belladonna, and one of these came to assume some little importance in a trial not long ago. A surgeon was accused of having poisoned certain of his relations by belladonna exhibited in a rabbit pie. Now, it so happens that on these animals belladonna exerts little or no influence, and it was contended that the animals might have eaten belladonna before their death, and thus account for its presence in the pie. The trial fell to the ground; but now-a-days, when experiments on animals are looked upon as one of the regular agencies in the detection of crime, this peculiarity of rabbits should not be overlooked. Another thing to be borne in mind as to its symptoms is the almost invariable occurrence of delirium; in fact, notwithstanding the order in which Dr. Taylor has placed it, there are few poisons which exercise a more decided influence on the brain.

Closely allied in many respects to belladonna is another substance often used medicinally; and although with us Datura stramonium is but seldom employed with evil intent, in India it is one of the most important poisons with which the medical jurist has to deal; being there extensively used, not only for the purposes of murder, but still more frequently for robbery: the ingenuity of the poisoners or "hocussers" being too often sufficiently great to enable them to escape with impunity. Stramonium is not the only species of datura employed there; others, as the D. tatula, D. ferox, D. fastuosa, D. alba, and D. metel, growing abundantly, are often made use of. The seeds appear to be most frequently employed for the purpose of stupefying, and in some instances the seeds of other plants are found mixed with those of the poisonous datura.

The effects of stramonium and of belladonna are very much alike, as any one may see by reading Dr. Taylor's cases illustrative of the effects of the two. This similarity appears to be due to the alkaloïds which they severally contain—atropia in the one case, daturia in the other. Bouchardat would make out certain

differences in the chemical properties of these two; but Schroff, with whom we cordially concur, holds the two to be identical. Certain symptoms are said by Husemann to characterise the effects of stramonium, but we cannot altogether coincide with him in this opinion. He holds that the drug in question gives rise to more local irritation and to greater stimulation of the sexual organs than belladonna does, but the reported cases do not bear him out in this. One word as to the detection of the alkaloïds daturia and atropia, if they deserve distinct names, except from their effects on the pupil, which, however, are by no means characteristic of these substances only, no very good test has been brought forward for them. That blank was to a certain extent filled in the first of these notices, where we called attention to the peculiar smell of dried rose-leaves to which they both give rise when heated with sulphuric acid. A very small quantity is sufficient for this purpose. The odour also appears to have been noticed by Dr. Guy in his experiments as to the volatilization of alkaloïds without the addition of sulphuric acid but by the aid of heat alone. The alkaloïd may be separated according to the principle already laid down.

In his treatise on Toxicology, Tardieu deals with all the chief solanaceæ at once: thus including not only the well-known poisons belladonna, stramonium, and hyoscyamus (tobacco is handled separately), but also the more doubtful solanum nigrum. Dulcamara berries have repeatedly produced toxical effects, and one case of death from their use is mentioned in the 'Lancet,' for June, 1856. But the effects of the solanum nigrum would appear to be more dubious; it is said to have acted prejudicially on some swine in Bavaria, but that again has been contradicted. Certain it is, however, that solanum gives rise to symptoms of poisoning in dogs, whilst the activity of the leaves has been vindicated by two cases recorded by Magne and narrated by Tardieu. In this instance there could be no doubt as to the plant which had caused death in one child and severe illness in another. The only point of importance distinguishing its effects from those of belladonna would seem to be the greater amount of abdominal irritation caused by solanum; there being much abdominal tension with purging, attempts at vomiting were ineffectual, perhaps from the paralysis produced by the herb.

We have spoken of the nefarious purposes to which datura is often turned in India; but in former days, when Thuggee was more prevalent than it is now, a still more dangerous substance was employed with similar intent. Datura in the hands of a native poisoner rarely causes death; but this was not the case where Bikh or Bish was employed (Chevers). This poison is derived from the roots of some species of aconite, probably the

aconitum ferox, which is also used for poisoning arrows in some of the hill districts of India. A variety of arrow poison brought from China would also seem to be produced by some species of aconite (Christison). The effects of aconite are, however, tolerably distinct from those of any plant we have hitherto referred to, the tingling sensation followed by numbness which it produces is almost characteristic of the drug; but in placing the accession of these some half-hour after chewing the poison, Dr. Taylor would seem to be in error, as the peculiar taste may be brought out almost immediately. One way in which fatal accidents by this plant may be brought about is by mistaking its roots for those of horseradish; of course such a mistake could not occur were both plants in leaf, it is in winter that such mishaps are most likely to happen. In no instance should the two plants be allowed to grow near each other. A little care is enough to enable any one to distinguish the two roots; but prevention is better than cure, and it is preferable to render any such mistake impossible. In any case, should doubt exist, the tasting of a small piece will suffice to clear it away; if no tingling follows it is safe, and *vice versâ*. But tragedies like those of Tain are not invariably the result of accident. Aconite has frequently been employed for suicidal as well as for murderous purposes. One most instructive case of the latter kind has been narrated by the first of our Irish toxicologists, Dr. Geoghegan; but the most interesting is connected with the infamous Pritchard. It would seem that his mother-in-law (Mrs. Taylor) had obtained some inkling of his nefarious practices, and the slow process of poisoning her by antimony was on the spur of the moment abandoned for the more speedy effects of aconite. This he cunningly mixed with Battley's solution of opium, which the old woman was in the habit of taking, and in which he had already placed a quantity of antimony. This admixture of aconite with opium would in all probability have escaped notice had it not been for the painstaking researches of Dr. Penny, of Glasgow, to whom no small praise is due in the matter. The cause of Mrs. Taylor's death was no doubt a mixed one, still the influence of the aconite is clearly appreciable in the history of her symptoms. The means employed for the detection of the aconite was by physiological reactions on the lower animals, but it was only in the substance which remained over in the bottle that it could be discovered. *In the body neither Dr. Maclagan nor Dr. Penny could discover a trace of opium or aconite.*

Of late years, the mode of detecting poisons above referred to has been coming more and more into fashion; but it is in connection with the substance we next propose discussing that it has assumed the greatest importance. Digitalis, although

not unfrequently exciting dangerous effects when its use as a medicine had been persevered in over a too protracted period, or to too great an extent, as well as occasionally producing fatal effects when taken accidentally or with suicidal intent (and the same might be said of digitaline), was unknown as an implement of murder until 1864, when the now notorious case of De la Pommerais came on for trial. The medico-legal portion of this *cause célèbre* was intrusted to Tardieu and Roussin. Digitalis is by the former gentleman classed along with tartar emetic in his group of *hypothenisant* poisons, and certainly its depressing action on the heart is most remarkable. When given in large doses digitalis soon gives rise to vomiting, intense and almost insupportable beating of the heart, profound weakness, and irregular pulse. Curiously enough, there appears as in poisoning by santonine, a derangement of vision with regard to colour. Dr. Taylor tells us that in one case, at least, the blazing fire appeared blue to the poisoned individual, whilst santonine gives rise to a yellow tinge in all things observed. In poisoning by digitalis epigastric pain is one of the most notable symptoms; yet gastric irritation does not appear to be strongly marked after death. The nature of the active principle of digitalis has been disputed, some holding that the substance known as digitaline is a complex body composed of a variety of others; whilst some, again, hold that it is a simple substance, of the nature of a glucoside. This discrepancy of opinion is still more apparent when the characters and reactions of digitaline are considered; for we find one man assigning great importance to one reaction, which another entirely fails to verify. Again, we find that one specimen of digitaline has one colour, another has another; none, however, are crystalline: some specimens are tolerably soluble in water, others scarcely at all so; and this apart from the fact that, to obtain anything like a proper aqueous solution of digitalis, it is necessary to dissolve it in boiling water. Hydrochloric acid with digitaline gives a green colour; but the purer the glucoside is, the less is this tint manifested, although with even the purest a greenish tinge is obtainable. Nor is the test of M. Grandeau to be more relied on; he holds that, if digitaline wetted with sulphuric acid be exposed to the vapour of bromine, a violet colour is produced. But here again doubt steps in. No two specimens of digitaline give exactly the same reaction, whilst treated in the same way many organic substances give rise to a fugitive tinge of violet (Tardieu). Most striking, however, are the observations of M. Goethals, who examined four distinct specimens of digitaline, and found all to differ in their chemical and physical properties. The mode of preparation is doubtless at the bottom of this diversity,

but it is such as to indispose one for seeking any reliable chemical test for digitaline. Having decided that we cannot rely on chemistry alone for the detection of digitaline, it has next to be considered how best to separate it from organic matters such as one has to deal with in an ordinary case of poisoning. It is evident from its reactions, it having no basic property, that the modified process already given will be useless for this purpose, so that but two are left for selection; the one is dialysis, the other the plan adopted by Tardieu and Roussin. With regard to the former, although highly praised by several chemists, Roussin's researches decidedly negative its value, and in this respect we most cordially agree with him. For, from what we have seen, dialysis, however promising it may appear when dealing with factitious mixtures, is, in most instances, utterly useless when employed for the analysis of such complex matters as the contents of a stomach.

The plan adopted by Tardieu and Roussin was to shred the animal substances into extremely minute pieces, to mix the whole with pure alcohol, and allow the substances to simmer over a water-bath for twenty-four hours, to filter and wash the substances left in the filter with boiling alcohol, to concentrate and repeat the process until a tolerably pure substance was obtained. With this they proceeded to make certain experiments on animals. This mode of analysis, although specially intended for digitalis, may with judicious alterations be rendered available for other substances; for all are not so soluble in alcohol as digitalis is. The great advantage it presents, with reference to the plan of investigation to be subsequently adopted, is that no deleterious ingredient is introduced into the suspected mixture. Again, when animals are to be experimented on, it comes to be a matter of some moment to determine how the substance is to be introduced into their systems. Dogs, rabbits, and frogs are most frequently selected; but, from the facility with which the former of these can vomit, it occasions a great waste of material to attempt to introduce any of the suspected material by their mouths. On the other hand, the process of hypodermic injection is easy, certain in its results, and requires a much smaller quantity of material than the others; it should therefore be adopted in all such researches. When, in experimenting with digitalis, the animal's heart is examined some hours after death, the ventricles are found contracted, and the auricles distended with black fluid blood; if at an earlier period, all the cavities are distended, for post-mortem rigidity soon sets in, and electricity loses its power of stimulating the organ. We have entered into this subject at greater length than we otherwise would have done had it not been for the great importance of the questions raised

at the trial of De la Pommerais, and also because the procedure adopted in this case may with similar results be employed with other substances having no very distinct chemical reactions.

Soon after the trial referred to Drs. Stevenson and Fagge, of Guy's Hospital, undertook a series of researches on the subject of poisoning by digitalis. They arrived at the conclusion that the only substances which give rise to symptoms and appearances similar to those produced by digitalis were squill and hellebore. The whole group of the Ranunculaceæ has of late years been attracting considerable attention, and certain investigations by Marmé have already superseded the researches of the gentlemen mentioned above, as far as the ranunculus family is concerned at least. Schroff some years ago settled that in the hellebores were two principles, a narcotic and an irritant; these Marmé has separated and named helleborin and helleborëin respectively. The Helleborëin acts very much in the same way as digitaline, but the helleborin in some instances causes death by inflammation alone. The former attacks the heart, the latter the nervous system and the bowels, giving rise to the purgation ordinarily observed. Other powerful poisons have been separated from plants belonging to this group, one of the most virulent of these being anemonin from pulsatilla. Delphinin is also a very active substance.

Somewhat allied to these in action are veratria and colchicia, the active principle of stavesacre. Dr. Taylor ranks these substances as irritants, a classification in which we cannot altogether concur. The most important bearing colchicum has is in connection with the well-known case of Catharine Wilson, rightly or wrongly hanged as a wholesale poisoner. Roux has reported in the 'Union Medicale' five deaths from the use of colchicum wine, which was mistaken for vinum cinchonæ, diarrhœa with vomiting, cramps, and retention of urine were observed. The bladder was found covered with red spots, and the brain with its membranes were injected. Accidents with veratria have not been uncommon, although Dr. Taylor does not mention many; another thing he has overlooked is any mention of the peculiar purple produced by the action of hydrocloric acid on this substance. Since the publication of his work, that gentleman has been engaged in a dispute as to the means of detecting veratria, in which he seemed to rest his opinion chiefly on the effects of the chlor-iodide of potassium and mercury on a suspected solution. We have collected many valuable facts as to the toxicology of these less known poisons, which want of space prohibits our making more public. Year by year our knowledge in this department is increasing, and there is really now no English book on toxicology which does justice to the subject.

One group of poisons we propose discussing collectively, notwithstanding a considerable diversity in their effects: we mean the arrow poisons used in different parts of the world, most of which are derived from plants belonging to the natural family Loganiaceæ, to which group nux vomica also belongs. These arrow poisons are to a certain extent allied to substances used in barbarous countries in a form of trial by ordeal; such are the Calabar bean and the recently described Akazga, for the drugs used in the one way are sometimes used in the other also. The great representatives of this group are strychnine and curare, they are also the best known. The many stories as to the wonderful effects of curare-brought by travellers from South America soon induced scientific men to examine its mode of action. Strange to say the mode in which the effects it readily superinduces are brought about has not been satisfactorily ascertained, consequently greater attention has been paid to it than to any of the others, the brilliant lectures of Claude Bernard probably contributing more to this than anything else could have done. Strychnine, again, chiefly owes its celebrity to the notorious case of Palmer. The relative actions of these two are even now unsettled. Dr. Taylor's chapter on the subject of strychnine is, we cannot but think, reprehensible. Engaged on a most important case at a period when comparatively little was known of the action of the poison, he was almost compelled to form strong opinions on, comparatively speaking, scanty grounds. He has been much and we think unjustly blamed for his manner of conducting this case, but nothing can, we think, justify the tone and manner he assumes in the work referred to. We must, in our position as unbiassed critics, condemn the conversion of a text-book into a weapon of personal attack.

Strychnine is ordinarily supposed to cause death by exciting the motor nerves, curare by paralysing them; in reality neither seem to affect that system of nerves; or, if Martin Magron's researches are to be trusted, both seem to paralyse or obstruct the outward stimulus communicated to the motor nerves. The centres appear to be the parts specially affected; with strychnia they become unusually sensitive of impressions from without, which accounts for the fact invariably noticed, that the slightest movement about or in connection with the patient suffering from the effects of strychnine gives rise to convulsions. Dr. Taylor alludes to this circumstance, but gives no explanation of it. It will be seen that it has a valuable therapeutic bearing; the most violent effects of strychnine do not last very long, and if by any means the convulsive attacks can be postponed for a period the patient will be saved. In the con-

vulsions, indeed, lies the patient's great danger; for we are persuaded, from the post-mortem appearances we have observed both in men and in animals, that in many cases death is brought about by asphyxia superinduced by the spasmodic arrest of respiration during the convulsive attacks. Dr. Harley, on the other hand, holds that oxygen is not converted into carbonic acid whilst the patient is under the influence of the drug. Tardieu also disbelieves in death being superinduced by asphyxia, and says that the heart is most frequently empty. In one of the best marked cases of poisoning by strychnine which could come under one's notice we distinctly observed the right side of the heart enormously distended, the left comparatively empty. The other viscera presented the appearances observed in asphyxia. Again, with regard to the stomach, both Drs. Taylor and Tardieu state that the poison does not affect it. In certain cases we have observed that organ distinctly and unmistakably inflamed when no poison had been taken, with the exception of strychnine. A question of some importance is the power of strychnine to affect the nervous system when applied directly to any of its branches. Dr. Harley holds that no effect is thus produced, but the experiments of Martin Magron have led to an opposite belief. With regard to curare, also, it has generally been held that when taken internally it is inactive, its effects only being evolved by direct introduction into the circulating fluid. The same gentleman has shown that this doctrine is inaccurate, for if a sufficient dose be given curare may cause death either way. The latest researches connected with this wonderful poison have been conducted by Voisin and Liouville. These gentlemen, examining the reactions presented by curare, came to the conclusion, that should a case of poisoning occur we must trust to physiological rather than to chemical science for its detection. The peculiar effects it produces in the animal economy are of a most interesting character, and are most vividly delineated by Claude Bernard in an article published some time ago in the 'Revue de Deux Mondes.' Voisin and Liouville recommend Roussin's process already referred to for the separation of the poison from organic substances. Should curarine be separated it may, they say, be distinguished from strychnine by the blue colour sulphuric acid produces with it. Curiously enough, the active principle of akazga (the ordeal poison described in the last number of this Review by Dr. Fraser) appears to give exactly the same reactions as strychnia, although its properties are remarkably different.

Dr. Taylor tells us that strychnine is sometimes employed for tainting arrows, that they may become poisonous, but there are many other poisons which are so used, and with each different

tribe it may be said that a different substance is used. This may account for the great diversity of opinion as to the mode of action of the so-called curare brought from South America. In almost every country which remains in a state of barbarism a kind of arrow poison is found, sometimes derived from one member of the vegetable kingdom, and sometimes from another; although the majority, as above mentioned, are derived from the Loganiaceæ. There are in South Africa certain kinds of drugs used in this way; they are usually derived from species of amaryllis and rhus (Van Hasselt), but it is chiefly in Asia and South America, according to our present knowledge, that such drugs are employed. We have already mentioned that in China a species of aconite is made use of for this purpose, but in Borneo a poison altogether different is employed. It is termed dajacksch, and has been examined by Braidwood under the superintendence of Du Bois Reymond. From a paper by the gentleman just referred to we learn that this poison acts chiefly on the heart, annihilating its powers of contraction before sensation or motion is elsewhere impaired. When the nerves are attacked, the sensory seems to give way before the motor system, the respiration of the animal experimented on becomes spasmodic, the pupils are contracted, and death ensues. (*Vide* 'Edin. Med. and Surg. Journal.') Tanghin and antiar, the former belonging to the Apocyanaceæ, the latter to the Logamaceæ, appear to be used in a somewhat similar way, although details as to the former have chiefly reached us in the shape of the results of ordeal in Madagascar. According to Kölliker, who has carefully examined the action of antiar, it first affects the voluntary then the reflex functions of muscles; but the heart is the organ first affected, and the muscles themselves appear to be affected before the nerves which supply them. With regard to tanghin which has been considered by Kölliker and Pelikan, they state that it does not excite tetanic convulsions, but that it affects the heart as soon without the medulla oblongata and the spinal cord as when they exist unimpaired. The arrow poisons of South America are generally supposed to be some variety or other of curare; in reality, however, among some tribes, substances containing picrotoxine instead of curarine or strychnine are employed. We have refrained from saying anything as to the action of picrotoxine, as notwithstanding the researches of Von Tschudi its mode of action is far from being thoroughly understood. The substance to which it most nearly approximates is strychnine, but being a glucoside and a neutral or almost an acid body, its reactions not only differ considerably from those of the alkaloïd referred to but are also of a somewhat indefinite character.

There are other poisons of which we should have spoken had space permitted, but we can only cursorily refer to one or two. Laburnum, although well known as an active poison, rarely if ever produces death; many cases are on record of its having proved dangerous especially to children, but the early vomiting which it seems to induce probably prevents fatal consequences. In connection with its chemical history, which is somewhat obscure, we may mention that Dr. Taylor has entirely overlooked the only paper which has been devoted to a close examination of the subject. It is contained in the 'Edinburgh Medical and Surgical Journal' for 1861-62, and is the work of a Mr. Gray. This gentleman came to the conclusion that laburnum contained three principles which he termed Laburnic acid, Laburnia, Cystinea. All, however, although reacting differently, seem to act to a considerable extent in the same way; they give rise to vomiting, cause sleep, and do not purge. His conclusions are not, however, very reliable, for there are some obvious mistakes of natural for morbid phenomena, and the chemistry is not very clear. Nevertheless, the paper deserves notice, as being almost the only one, apart from that of Christison, in our language, which is devoted to a scientific investigation of the subject. Yew and privet Dr. Taylor discusses last of all: we have but little to say of them; the importance of the former depends on its use as an abortive, a mode of employing it which often entails fatal consequences. The substance has been scientifically examined by Schroff, who curiously enough states that the berries are not poisonous; one case given by Dr. Taylor completely does away with this notion. Schroff holds that in its action it is closely allied to savine, both containing an irritant and a narcotic principle. Speaking of savine, we would refer Dr. Taylor to a curious case narrated by May in which this substance, caused death in forty-eight hours, but the grand peculiarity it presented was the perforation of the cul de sac of the stomach in two places; it is, however, rather doubtful whether this could have been the result of the poison itself.

One other group of poisonous substances and we have finished; but the importance of sundry of the gaseous poisons is too great to allow of their being entirely overlooked. The best and most recent work on the subject is from the pen of Dr. Eulenberg of Cologne. This writer subdivides them into such as produce death by excluding air, as nitrogen, hydrogen, &c., these having no direct deleterious influence upon the system. Secondly, the toxicæmic gases including those that stupify, as carbonic oxide, carbonic acid, and coal gas; those that irritate, as ammonia, chlorine, sulphurous acid and nitric oxide. What he terms the biolytic, or what some would call septic gases, include sulphuretted

hydrogen, sulphide of ammonium, the gas of sewers, arseniuretted hydrogen, phosphoretted hydrogen, and so on.

The two last enumerated have been referred to under the heading of the material which renders each dangerous; so that from one point of view the carbon compounds are probably the most important. Much has been written on this subject, particularly of late years, and no one has contributed more to our knowledge of the subject than the distinguished Claude Bernard. For a capital paper on the action of carbonic oxide, probably the most interesting of the group, we may refer our readers to the May number of the new 'Journal of Anatomy and Physiology.' It is from the pen of Dr. A. Gamgee, a rising toxicologist, and contains most of the facts it is desirable to know in connection with the subject. Not the least valuable portion of Eulenberg's work is a plate exhibiting the peculiar colours the blood exhibits after poisoning by certain gases. Probably the most striking of these is the bright red tint produced by carbonic oxide, notably contrasting with the dark hue consequent on the absorption of carbonic acid. This colour appears to depend on a combination of the carbonic oxide with the hæmoglobin of the blood, and forming with it a permanent compound of a more stable character than that produced with the bodies ordinarily combining with hæmoglobin, viz., oxygen and carbonic acid. This compound resists the absorption or evolution of oxygen, expelling a portion of the oxygen and taking its place, so as to render the blood useless as a means of conveying oxygen to the tissues. When hæmoglobin is in its normal condition, it may exist either in a highly oxygenized state, or in one where less oxygen is present. The normal hæmoglobin gives with the spectroscope two well marked and defined bands, but in its oxidized form only one of these is present. Carbonic oxide prevents the conversion of the one into the other; the blood containing less oxygen than usual, cannot part with as much, hence also hæmoglobin cannot readily be reduced to its less oxidized form. These peculiar changes Dr. Gamgee holds to be constant, and to furnish us with the means of detecting death either by carbonic oxide or by charcoal, forms which give rise to like appearances. Not, however, invariably, for a dark colour of the blood has sometimes been noted after death from this latter cause. Nevertheless, a florid hue of the upper portions of the body may to a certain extent be considered indicative of this mode of death by charcoal fumes. Of the other gases we cannot now speak, our space forbids. Suffice it to say that their special mode of action is as yet comparatively unknown, and researches in this direction would probably conduct to a rich mine of knowledge. We should under this heading have referred to

certain accidents which have occurred from aniline vapours, turpentine vapours, &c.; for strictly these substances should be treated as gases when they cause death by being respired. It is, however, more convenient to deal with them elsewhere.

Having now finished our survey of the toxicological portion of Dr. Taylor's work, we are in a position to estimate its value aright. In so doing the mode of its appearance must be borne in mind, it constitutes part and parcel of a work on medical jurisprudence, many of the details which give value to a work on toxicology, may therefore be omitted. Not so in a book entirely devoted to poisons; and nowadays, when the reactions of poisons on the lower animals are assuming such an importance, it comes to be a matter of necessity that something more than a detail of symptoms as observed in man should be given. In this respect, Dr. Taylor's works are defective; as related to medical jurisprudence they are good, although some important points have been overlooked, but from a scientific point of view they do not furnish types of excellence. It must, in fact, be confessed that at the present moment we have no English work which represents the advanced state of toxicology, and we, alas! can hardly hope for a new edition of that best of all works on the subject—Christison 'On Poisons.' Much has been done both in this country and abroad; we have striven to pick up some of the crumbs of knowledge which have fallen from a master's table, but at best we can do but little, our means are limited; and if we have supplied a faint sketch of the toxicology of to-day, it is the utmost which could be expected of us. The subject is, however, in its infancy, but the important applications many substances hitherto looked on as dangerous in the extreme, are daily receiving, leave us room for hope that in course of time their effects will be better understood.

To aid the officers of the law in the detection of crime is the duty of the medical jurist, but the toxicologist must in certain instances assume the part of the physiologist as well. There are many substances which have never been employed for the destruction of human life, yet the study of their action on the animal economy may be a matter of great interest, giving us an insight into the processes of life not otherwise attainable. For these and similar reasons we could desire that toxicology were more actively studied in England, but we also fear that the practical tendencies of our countrymen will ever leave such investigations to be conducted by a select few.

Review III.

Die Lehren vom Syphilitischen Contagium und ihre Thatsächliche Begründung. Von Dr. Heinrich Auspitz, Docenten für Hautkankheiten und Syphilis, an der K.K. Universität in Wien.

The Doctrines of the Syphilitic Contagion and their Actual Foundation. By Dr. H. Auspitz, Lecturer on Skin Diseases and Syphilis in the University of Vienna. Vienna. Braumüller. Pp. 384.

Few subjects have of late years received more attention than syphilis, few deserve this in a greater degree, and we are glad to say that, although our knowledge of the subject is yet very imperfect, still the study of syphilis has not been without most important results. But, to advance our knowledge in any remarkable manner, it is necessary to limit our attention to one particular department, for the whole subject of syphilis has even now acquired most unmanageable dimensions, and we are therefore inclined to think that Dr. Auspitz has done well in limiting his attention in the present volume to the question of contagion, seeing that it involves most of the theoretical questions connected with the disease. Nor, apart from the pathological results of syphilis, is there in all probability a more important or a more interesting field of study connected with the disease than this of contagion. So many men have already examined the question with, it must be confessed, such diverse results, that a careful and impartial estimation of the relative value of these various opinions comes to be almost an essential. Such an investigation Dr. Auspitz professes to make, his results we shall lay before our readers, and allow them to judge of the success which has attended his labours.

With that prolixity, that ponderous yet thorough-going style of treatment so dear to a German mind, Dr. Auspitz introduces his theme by an inquiry as to the various kinds of contagious disease to which the human being is liable, and by settling the relation which syphilis bears to each, then setting about inquiring wherein lies the essence of its contagiousness. This question, like many others, is far more easily put than answered. But although a direct reply cannot be given, still certain causes which may have been supposed to have some influence on the propagation of the disease are now shown to be of no potency. Thus the discovery of Donné of the existence of certain animalcules in the pus of chancres was at one time supposed to be of the greatest significance; but subsequent investigations, by demonstrating their existence in other fluids, and the occasional

innocuousness of pus containing them, definitely settled the importance to be assigned them. Nor has chemistry been more successful in making out and separating the syphilitic virus; for that there is a virus no one now seeks to deny, although the school of Broussais long maintained that the secondary symptoms were merely the consequence of the local lesions, constitutional reaction following in the train of the ulcers as they would in that of any other injury. Chemistry has been able to demonstrate certain abnormal conditions as existing in the blood of patients labouring under syphilis, but nothing very diverse from what may be found in other exhausting diseases: thus, the red blood corpuscles are notably diminished in number, and the quantity of albumen present is augmented, whilst that of the fibrine remains unaltered; in mercurialism, on the other hand, the amount of the albumen in the blood is diminished.

But although the essential nature of the syphilitic virus is unknown, as is, indeed, that of other contagious disorders, still many points in connection with the propagation of the disease are well understood—the vehicle, the mode of communication, the immediate result, and the secondary consequences; but as these last are very far from being constant or invariable, sometimes appearing in one form, sometimes in another, of all degrees of severity, attacking very different organs in different individuals, and, it must be said, lasting various periods in various constitutions, it comes to be a matter of some importance to settle whether these various modifications depend on accidental circumstances connected with the individual affected, or are the constant product of different varieties of poison. In other words, we have to settle whether the syphilitic poison is simple or multiple; whether we have one or more distinct and non-interchangeable forms of disease.

There have probably been few doctrines which have in their time been more vehemently insisted on and more determinedly opposed than the unity and duality of the syphilitic virus. Experiment and argument have both been invoked, yet even now the minds of many men halt between the two opinions. To dispose of any such question more than one method is available: one may either limit himself to the observation of cases produced in the ordinary way, or, by artificially inducing the disease, be able to settle its conditions more exactly, or better still to combine the two plans of study. To enable himself to come to a correct conclusion on the whole subject of syphilitic contagion Dr. Auspitz has adopted what might be termed the historical plan, and has, accordingly, divided his work into chapters, each containing the history of syphilis during a certain period, treating each question in the order in

which it first made its appearance. The first chapter, accordingly, deals with syphilis in antiquity and the former portion of the middle ages; the second with syphilis during the latter part of the fifteenth and throughout the sixteenth centuries; the third treats more especially of gonorrhœa; the fourth of syphilis, as studied by Hunter and Ricord; the fifth and sixth of the doctrine of the dualists and unitarians respectively, whilst the last is occupied with a *résumé* of the whole subject.

To begin: Whether syphilis did exist during antiquity is at least a disputed question; some will have that it did, others that it did not; and no little pains and learning have been wasted on what after all is a most unprofitable discussion; certain it is, however, that towards the end of the fifteenth century the disease assumed proportions and characters which had they previously existed could not have failed to attract attention. Excoriations and simple sores so produced were, no doubt, as common in early times as now; but the existence of secondary symptoms is rather more doubtful. We have heard of a youth, with more ingenuity than reverence, attempting to show that the disease under which Job laboured was secondary syphilis; whilst others, with no less obliquity of understanding, would have it that the disease which ravaged the hosts of Israel in the valley of Moab was of a similar origin. It has been thought that some of the ceremonials prescribed by Moses were aimed against the propagation of gonorrhœa, but we must descend to secular writers of a much later date if we are to form any definite and well grounded opinion as to the existence of secondary syphilis. The terms employed by the best known medical writers Hippocrates, Galen, Aretæus, &c., are so vague and general, and withal so indefinitely employed, that we cannot deduce any authoritative statement from their writings, but from one epigram of Martial (book vii, ep. 7), Auspitz thinks it evident that condylomata and their contagious character were well known. But it must be observed that the term here employed is *ficus*, which, from a quotation elsewhere given by our author wherein ficus and condylomata are both mentioned, appears to be also used to designate warts. Other lesions which are most frequently the result of syphilis are also alluded to by the writers of antiquity. Thus Plutarch speaks of joint affections and enlargements of the bones; while others allude to disorganisation of the bones of the nose: but in no case can we be absolutely certain that these were the consequences of syphilitic contagion.

The writings of the middle ages are more definite on the subject of venereal sores, and in the work of Valescus, supposed to have been written about 1418, we find hardness (inflammation, &c.)

mentioned as a specific character in certain intractable forms of the disease; but the connection between sores and constitutional symptoms has not, in our opinion, been fully made out. If we are to accept Pruner's notion, that towards the equator secondary syphilis tends more and more to assume an exanthematous character, rather than to attack the nose and mouth as in northern climates, we might account for this omission seeing that most of the writers on medicine in these days, Arab and others, dwelt towards the south of Europe. But who that has seen syphilis in a warm climate can agree with him? We must, therefore, demur to Dr. Auspitz's conclusion: he thinks that the existence of syphilis previous to the end of the fifteenth century has been clearly demonstrated, but with this opinion we cannot coincide. Simple venereal ulcers with complicating buboes were no doubt known; but for their production no specific virus is necessary, neither do they beyond what might be expected of sores in such a situation disorder the general health; the fearful consequences which followed syphilis within a few short years of this period were of a very different character, and how on Auspitz's view, are we to account for this sudden exacerbation? From the year 1490 up to 1494, we come across references to a venereal disease; but it was not till the winter of 1494-95, when Charles VIII was besieging Naples, that syphilis is ordinarily assumed to have broken out in its most malignant form. In this case there can be no doubt that typhus fever had more to do with the extraordinary mortality than syphilis had; although the mortality from the two diseases is ordinarily commingled. If hitherto it had been doubtful whether any secondary symptoms, properly so called, followed the appearance of genital ulcers, in the cases which now occurred there could be no such dubiety. Further, the disease which had hitherto prevailed was communicable by sexual connection only; such was no longer the case, and the malady spread with appalling celerity. It is curious to notice that from this origin it is almost universally known by the writers of the time as the *Morbus Gallicus* or *Neapolitanus*. These writers, also, almost all note the hardened bases of the ulcers and the rapid accession of general symptoms after the appearance of the primary sores, which symptoms would seem to have been of a severity not only unprecedented at these times, but actually unknown at the present day. How, then, are we to account for this extraordinary exacerbation of a comparatively speaking mild disease, or for the appearance of one hitherto unknown? Some will have it that the disease was entirely new and of spontaneous origin; others, that the old disease was combined with a new one; but the two theories in most favour are, that it was imported either from America or from Africa; the

other, that peculiar circumstances had modified the pre-existing disorder. From what we have already said, it might be anticipated that Dr. Auspitz inclines to accept the latter theory, but in this respect we are by no means disposed to agree with him. We have seen that it is far from certain that the venereal ulcers known before this time were ever followed by secondary symptoms as *we* speak of them; and Dr. Auspitz brings forward nothing to account for the suddenness of the appearance and the intensity of the character of these. On the other hand, we know that Columbus returned from his first voyage in 1493, and from recent researches of which Dr. Auspitz does not seem to be aware the objections which have hitherto been urged against this theory of its introduction, which was so vehemently maintained by Astruc are almost, in fact we might say altogether, disposed of. Certain it is, that within a very few years from the time we speak of, the disease was widely spread abroad throughout Christendom.

Of the sixteenth century we need say but little, of course the knowledge of syphilis was increased, and great improvements were effected in the mode of treating it; but still the, comparatively speaking, simple disease gonorrhœa was confounded with it, nor was it till the eighteenth century that the one was disassociated from the other, although previous to this time it had been ascertained that the discharge, which was accepted as diagnostic of syphilis, was by no means invariably followed by secondary or constitutional symptoms. It will ever remain an honour to English surgery that it was almost entirely through the exertions of its professors that the distinction was made; Cockburn in London, and Balfour in Edinburgh, being the first to uphold the non-identity of the disorders; whilst probably the most powerful opponent of this doctrine was John Hunter, who published his classical work on the venereal towards the end of the century. He showed from his dissections that in gonorrhœa there was no excoriation of surface as there was in ulcers of the external parts; but this he attributed to the difference between a secreting and a non-secreting surface. This stage of the controversy is notable as being that wherein experiment is invoked for the first time: John Hunter having tried the effects of inoculating pus from a discharge supposed to be gonorrhœal, but which gave rise to a syphilitic sore. He does not say whence this pus was taken, and we can now readily enough understand that the discharge had in reality been either wholly or partly syphilitic. At the same time Hunter was aware of a mild form of discharge, which he likened to fluor albus in women. Hunter's opinion was not long allowed to go unchallenged; for another celebrated man, Benjamin Bell,

had by his observations and experiments arrived at a completely contrary conclusion. He was also the first to point out the existence of internal chancres, and to thus account for the formation of sores by pus discharged from the urethra. Sir Astley Cooper also took this side of the argument. We shall not speak of any of the subsequent investigators until we arrive at the time of Ricord, who published his famous 'Traité pratique des Maladies Vénériennes,' in 1838. This celebrated writer finally dispelled the notion of the identity and intercommunicability of gonorrhœa and syphilis: but he went further. Not satisfied with proving that concealed chancres (chancres larvés) would suffice to account for the cases on record in which a urethral discharge had been shown to be capable of producing sores, he affirmed that all such discharges of a non-syphilitic nature were the result of a simple non-specific inflammation, most readily produced by a similar discharge, but yet capable of originating in other ways. In this he no doubt went too far, but his error was of importance, showing how readily such discharges could be produced by acid urine, by menstrual fluid, and many other things, as well as the groundlessness of the assertion that all urethral discharges (non-syhilitic) were specific. Auspitz accepts this modified view, viz. that the weight of testimony inclines one to think that in the gonorrhœal virus, strictly so called, there is something specific; but we confess that even now there are cases the nature of which it is far from easy to determine.

Having thus shown how the two diseases have come to be separated, Dr. Auspitz proceeds to the consideration of the genital ulcer, with which he has more immediately to do. We have already hinted that many looked upon this ulcer as being of a simple character, and considered that the disease was not syphilitic until secondary symptoms (usually known as the "great pox") made their appearance; but in Hunter's great work we have the characters of the infecting chancre distinctly and definitely laid down. The characters which he selected, particularly the induration at the base, are even nowadays accepted as distinguishing the so-called true or Hunterian chancre. From other parts of Hunter's writings it can be distinctly made out that this was the only form of sore which he recognised as capable of giving rise to secondary symptoms, although he was well aware of the existence of other and non-specific genital ulcers. The same doctrine was advocated by Abernethy and Carmichael, the latter of whom mentions five kinds of non-specific sore, viz.—(1) a superficial ulcer without hardening, but with elevated edges; (2) a superficial ulcer having neither of these characters; (3) an excoriation with the secreting of pus; (4) a

phagedenic, and (6) a sloughing sore. Certain of these he admitted gave rise to constitutional symptoms, but of a kind to be distinguished from the sequelæ of a true chancre. Buboes were early observed to be a consequence of venereal disease, and at an almost equally early period it was discovered that there were some which suppurated and some which did not, as well as that the latter were followed by constitutional symptoms, the former not so; but it was only, comparatively speaking, recently that the distinction between sympathetic and specific buboes was fully made out. Of the truly secondary symptoms *condylomata* appear to have been early observed; but the differences between condylomata and warts were not so readily recognised, although it was known that the former were capable of propagating the disease. Of the important researches of Hunter on these and allied subjects we cannot now delay to speak, but must hurry on to the doctrines of the Ricord school.

The name of this eminent syphilographer will ever be associated with the disease for whose elucidation he did so much. His plan of procedure was mostly experimental, more so than had been the case up to his time; and, accordingly, he fixes upon inoculability as the essential characteristic of a true venereal sore. He also looked upon the constitutional symptoms as entirely depending on the local affection, so that if the latter could be converted into a simple ulcer within a given time, no secondary symptoms would follow. His conclusions with regard to buboes were, if possible, still more important: he held that the specific bubo was alone transmissible by inoculation, and that when a bubo could not be so propagated no secondary symptoms would ever follow. He further maintained that secondary symptoms could not be transmitted by inoculation, although they could be communicated from parent to offspring, whilst tertiary symptoms could neither be propagated the one way nor the other. Mucous tubercles he held to be non-contagious, but admitted that they could not always be distinguished from certain forms of chancre. It will thus be seen that Ricord clung to the unity of the syphilitic poison, but in many other respects his views became considerably modified by more enlarged experience; and the doctrines contained in his celebrated 'Letters,' differ considerably from those enunciated in his earlier work ('Traité pratique'). During the period which had intervened between the publication of these two works, Ricord's attention had been specially directed to the induration of certain chancres and the softness of others; and he saw that whereas in the case of the latter kind there was always some doubt as to the nature of the sore, with regard to the former there could be none, and that in their train secondary syphilis invariably

followed. Still he adhered to the doctrine of unity in the syphilitic virus, looking upon hardness and softness as characters dependent on accidental peculiarity of position and constitution. He also stated now that when a bubo secretes pus capable of propagating the disease, the malady is then got rid of, and no secondary symptoms follow; not so, however, if an indolent bubo end by forming non-contagious pus, for thereby no such immunity is secured. Another of his most important doctrines was, that one attack of hard chancre effectually provided against any subsequent infection, and that this immunity was transmissible: but here we must leave Ricord.

In his fifth chapter, Auspitz proceeds to discuss the dualistic theories which inevitably rose from the investigations of Ricord. Bassereau was the first to advocate the notion that, as two such well-marked varieties of syphilis existed, as those constantly characterised by hard and soft sores, with their varied sequelæ, it was surely more rational to seek some reason for this in a difference in the virus, than in modifying circumstances of a variable nature. He accordingly recognised two forms, the true and the false chancre, holding that the latter has existed from all time, the former since the last decennium of the fifteenth century only. But matters did not stop here; in 1854 Clerc advanced the notion that the two forms of chancre (the infecting and the non-infecting) were, in reality, identical; only the latter (chancroid) was produced by the former being ingrafted on a constitution already syphilised, that it bore the same relation to a true chancre that varioloid did to smallpox. The opinions of both rested on too narrow a foundation. In 1858 there appeared a new series of lectures by Ricord, edited by Fournier, in which the doctrines advanced by the former were still further modified. They came to the conclusion that both simple and infecting chancres, reproduced themselves as such on the healthy individual, but that the inoculation of a syphilitic patient with a hard chancre was commonly followed by no positive result; if any sore was produced it was soft. On the other hand, they admitted that soft sores if infecting might propagate hard ones. Diday again held that there were two distinct diseases—chancre and chancroïd, the one affecting the whole system, the other the skin and lymphatics only; neither can one give rise to the other, but when a chancroïd sore becomes ingrafted on constitutional syphilis, the resulting sore might propagate true infecting sores. The most recent French writer mentioned by Dr. Auspitz is Rollet, whose conclusions he gives at full length; but it will be enough for us to say that he firmly maintains the duality of the syphilitic virus, and that syphilitic pus, whether from a primary or secondary sore, always gives rise

to an infecting ulcer: at the same time he upholds the doctrine of mixed sores by which he accounts for many of the anomalous results attained by earlier observers.

One of the great difficulties in the way of earlier writers, and what prevented them from coming to right conclusions was, that their inoculations were practised on individuals who had been the subjects of sores, the system of inoculating healthy individuals being only introduced by Wallace of Dublin, in 1835, though it has since been followed out to a great extent by the German school of syphilographers. We cannot stay to discuss the researches of Vidal, of Waller, of Rinecker, of Danielssen, of Gibert, of von Bärensprung, of Lindvurm, of Hebra, of Rosner, and of Bidenkap on this subject, important though they be: we have already delayed too long in tracing the gradual evolution of the doctrines now current.

The next question discussed by Auspitz is that of the transmission of the syphilitic virus by vaccine matter; a question of no ordinary importance, and one which even now is unsettled. Here, again, an English surgeon (Moseley) was the first to point out the existence of a contagious disease resulting from vaccination, but its nature was not then understood, although it is described as yielding to mercurial treatment. Curiously enough, almost all subsequent details, for some years at least, came to us from Italy, where the transmission of the disease seems to have been not uncommon, with, in some cases, the most disastrous results. To speak of some of the more recent instances: in 1859 two French soldiers were vaccinated from a comrade who had recently suffered from syphilis. In one instance there was no result, in the other a syphilitic sore followed. In this case it is distinctly stated that there was blood on the lancet used for the inoculation. One of the apparently best verified sets of cases was recorded by Pacchiotti in 1862. The healthy child of healthy parents was vaccinated, and when the pustules came to maturity, forty-seven children were vaccinated from his arm. It turned out that this child had been in constant contact with, and been suckled by a woman labouring under secondary syphilis; accordingly thirty-eight out of the forty-seven thus vaccinated became affected with syphilis, several died; and of those who had been vaccinated from one of their number, seven more were attacked.

The first child was vaccinated on the 24th of May, and on the 24th of the subsequent February there were affected with disease from this child forty-six other children, twenty-four mothers, five husbands, and three brothers of those attacked. On the 27th all were removed to the hospital at Turin. In this case, also, it is mentioned that the lancet used in vaccinating

from the first child was bloody. There are other narratives to which we might refer, but these will suffice. Experiment was called into play to settle the disputed question, if possible; accordingly, vaccination was practised with lymph taken from syphilitic individuals with negative results; not so when the lymph was mixed with the pus of primary or secondary sores. Fridinger in Sigmund's clinic undertook a number of experiments of the latter kind, and with most decided results, no vaccine pustule being produced, but in its place a well-marked sore. The conclusions to which Dr. Auspitz, at the end of a remarkably able chapter comes are as follows: That vaccination may give rise to the inoculation of syphilis, but that we are not in a position to say decidedly when syphilis exists in combination with the vaccine virus; that this power of transmission exists in the virus itself, though we cannot look upon the lymph as being a syphilitic product; it is rather ordinary lymph which has had syphilitic properties ingrafted on it by passing through a syphilitic constitution; not a molecular change, merely a mechanical mixture of vaccine and syphilitic products. We cannot follow Dr. Auspitz into his inquiry as to the results of clinical observation on syphilis; suffice it to say, that the experiences of the Vienna school decidedly inclines to the support of the dualistic theory.

Among the most interesting questions connected with syphilis are those of late years advanced as to syphilization, and its effect on the disease. The idea is not a new one, for it was advocated by Astruc many many years ago, he holding that a saturation with the syphilitic poison was both possible and advisable. Not very long ago Diday broached the idea of inoculating people with syphilis as one would with the vaccine virus, but of course the project was not carried into execution. A still more visionary proposal was made by Jeltschlinsky, of Moscow, who imagined that the true cowpock exercised a preservative influence. But the great advocates of syphilization have been Auzias Turenne, Sperino, and Boeck. Although the theories they have severally adopted have been different; the two former desiring to saturate the system with syphilis, the last to eliminate it by a natural process stimulated by repeated introduction of the poison. This mode of treatment has been chiefly adopted by Scandinavian surgeons. Although some experiments have been made in our country, the results have not, however, been encouraging, and most of our countrymen are strongly opposed to the practice. This chapter finishes the historical portion of Auspitz's work, the final portion being occupied with conclusions founded on experimental and clinical data. Of the former we subjoin the most important, viz. that the trans-

ference of the secretion of a constitutional syphilitic affection (hard chancre included) to one affected with syphilis usually produces no positive result when the secretion is devoid of pus corpuscles. When, however, by artificial or natural means such affections take on a process of pus formation, the inoculation of the pus produced either (in from twenty-four to seventy-two hours) pustules and soft inoculable chancres, or, after longer incubation, infiltrations capable of infecting a healthy individual.

We wish we could follow our author all through his valuable deductions from clinical data, but our space forbids; they are, however, summarized in the following dogmas:

"There are two forms of chancre, a simple and a specific; and this is shown by either invariably reproducing its own species in a *sound constitution*, by a period of incubation in infecting sores, and the want of this in non-specific sores, by characteristic differences in their anatomy, minute and otherwise, and the pathology of either, by the different nature of the buboes resulting from the two, by the difference in their amenability to treatment, and by the inoculability or the reverse of their secretion in a syphilitic individual."

We do not say that we entirely agree with him, but the questions in dispute are here advanced in a straightforward and intelligible shape.

Altogether, we are highly pleased with this work, it gives a clear and able *résumé* of the many points in the history of syphilis which have from time to time been the subjects of discussion. We doubt if it would be possible to write a work which would be entirely unexceptionable to every one, so fierce have been the debates on this most interesting theme. To say that Dr. Auspitz has done this would be false; but we can say that his views are uniformly stated without dogmatizing, and in no case have they been formed without good grounds. The work will be found extremely useful to men engaged in the study of syphilis, not only as containing an immense amount of valuable information, but also as giving the sources whence this has been derived in such a fashion as to admit of ready reference to the original authority. We congratulate Dr. Auspitz on the result of his labours.

Review IV.

1. *Lehrbuch der Hebammenkunst.* Mit 62 Holzschnitten. 2te Auflage, Leipsic, 1864. Von Dr. Bernhard Sigmund Schultze, Professor der Geburtshülfe, Director der Entbindlungs anstalt und der Hebammenschule zu Jena.

Text Book for Midwives. By Dr. B. S. Schultze, &c., Professor of Midwifery, Director of the Maternity and School for Midwives at Jena.

2. *Eine Extrauterinschwangerschaft.* Von B. S. Schultze. Abdruck a. d. Jen. Zeitschrift fur Medicin, &c. I. 2. 1864.

A Case of Extra-uterine Pregnancy. By B. S. Schultze.

3. *Ueber Palpation normaler Eierstöcke und Diagnose geringer Vergrösserungen derselben.* Von B. S. Schultze. Abdruck a. d. Jena Zeitschrift f. Med., &c., I. 3. 1864.

On Palpation of the Normal Ovary and Diagnosis of slight enlargements of it. By B. S. Schultze.

Ueber Superfecundation und Superfetation. Von B. S. Schultze. Dazu Tafel 1. Abdruck a. d. Jen. Zeitschrift, II. 1. 1865.

4. *On Superfecundation and Superfetation.* By B. S. Schultze. With a lithographed plate.

5. *Zur Kenntniss der Todesart des Kindes bei vorzeitiger lösung der Placenta.* Von B. S. Schultze. Abdruck a. d. Jen. Zeitschrift f. Medicin, &c., I. 2. 1864.

On the Mode of Death of the Child in Premature Separation of the Placenta. By. B. S. Schultze.

6. *Beiträge zur Casuistik der Geburt bei verengtem Becken.* Von Ludwig Pfeiffer, Inaug. Abh. fur Doctorat Jena. 1863.

An Essay on Labour with Contracted Pelvis. By L. Pfeiffer. Thesis for the degree of Doctor at the Faculty of Medicine at Jena.

7. *Zur Diagnose der Hæmatocele Retro-uterina.* Von Paul Kämpffe. Inaug. Abh. f. Doctorat Jena. 1865.

On the Diagnosis of Retro-uterine Hæmatocele. By Paul Kampffe. Thesis for the degree of Doctor at the Faculty of Medicine at Jena.

8. *Facts in relation to Placenta Prævia, with a Review of the various opinions respecting its Anatomy, Physiology, Pathology, and Treatment.* 27 pp. with 8 lithographed plates.

1865. By Isaac E. Taylor, M.D., Professor of Obstetrics, &c., &c., in Bellevue Hospital, New York.

The book and essays on our list are, with one exception, from the pen of the learned Professor of Midwifery at the University of Jena and his pupils. They show the careful study given to a branch of medicine which has lately made considerable advances amongst ourselves, and merit attention as an example of its progress in that school as well as for their intrinsic value.

The case of extra-uterine pregnancy appears to belong to the class 'graviditas interstitialis.' Dr. Farre[1] in his brief *résumé* of the varieties of tubal gestation remarks "that the walls of the sac, being in this case usually much stronger than when the ovum lies nearer to the distal end of the tube, resist pressure for a longer time, and, consequently, the fœtus may attain a greater growth." There is also less danger of hæmatocele from rupture.

The difficulties in the way of examining, during life, the healthy female genital organs, is the chief cause of the discordant opinions that have so long been a reproach to the profession. The ignorance as to the amount of divergence from a supposed standard of health which may take place without being morbid has been the fruitful parent of error. Observers have been led to treat as disease what on more accurate observation has proved to be an innocent change of position, or a secondary result of some pathological condition has been treated as the primary, and with a fruitless result until the real exciting cause has been discovered. Careful diagnosis is our only remedy, and the assigning to each organ the part it takes both in health and disease. Just as many diverse diseases of the chest were formerly confounded together, so diseases of the uterine organs are still frequently misunderstood, partly from neglect in tracing out the history, partly from want of dexterity in investigating and determining the exact seat of the disease. The second paper is an attempt to elucidate this difficulty.

Handbook for midwives.—The midwife is regarded in Germany as a person legitimately exercising her art, and not as an interloper and invader on the territory of 'the doctor.' She is there educated in a hospital and expected to fit herself by study for her vocation, so that she may perform her duties with knowledge instead of by rule of thumb; and being able to recognise the approach of danger, may call in time for more highly instructed obstetrical assistance. That the value of sound preliminary instruction is generally admitted, the establishment

[1] 'Cyclopædia of Anat. and Physiol.,' p. 621, vol. v, Suppl.

of a system of nursing in many of our hospitals is a sufficient proof. The education of the midwife is but an extension of the same principle.

Professor Schultze recognises the sphere in which the midwife's duty lies. His book is an able guide. He gives the requisite information in full detail, without going beyond the confined circle in which the midwife and nurse should act, and he adapts his language to the comprehension of the class for whom he is writing.

The book is divided into seven parts.

Part I begins with an outline of the human skeleton and viscera, the vernacular names being given. The skull of the child and dry pelvis of the woman, the external and internal female genital organs, are described briefly and plainly, and illustrated with fifteen large figured woodcuts.

Part II describes natural pregnancy, the mode of reckoning its duration, the ovum at the second, seventh, and twelfth weeks; the fœtus of sixteen, twenty, and forty weeks, the marks of a mature child contrasted with an immature. The projection and height of the uterus at the various periods are shown by an ingenious diagram, and general but minute rules are given as to the manner of living of the pregnant female.

In Part III regular labour is described, and the mechanism illustrated by diagrammatic woodcuts. With reference to the perinæum he directs that, if the tension on it threatens rupture the birth of the head is to be delayed by pressure on it with two fingers until the tissues yield normally. He points out the vulgar errors committed in childbirth, and reckons among them the putting a finger into the mouth of the child to clear away mucus. The midwife's 'bag' should contain a clyster, syringe, a catheter, a vaginal tube, scissors and thread for the funis, sponges, oil, and a phial of sal volatile and of æther.

Part IV is occupied with the condition of and directions about the mother and child, from the labour until she gets up. No meat is allowed for the first four days, a treatment going out of fashion in England.

In Parts V, VI, and VII, brief and accurate directions are given how the midwife may recognise any departure from the ordinary course of pregnancy, unnatural positions of the child, twins, extra-uterine fetation, irregularities in the health of the mother, on the position of the uterus, metrorrhagia, miscarriage, abnormal presentations, abnormal conditions of the pelvis, lesions of the uterus and soft parts, still birth, and the various dangerous complications, whether regarding the mother or child, which require medical assistance. The book is arranged in numbered paragraphs, and a full index is supplied.

In the essay on superfœtation, Professor Schultze attempts to throw light on a question which presented more interest in former days than it now awakens. He examines the problem as a possibility and a probability, and decides in favour of the first and against the second.

Our text-books on midwifery give little information as to the cause of death in still-born children, and the subject is generally passed over very cursorily; and we have, therefore, given a full abstract of the paper by Schultze on this subject.

Two theses for medical degrees on hæmatocele and contracted pelvis respectively are the next in our review, and we conclude with a notice of an essay on placenta prævia, by Professor Taylor, of New York.

Extra-uterine Pregnancy.—D. K—, æt. 37, was admitted to the hospital by Professor Schultze, December, 1862, with symptoms of sub-acute peritonitis. She said that she was about thirty weeks pregnant, and had suffered abdominal pain since the first seven weeks, with dysuria and painful defecation. She had felt the movements of the child a few days before admission. The abdomen contained a central tumour like a uterus of twenty-eight to thirty weeks' pregnancy. On vaginal examination, the finger passed two and a half inches into the uterus, and then met with firm elastic resistance. By combined external and internal palpation a fœtus was distinctly made out, a portion of the tumour being resonant. Extra-uterine pregnancy was diagnosed.

January.—Œdema of the lower extremities and vulva appeared.

February.—The abnormal tumour had diminished in size, lay more to the left side, was harder, and more fixed. An abscess began to form in the abdominal wall, which burst in March, discharging matter with a fecal smell. A probe passed through this opening came upon bone, as also did a sound passed per uterum. The bladder was in a normal condition.

In April some fœtal bones passed with the urine, and a fistula was found in the posterior wall. Bone, blood, and muco-pus continued to pass per urethram till it became blocked by a cranial bone.

May 12th.—Patient being under chloroform, the abdominal fistula was enlarged to admit two fingers, and through this the most part of the bones of a fœtus was removed with forceps; but the occipital and parietal bones were too firmly adherent to the cyst-wall to be dragged away safely; they were, however, taken away on May 21st.

May 31st.—A number of small bones passed by stool.

June.—Symptoms of uræmia with inflammation, first of the

right and then of the left kidney supervened. The patient, however, struggled through, and became convalescent.

August 10th.—The catamenia appeared. A year later the patient was in perfect health. On examination, the uterine cavity measured two and a half inches straight, but two and three quarters to the left 'cornu.'[1] On the right side an ovary was felt behind the uterus in the Douglas' space. At the left cornu of the uterus the left ovary, double its natural size, was recognised. The bladder was healthy. Schultze suggests that the death of the fœtus was due to perforation of the rectum, the communication being shown by the fœcal smell of the abscess matter.

The case is remarkable as an instance of perforation of bladder, rectum, and abdomen, with closure of the fistulæ and restoration to health.

As to the propriety of operation in such cases, and the period for operation, it is impossible to lay down any rule. The great tolerance that the abdomen has of such a body is a strong argument for temporising. The result in the present case is, perhaps, the best proof of the wisdom of the proceedings.

On Palpation of the Ovary.—Schultze says he finds no difficulty in feeling the healthy ovary by external palpation, combined with the vaginal touch, except when the abdominal walls are very tense or very fat. He does not, however, call attention to the position in which the patient must be placed for such an examination. It is quite impossible to make an exact diagnosis if the patient is only placed in the ordinary accouching position. She must be on her back, the shoulders supported, the legs drawn up so that the heels are a few inches asunder, and the thighs fall apart easily; in fact, so that the abdominal muscles may be quite relaxed. Dr. Marion Sims has described the attitude very exactly; but the vaginal hand may be passed underneath the thigh sideways, instead of from the front as he advises. In the examination we must be on our guard against mistaking for the ovary a cyst in the broad ligament, an enlargement of the Fallopian tube, a fibroïd outgrowth of the uterus, a fœcal mass; even a swollen lymphatic gland in the neighbourhood of the rectum has been taken for the ovary dislocated into the Douglas' space. Schultze does not think much is to be gained by examination per rectum, as, except when the ovary is dislocated downwards, it can rarely be felt. The healthy ovary is usually not sensitive to the touch, and not always even when inflamed, so long as the peritoneum is

[1] Is there not some reason to believe that this might have been an instance of a bicorned uterus, which would account for the absence of rupture at an earlier period.

unaffected. Both during and after inflammation the ovary is often found in the Douglas' space, and in the latter case does not always regain its normal position. "In many cases," says he, "I have found that oophoritis was the real cause of the symptoms assigned to prolapsus and metritis, and on this being cured, health was re-established, although the uterus remained flexed or verted; while conversely, when painful sensations have been relied on, I have found that the ovary was free from pain." A case is detailed where retroflexion of the uterus was coincident with ovaritis. Attempts were made to reduce the flexion by means of Simpson's intra-uterine pessary; but no relief was obtained until the ovaritis was resolved, and then the patient became free from pain and discomfort, although the uterus was still retroflexed.

Superfecundation and superfetation have frequently been used as synonymous; but it is more convenient to apply them to distinct ideas. *Superfecundation* then, supposes that two or more ova are separated at one ovulation, and that they become impregnated at different occasions, but within the space of a few days. The probability of this is not disputed, though it cannot be proved; *superfetation* supposes that, after pregnancy has been going on for several weeks or months, a second ovulation occurs, and that the ovum of this period becomes also impregnated.

The arguments against this are twofold:

A, that it is a physiological impossibility; *B*, that the supposed cases can all be explained in other ways.

A, it is asserted that, 1, ovulation does not occur after pregnancy has taken place; 2, that the effects of pregnancy prevent, *a*, access of another ovum to the uterus, and, *b*, access of semen to such an ovum.

A 1. Suspension of menstruation during pregnancy is the rule, but there are undoubtedly many exceptions. Many authors[1] are of opinion that ovulation continues during pregnancy; ovulation, then, cannot be denied, although we cannot prove it.

A, 2. The decidua is only the hypertrophied mucous membrane; the passage from the Fallopian tube to the uterus is, therefore, open. The union of the decidua vera and reflexa takes place about the twelfth week; up to this time therefore the semen can pass up the uterus, and after this period the decidua may be adherent to the uterus only in places, as in the case of dropsy of the gravid uterus reported by Dr. Duncan.[2]

[1] Hohl. 'Lehrb. der Geburtshülfe,' 1, Aufl. 1855, p. 347; Scanzoni, 'Lehrb. der Geburtshülfe,' 3, Aufl. 1855, p. 320. The same. 'Beiträge zur Geburtsh und Gynak,' iv, p. 311, 1860. Ritchie.

[2] 'Edinburgh Monthly Journal,' April, 1863. See too Hegar's "Researches

But though superfetation is thus theoretically not an impossibility, its reality is by no means proved.

B. Kussmaul[1] refers to two cases (Dejëan and Bellengsen), where a five and a seven months' dead fœtus were brought forth together with a three months' *fresh, almost living* fœtus respectively. In both women the uterus was double.

Meisner[2] describes a case where twins were contained in one ovum, one of which, apparently twelve days dead, measured fourteen and a half Paris inches (seventeen English), and presented the signs of thirty weeks' growth; the other was mature. The difference of development could not have been due to superfetation.

But the most remarkable cases where fœtuses showing different degrees of development have been born at the same time, are five cases of triplets related by D'Outrepont,[3] Klykpennink,[4] Bock,[5] Hausbrand,[6] Rothamel.[7]

The fœtuses in the first three cases are described as being *well nourished, round,* and *fresh,*—the difference of development between the fœtuses in each set of triplets as representing the ratio of four months to nine months. Now, the difficulties opposed to superfetation taking place are *à fortiori* greater in these cases than in doublets.

Schultze next discusses the case, where twins are born at considerable intervals of time, their growth corresponding to the period of birth. "I have," says he, "already shown that the fact of twins being found alive in different stages of development, as twenty-four to thirty-six weeks, or even seventeen to forty, does not necessitate the supposition of a different period of impregnation." But we must observe that in none of the cases quoted are the secondary fœtuses described as actually alive, some are said to be *well nourished, quite fresh,* and others are less accurately described. Schultze proceeds to argue on the above assumption, that the cases reported by Desgranges,[8]

on the Decidua," &c., in 'Monatschrift für Geb.,' December, 1863, bd. xxii, p. 429.

[1] Kussmaul, 'Zur Mangel, &c., der Gebärmutter,' p. 296, Wurzburg, 1859.

[2] 'De Secundinis ac de Superfetatione, Diss. Inaug.' Leipsiæ, 1819.

[3] 'Geburtshülfliche Demonstrationem,' Weimar, x Hefl., 1829.

[4] 'Praktisch. Zeitschrift,' 1835; "Nov. Dec.," Referat in 'Schmidt's Jahrbuch,' xv, 306.

[5] 'Beschreibung eines Falles von Drillingsschwangerschaft Inaugural-dissertation,' Marburg, 1855.

[6] 'Med. Zeitschrift,' v. v, f. H. in Pr., 1836; No. 14, Referal in 'Schmidt's Jahrb.,' xi, 311.

[7] 'Zeitschrift für die gesamonta Heilk., &c., Kurhessens, Vereinsblatt Kurhessicher Aertze und Wundärtzte,' Bd. 1, heft i, 1842; 'Schmidt's Jahrb.,' 37, 267.

[8] Kussmaul, op. cit., p. 299, 300.

Eisenmann,[1] Barker,[2] Thielmann,[3] may be explained in a similar way. He suggests that if the smaller fœtus in Klykpennink's case had not been born until it had reached its full development, that there would have been a difference of twenty-four weeks between the birth of its already mature brother and of itself. In the museum at Jena is a preparation exhibiting the placenta and membranes of a child at term having annexed at one side a second placenta and ovum, within the membranes of which a six-weeks' old fœtus floats. The fœtus, however, though its structural development is only apparently of six weeks, seems to be of much older date, as it is long instead of round, measuring three inches instead of a little over one inch; the funis is eight inches in length, and the amnion and chorion are joined together, the cavity having disappeared. Schultze concludes that the possibility of superfetation cannot be denied, but that no presumptive case at present reported will bear accurate criticism.

In a very able paper, read before the Obstetrical Society of Edinburgh, November 24, 1864, and published in the 'Edinb. Monthly Journal,' Dr. Bonnar, of Cupar, Fife, has collected a large number of reported cases. After examining them critically, he urges some arguments in favour of the probability of superfetation. After showing that the uterus is not again ready for impregnation until the fourteenth day after delivery, and that 180 days of intra-uterine birth is the least number necessary for a child that shall live to puberty, he brings forward from the peerage three cases, amongst others of less weight, where persons lived to adult age and married who must either have been conceived during the uterine life of their elder living brothers, or have themselves been in uterô only 168, 159, and 113 days respectively.

On still-birth. The pallor and suspended animation occurring in new-born children as a result of premature separation of the placenta, have been wrongly attributed to loss of blood, whereas the mode of death is really asphyxia (apnœa of Watson), "for," says Schultze, "on injecting a placenta with milk by the umbilical vein, not a drop escapes from the uterine surface." Pernice has described the symptoms and anatomical differences between asphyxia and anæmia in the newborn. He recognises the difficulty of explaining the mode of death when the separation of the placenta has been so slight as only to diminish the respiratory surface to a very slight extent; at the same time he happily brings forward Herholdt's suggestion, that the uterine

[1] 'Eisenmann, Tabulæ Anatomiæ quatuor, uteri duplicis Observationem cet. Argentorati,' 1752.

[2] 'American Monthly Journal.'

[3] 'Med. Zeitung Russlands,' 1853, No. 50.

hæmorrhage reduced the action of the maternal heat, and thus the fœtus did not receive a sufficiency of oxygenised blood.

The rationale of the apnœa is thus explained by Schultze: —Deficiency of oxygen excites an attempt at intra-uterine inspiration; this, being repeated, directs the blood of the right side of the heart through the lungs; the quantity and pressure of the blood in the ductus Botalli and descending aorta are thereby diminished, and the *vis à tergó* being thus weakened, the placental circulation becomes slower and less in quantity. In proportion as the course of the blood through the lungs is quickened by the fruitless acts of inspiration, the left auricle becomes gorged by the pulmonary veins, and the space for the blood of the placenta and venæ cavæ encroached on with the choking of the foramen ovale. The repletion of the right auricle (and later also of the left) induces stagnation in the whole venous system of the body, the umbilical vein being the last to suffer because of the hepatic capillary system interposed between it and the heart. The diminution of pressure on the umbilical arteries, and later the increased pressure on the umbilical vein, favour the increase of carbonic acid in the fœtal blood, and respiratory acts are repeated so long as the medulla oblongata answers its stimulus. It is the sudden abridgement of the respiratory surface which excites the first inspiration as we see happen at normal birth from the rapid accumulation of carbonic acid. There are, of course, other causes which act mechanically on the circulation, as compression of the umbilical arteries, to which is perhaps due the congestions we find in the kidneys and genital organs, and the ecchymoses in the parts supplied by the intercostal, bronchial, and coronary arteries.

Dr. Kämpffe details two cases of retro-uterine tumour, the symptoms of which are diagnostic of hæmatocele, though not confirmed either by post-mortem section or tapping.

P. G—, a multipara, thought herself again pregnant, the catamenia having ceased fourteen weeks; symptoms of abortion came on followed by persistent dysuria, pain in defæcation and in the iliac regions. The patient looked exsanguine, but there were no inflammatory symptoms. The uterus was thrust forwards and upwards by a fixed firm elastic swelling which extended into the abdomen, to a level with the anterior superior spine of the ilium. In about six weeks the tumour diminished and became firmer in consistence. The catamenia returned but were copious and irregular. The general anæmic condition was still marked. Examination made four months later showed the uterus normal, and a rounded hard tender lump in Douglas' space. The left ovary was found *in sitú*, but the right was not perceptible.

P. S.— was delivered by forceps; the labour was followed by parametritis. She afterwards suffered from syphilis and consequent abortion. Subinvolution of the uterus and metrorrhagia deteriorated the general health. Catamenia became regular for some months. On January 9th, 1864, they had continued ten days, were excessive, and pain came on in the lower belly with symptoms of pelvic peritonitis. On vaginal examination, resistance was felt, with tenderness on pressure in the posterior cul de sac.

February 4th.—Again metrorrhagia, with increase of pain, and a post-uterine tumour became distinct, rising up to the umbilicus. Anæmia was marked; under the use of ice and full diet the tumour diminished.

March.—Catamenia regular; no increase in the tumour, which gradually disappeared. An irregular mass was felt at the situation of each ovary.

The first case illustrates Bernutz's[1] description of peri-uterine hæmatocele from menstrual retention. The second seems to come under his fourth group of peri-uterine hæmorrhagic hæmatocles.

Contracted Pelvis and its influence on Labour.—Dr. Pfeiffer writes the thesis for his degree on the equally contracted pelvis (æquabiliter justo minor). He takes as his standard Litzmann's guage 3″ 6‴ internal conjugate diameter. Litzmann found the per-centage of these pelves 14·9; Michaelis 13·1. The contraction of the pelvis Dr. Pfeiffer regards as a frequent cause of limb and funis presentations, as the head does not fill the pelvis properly. The teaching at Jena agrees with our own as to the danger to the mother from long-continued pressure, and the advisability of early assistance. If the conjugate diameter is not less than 2″ 9‴ the forceps should always be first applied if less then craniotomy or Cæsarian section. Turning instead has not, in the experience of the hospital, saved the child's life. If the child be dead, cephalotripsy is far more favorable for the mother than extraction with the forceps.

Premature labour should not be induced before the twenty-ninth week, and in slight cases of contraction the thirty-second to the thirty-fifth week is preferable.

The modes mentioned are—the uterine douche of Kiwisch; the tampon and Cohen's method with the catheter. No notice is taken of the plans for dilating the cervix either by a pessary or air bag.

Tables of the measurements of the pelvis and children in thirty-two cases are drawn up, and details of thirteen labours

[1] 'Diseases of Women,' Bernutz and Goupil, vol. i, p. 74, 208, and 219, New Sydenham Society, 1866.

are given. No. 1 Cæsarean section; 2, the cervix was incised; 3, forceps used; 4, her first labour was with forceps, her second abdominal presentation, turning failed, the spine was divided and the trunk extracted piecemeal, her third premature induction and turning; 5, forceps failed, craniotomy; 6, premature labour induced by douche; 7, douche for several days, then tampon, then catheter passed between membranes and uterus, and douche again repeated; 9, forceps; 10, the vulva was incised; 23, vulva incised; 25, tampon, turning; 27, forceps; 32, vulva incised.

Dr. Isaac Taylor criticises the Lettsomian lectures of Dr. Barnes on placenta prævia with considerable acrimony, and declares himself unable to appreciate his theory, his physiology, his anatomical views, or his method.

Dr. Taylor begins his essay by insisting on the non-shortening of the cervix uteri during pregnancy, and agrees with Dr. Farre on its real anatomical character. He illustrates his view by three plates depicted from nature, one of which, coloured, shows a preparation of a portion of the uterus of a woman who died six hours after labour at term, exhibiting very distinctly the cervix as contrasted with the corpus uteri. But Dr. Taylor seems to have taken as anatomical drawings, plates which Dr. Barnes evidently intended as mere diagrams. Many writers, as Ramsbotham, Churchill, Lee, are not explicit in their description of the cervix uteri and use language which may mislead, as it evidently has done Dr. Taylor; but it is impossible to suppose that they consider the canal of the cervix uteri to be the seat of placental attachment, since the placenta has already become fixed before the cervix has even apparently become shortened. Portal, in 1672, in describing placenta prævia, says, "il estoit collé à l'orifice interne," "je sentis le placenta qui environnoit en dedans l'orifice interne." The apparent difficulty has arisen from later writers speaking of the os uteri without distinguishing between os internum and os tincæ.

Dr. Taylor aptly compares the action of the cervix with that of the horse's anus in defæcation, the expansion of the canal producing an apparent effacement.

The treatment in placenta prævia advised by Dr. Taylor, is, "To resort to the tampon if the flooding is profuse or active, and the cervix not yet expanded, the os tincæ closed. Whether this occurs at the seventh, eighth, or ninth month, or full term, I prefer using the ordinary rolled surgical bandage two inches wide." When the cervix opens, if the pains are feeble, rupture the membranes, and then, if necessary, give ergot. "*There is no call for turning in cases of this character.*" "If there be commencing exhaustion, and the hæmorrhage is profuse, resort to

Dr. Simpson's method," (*i. e.* separate the whole placenta), and when the patient rallies attempt version. Should there be transverse presentation, rectify it by Dr. Hicks' combined method of version, and if this does not succeed use internal version.

Review V.

Report of the Sanitary Commission for Bombay, 1864. 8vo, pp. 285. Bombay, 1865.

It is much to be regretted that the several reports of the sanitary commissions at the different presidencies of India have not, as far as we know, been made generally public in this country. The only one that has reached us is the first issued by the Bombay commission; and as this does not contain copies of the correspondence between Dr. Leith, the president, and the War Office on various topics about sanitary matters referred to, the information given is not so complete as might have been reasonably looked for. Whether the unwillingness to bring before the public eye all the printed official documents relating to the different commissions be on the part of the authorities of home or of those in India, it is to be hoped that all reticence will henceforth cease, and that the several publications issued by these boards shall be made freely and at once accessible to all who are interested in the results of their proceedings.

The unnecessarily high death-rate of 69 per 1000, assumed by the Royal Commission for the British Army in India generally, excited no small amount of criticism among the medical officers of the service there as well as in this country. In the Bombay presidency more especially, in which the mortality among the troops was always much less than in most other parts of the peninsula, strong exception was taken to the standard adopted by the commissioners; nor could this be wondered at when it appeared that not only had the death-rate in the last fifteen years never reached one half the above estimate, but also that, during this period, there had been a very marked and progressive diminution going on in the annual amount of sickness and death throughout the Bombay army. It would seem that the comments made on this subject by the sanitary commission of this presidency had caused the issuing of some counter-remarks by the Barrack and Hospital Improvement Commission of the War Office; for the report now before us contains a reply to

these remarks on other points besides that of the annual death-rate, which had given rise to difference of opinion, such as the sort of latrine best suited for barracks in India, the readiest and least noxious means of disposing of latrine and other sewage of cantonments in India, the arrangements for the water supply, &c. Plans and methods which are indubitably the very best in this country will often be found to be scarcely applicable, at least without considerable modification of details, in regions so very dissimilar in their condition as a British county must be from a district of Hindostan; and the Royal Commissioners seem to have sometimes overlooked this consideration.

Although the statistics for one year only of the relative sickness and death among British and native troops afford slender grounds for comparison, the subject is so important for future reference, that the following data may be worth recording. In the medical year of fifty-three weeks, from 26th December, 1863, to 30th December, 1864, the mean strength of the British troops in the Bombay presidency was 12,412. The admissions into hospital were at the rate of 1514 per thousand of the force; and, on an average, there were 59 per thousand always in hospital. The death-rate from all causes, both in and out of hospital, was 15·87 per thousand. Besides this decrement, 169, or 13·6 in every 1000 men, were invalided for discharge; so that the total loss to the service, in the course of the year, was at the rate of 29·4 per 1000 of the entire force.

Fevers and hepatic disease were the chief causes of death; 28 per cent. of the whole mortality was due to them. Cholera (which proved fatal in rather more than in half the attacks) stood next; it occasioned 10 per cent. of the deaths; and about the same proportion was attributable to diarrhœa and dysentery. Of the total 197 deaths, 23 were from violence; 6 of these were from suicide.

The native force serving at stations in Bombay averaged 22,501. The hospital admissions were at the rate of 975, and the deaths at the rate of 14·30, per thousand. Particulars are not given as to the principal causes of either the sickness or the mortality. All that is stated is that cholera occasioned considerably more than a third of the whole deaths, the ratio from this disease alone being 5·91 per 1000 of the entire force. The want of details respecting the health-state of the native troops in India is a great desideratum, which it is earnestly to be hoped will ere long be supplied. The materials are in existence, and it requires but an order of the Secretary of State for India to have them utilised for the good alike of the service and of statistical science. Besides the above force at Indian stations, there were 1425 native soldiers on foreign service in China, and 989 were

stationed at Aden. The death-rate in China was very high, being 29·47 per thousand—4·21 from cholera alone. At Aden, although there was no cholera, the ratio of mortality was, nevertheless, 25·71; "due in great measure to the scorbutic diathesis that troops acquire at that station." If such be the explanation of this extremely high death-rate, the remedy is of course obvious by the regular issue of lemon-juice and fresh vegetables to the troops.

At the present time, when the subject of the prophylaxis of cholera is attracting so much attention, the following recommendations, as to the measures that should be enforced in respect of what is called the "pilgrim nuisance" during an epidemic season, are worthy of note:

"It might be ruled that pilgrims returning from a pilgrimage at which cholera has appeared, shall not enter any cantonment or town until after reasonable evidence that that they are free from infection;—the evidence required being that they are free from diarrhœa or other signs of the disease, and that two whole days have elapsed since they were in personal communication with, or in the society of, a person ill with cholera or diarrhœa. Should there not be indisputable evidence of this, the pilgrims should be kept under observation for forty-eight hours; and after that time, if no symptoms of the disease be manifested among them, they might be allowed to pass. Should, however, any of the party show signs of cholera, they should be separated at once from the rest, and these others should begin anew a two days' period of probation. . . . There should be separate accommodation for the sick. If proper tents be not readily procurable, native blankets joined together and stretched over bamboos or other poles would do."

Suitable latrines should of course be provided for the *detenus*, and arrangements made that earth be thrown in on the excreta by every person making use of them. The discharges from the sick should also be disinfected with lime, chloride of zinc, or some other similar corrective.

The only other topic to which we can now briefly allude is the important and, it must be confessed, somewhat difficult question "as to the period during which troopships with smallpox cases on board should be detained in harbour before putting to sea." The subject was referred by the Bombay board to the Barrack and Hospital Improvement Commission for its opinion, to guide them as to what should be done in such cases in future. The Commissioners seem to avoid committing themselves to any definite recommendation on the point in question, as will appear from the following passage in their reply:

"*After* actual cases of smallpox or other epidemic diseases have been removed out of the ship, *after* incipient cases of the disease have

been disposed of, *after* every necessary hygienic measure has been carried out, and the health of those on board has been ascertained to be satisfactory, then the question as the period of detention of the vessel has to be decided. If there is no reason to apprehend a renewal of attack, the vessel may proceed on her voyage. If it is considered advisable to detain her for a day or two, it should be done."

The caution of this reply will probably have somewhat disappointed the wishes of the board; they will not feel much strengthened by the advice.

Review VI.

1. *Die Inhalations-Therapie in Krankheiten der Respirations-Organe, mit besonderer Berücksichtigung der durch Laryngoscop ermittelten Krankheiten des Kehlkopfs.* Von Dr. Georg Lewin. Mit 25 Holzschnitten Zweite Auflage. Berlin. 1865. Pp. 506; mit Anhang, pp. 37.

The Treatment by Inhalation of Diseases of the Respiratory Organs, with especial reference to the Diseases of the Larynx as revealed by the Laryngoscope. By Dr. G. Lewin.

2. *On Inhalation as a means of Local Treatment of the Organs of Respiration by Atomized Fluids and Gases.* By Hermann Beigel, M.D., &c. With Woodcuts. London. 1866. Pp. 200.

3. *On the Treatment of Affections of the Throat and Lungs by Inhalation, &c.* By W. Abbotts Smith, M.D., &c. Second Edition. London. Pp. 48.

4. *Inhalation, the most rational treatment for Diseases of the Respiratory Organs.* By C. F. Schmid, M.D., and C. Milner. M.D. London. 1865. Pp. 35.

Public attention has been called to the subject of the inhalation of medicated vapours, by a recent trial of considerable interest to the profession. So far as the opinions expressed by the able physicians engaged on the side of the defendant may be allowed to represent the views entertained by the profession at large respecting the usefulness of such inhalations in chest disease, they may be cited in argument against the existence of such utility. On the other hand, much value was assigned to them on the plaintiff's side, almost wholly, however, on a bare assertion. At the same time the inference was fairly deducible, that little attention and slight experimental research have hitherto been

bestowed by medical practitioners generally upon the subject of medicated inhalations, and that it is most desirable to have this question in therapeutics placed on a satisfactory footing.

Admitting that, as a rule, the direct application of medicinal agents to a diseased tissue is of more certain efficacy than the operation of medicines reaching it through the general circulation, there is, *à priori*, ground for believing that such agents must exercise greater effect upon the pulmonary structure if brought into immediate contact with it. Moreover, it is apparent at once, that certain agents from which much advantage may be predicated in pulmonary affections are available only medicinally when they can be inhaled and so reach the morbid tissue. The benefits likely to accrue from inhalation, invited the attention of the earliest practitioners of physic, from whose works various observations and regulations as to the methods and scope of its application may be collected. Both Lewin and Beigel give a brief historical sketch of inhalation. The former distinguishes four periods; in the first only vapours were resorted to, in the second gases were inhaled, in the third the vapours of various chemical compounds, and in the fourth and last compressed air and pulverised fluids. The first period embraced that of Hippocrates, Aretæus, Galen, Pliny, Dioscorides, and subsequently that of the several Arab physicians. The agents used by these ancient authorities for inhalation were various balsams, volatile oils, and vapours, mostly of natural origin, including both sulphurous and arsenical. The second period is that of Priestley, Scheele, and Beddoes, who experimented with oxygen and other gases. The third period in question is, in fact, nothing more than the extension of the second in the hands of contemporaneous and later experimenters too numerous to name, who tried chlorine, iodine, the vapour of tar and turpentine and of various narcotics. The advocates of compressed air ushered in the fourth epoch, and have in their turn given place to the admirers of pulverized or atomized fluids. Indeed, the inhalation of other than atomized fluids scarcely finds a place in these treatises on inhalation, so completely has this last invented plan supplanted all others. Lewin sums up all that he has to say on the inhalation of medicated vapours and gases in some fifteen pages of a work containing 500; but Beigel devotes ten pages of his much smaller work to recount the history, the modes of using, and his own experience of "volatile chemicals, gases, &c.," most of this space, however, being taken up with remarks on the inhalation of oxygen.

It will be better in this place to give an abstract of the teachings of these authors on these older materials for inhalation before proceeding with the notice of the modes of using

and of the effects of atomised fluids. We may likewise here remark, that the brochure of Dr. Abbotts Smith gives a fair *résumé* of the state of knowledge respecting medicated vapours and gases, and especially concerning the uses of oxygen when admitted into the respiratory tubes.

Lewin begins his dissertation on the inhalation of vapours and gases by remarking that, although these materials have been resorted to since the earliest historical period, and also to a considerable extent within our own times, yet no exact and scientific basis has hitherto been afforded physicians whereon they might found a definite and satisfactory opinion respecting their utility as therapeutical agents. The reason of this he considers to be, that they do not present the same definite traces of their presence and action, as do the finely divided drops of fluid obtained by the use of an 'atomiser,' whether during life by the agency of the laryngoscope, or after death upon careful inspection.

The inhalation in question, termed by Lewin aëropneumatic, may be carried on at the ordinary, or at a higher temperature of the substances employed. The gases, such as oxygen, the vapour of sulphur, chlorine, iodine, creasote, camphor, ether, chloroform, &c., and the newly suggested nascent vapour of sal-ammoniac, are inhaled at the ordinary temperature. Professor Fuchs, of Göttingen, was the first to suggest the employment of hydrochlorate of ammonia in a nascent vapoury form, for inhalation in chronic pulmonary catarrh. The first attempts so to employ it were made by Fuchs, Lassèque, and Gieseler, who all used heat to generate the vapour. Lewin subsequently experimented with it and invented a simple apparatus whereby the nascent vapour was evolved as a consequence of chemical action without the aid of heat, and the too rapid evolution and the consequent irritation attendant upon the former plan were avoided. The apparatus referred to the inventor does not fail to give a sketch of, but, unfortunately, we can discover no history of his experiments with it, or of the results achieved by it.

The inhalation of iodine, it was surmised, would constitute a potent remedial agent in phthisis, inasmuch as iodine operates so powerfully on the lymphatic system in scrofula, but experience with it has resulted in disappointment. Creasote, on the other hand, has answered better, particularly in cases of advanced phthisis with vomicæ and putrid expectoration. It has appeared to operate as an astringent, and to remove the putrid odour. The fumes of liquid pitch probably prove useful in similar cases owing to their chemical similarity to creasote.

Various gases, administered in a pure form, or else mixed

with other gases or with atmospheric air in various proportions, have been largely tried at the Children's Hospital, Vienna, in cases of convulsive cough, but no decided permanent advantages have been found to follow their use. Pure oxygen, with an equal quantity of air, and laughing gas, have the power of suspending paroxysms of cough whilst inhaled, but work no cure; nitrogen and hydrogen, pure and mixed, proved rather mischievous, as did also ammonia in a gaseous form; one and all of these agents inducing cough and causing irritation. But the inhalation of carbonic acid was most detrimental, by reason, as Dr. Hanke supposes, of the great disturbance produced by it in the diffusion of gases ever normally going forward in the lungs. This physician further surmises that the paroxysms of pertussis are due to the accumulation of carbonic acid in the lungs.

Lewin recounts the virtues of coal-gas inhaled in gas-works during the purification of the gas by means of lime, as testified to by various physicians in Holland and in France, but he is unable to confirm them by experience of his own.

Among medicinal substances the vapour of which is inhaled at an elevated temperature are:—camomile flowers, mallow, lavender, valerian, henbane, hemlock, cherry laurel, tobacco, some volatile oils, and resinous substances. Like most inventive medical mechanicians, Lewin has a form of inhaler of his own, for which he claims superiority above all others. He refers to the vegetable substances named as having proved useful in his hands in rebellious cases of bronchial catarrh. The oil of the Pinus pumelion is smoked in a pipe, and would appear to constrict the dilated vessels of the bronchi, to promote expectoration, and to act as a diuretic. Professor Gerhardt, of Jena, places from six drops to two teaspoonsful of the oil of the Pinus silvestris in hot water, and orders the inhalation of the steam arising from it in various chronic chest diseases; not, however, in phthisis, in which it is useless, if not as may happen, prejudicial.

Lewin reproduces a paper on the method of inhalation as practised in Persia, written by Dr. Polak. The Persian pipe, or narghile, is the instrument used, and the medicinal agent is mixed with the tobacco smoked. The smoker not only draws the vapour (which in the pipe in question has previously passed through water) into his mouth, but actually also into his lungs by inspiration, and thus gains the full and direct effect of the medicinal materials used, the tobacco probably aiding by inducing a relaxed state of the vessels. In chronic catarrh and bronchitis the Persians mix ammoniacum and galbanum with the tobacco; in convulsive maladies and hysteria, assafœtida; in sciatica,

mastic. Quinine, digitalis, several narcotics, borax, and sal ammoniac, and other drugs may be used in a similar manner. In syphilitic affections the Persians likewise introduce mercury into the system by the same expedient, a cinnabar pastile being put into the pipe.

Polak's account of the effects of this sort of inhalation in the treatment of disease is very encouraging, and suggests the desirability of putting it on trial. A somewhat similar plan of introducing medicinal substances into the system has for a considerable period been followed occasionally, both in this country and on the continent of Europe, by the use of medicated cigars, of 'herb' tobacco, and of stramonium. The last named and other vegetable products have been smoked with the view of relieving various chest disorders, and have acquired more or less popularity, rather, however, outside the profession than in it. But the Persian plan presents a decided superiority, for it provides for the inhalation of the smoke or vapour within the lungs themselves, whilst it also guards against heated smoke, and necessarily adds a minute quantity of watery vapour to the material inhaled in its passage through the water of the reservoir of the narghile.

Dr. Beigel has no faith in such 'simples.' He regards their efficacy as very problematic and their application as resting upon "no scientific grounds." He, however, after penning this last clause, finds it involve a too sweeping criterion for judging of the propriety of administering medicines; a dangerous critical weapon, cutting like a two-edged sword every way, and calculated to smite fatally his own favorite plan of spray inhalation as well as the antique methods of inhaling medicated vapours and smoking herbs. Consequently, he forthwith seeks to modify and mollify his statement by observing that 'no strict scientific reason' can be given "for the application of the vast majority of the chemicals constituting the materia medica. . . . . we apply them as we say empyrically (*sic.*), and we must further confess that the most valuable medicaments range under this class." From these remarks we conclude that Dr. Beigel's unfavorable impression concerning the inhalation of medicated vapours depends rather on foregone theoretical opinions than on practical acquaintance with it.

In the inhalation of oxygen gas he discovers a most welcome example of a medicament for the application of which a scientific reason can be given, and to which, therefore, a first place ought to be allotted in the materia medica. The scientific reason discoverable may be stated in a few words. In all diseases in which dyspnœa forms one of the most dangerous consequences, the body generally is held to be suffering from a deficiency of

oxygen and the lungs to be oppressed by unnatural labour in endeavouring to effect the decarbonisation of the blood. Consequently, by supplying the lungs with pure oxygen, or with air containing a larger proportion of oxygen than commonly exists in it, the labour of those organs is lightened and the patient thereby placed in the best possible condition. "In diseases of the heart the same takes place, with the difference that imperfect circulation of the blood is the cause of its being insufficiently decarbonized" (p. 70).

Now it happens that the chemical philosophy contained in the above described 'scientific reason' for administering oxygen has not passed unchallenged. In heart disease physicians say it is contra-indicated, and in phthisis it has been urged there is seldom indication for throwing more oxygen into the circulation, the well-being of the whole system being kept in view. An excess of oxygen has been supposed to accelerate the destructive process of phthisis, and if this be so, the inhalation of that gas cannot be conceived a likely means to improve the condition of the patient. Yet phthisis is especially cited among diseases in which there is a demand for more oxygen than ordinarily attainable, except, as admitted indeed, in its inflammatory stages. Dr. Abbotts Smith excepts the last stage of tubercular consumption from those morbid states in which oxygen inhalation is useful, inasmuch as this proceeding is found to excite inflammatory action followed by increased expectoration and more urgent cough. He, moreover, speaks but doubtfully of the virtues of oxygen in other stages of phthisis; for all that he asserts regarding it is, that "when inhaled earlier in the course of the affection it *may be* productive of considerable benefit."

In these several works noticed this subject—the inhalation of oxygen as a curative process—is very slightly touched upon; and even the meagre account supplied is chiefly of a second-hand character, being borrowed from the researches of Demarquay, and affords very slight indication of the writer's own practical acquaintance with it.

Dr. Abbotts Smith has succeeded in giving the most definite lessons concerning the indications and contra-indications for administering oxygen. To bring our observations on this subject to a close we will briefly quote them:

"The efficiency (we presume efficacy is meant) of oxygen, is undoubted in most cases of disease attended by debility and diminution of the red corpuscles of the blood. In asthma, it sometimes acts like a charm; in the later stages of consumption it is inadmissible, and it is contra-indicated in affections accompanied by much fever; in deep-seated inflammatory disorders and visceral diseases; in most affections of the heart and large vessels (excepting cyanosis); in

neuralgia occurring in plethoric individuals, and when the hæmorrhagic diathesis or acute congestion is present."—pp. 18, 19.

This meagre account, though the best to be gathered from the works under notice, leaves much to be desired to render it a safe guide to the practitioner who would test the value of oxygen inhalation among his patients. It is, indeed, an account that affords little encouragement to medical men to make trial of this therapeutical agent. Even in spasmodic asthma—a malady in which its utility is more distinctly asserted—it is merely palliative; a sufficiently valuable quality, however, where suffocation is threatened, and therefore in this respect not to be despised.

It may be presumed that for the future the inhalation of vapours and gases as heretofore practised will be rarely resorted to by practitioners, now that the more effective plan of employing atomized fluids is making its way as a recognized proceeding, and is rendered more practicable by the improvement of the requisite instruments. This commitment, however, of the long-established modes of inhalation to the limbo of bygone inventions cannot take place without a passing regret that those modes were never fairly and fully put upon their trial. The powerful action upon the lungs and system generally of many gases and vapours is a matter assured to us by ample observation and experiment. Indeed, chloroform and ether, administered as anæsthetics in vapour, hold a position unshaken by the discovery of the inhalation of atomized fluids. But, apart from these two substances, many others still have a claim upon our attention as therapeutical agents. Watery vapour has been proved by Dr. Madden to be absorbed by the pulmonary tissue, and its great use in catarrh and in laryngitis has been well established since Dr. Mudge advocated it in 1779. The admixture of various balsams, of sundry oils and of camphor, with watery vapour has also been shown to be frequently beneficial in bronchitis, in laryngitis, and in asthma; and the diffusion of the vapour of oil of juniper with steam has been recommended, used by way of inhalation, as an active diuretic in dropsy; consequently it may be expected that though the older forms of inhalation should be generally laid aside by the profession, they will survive, among the public, as remedial expedients, and ever and anon will start into popularity, it may be in a new dress, and more or less disguised as novelties.

The first definite use of finely-divided sprays of atomized or pulverized liquids, brought directly into contact with the respiratory passages, was made by Dr. Sales-Girons, who also invented an effective portable instrument to produce the spray

and impel it into the open mouth of the patient. The instrument, as he contrived it, has since been subjected to various modifications and improvements. Lewin refers the several forms proposed to two classes: the one in which a fine stream of water is driven by the intervention of an air-pump against a disc, knob, or button, whereby it becomes broken into fine spray: the other in which either a current of air or of steam is employed to break up the fluid directly, on the principle of the scent-blowing tubes, so well known to all. The former plan is illustrated in the construction of the apparatus called by the Germans a "hydroconion," invented by Sales-Girons; the latter, by the hydroconions contrived by Bergson, Siegle and others.

Much ingenuity has been expended upon these instruments, to render them more portable and more efficient; and almost every doctor who has paid special attention to spray-inhalation has vaunted some improvement he has introduced in the mechanism of his apparatus. Dr. Beigel has not failed as an inventor; he has taken in hand Siegel's instrument, and with the co-operation of Krohne and Seseman, the instrument-makers, has contrived so to modify it as to reduce its cost and its dimensions, without lessening its efficiency. Indeed, he has so far succeeded, that an atomizer may become a *vade mecum* with the travelling invalid. But Dr. Andrew Clark has the merit of adapting an apparatus within the reach of all who desire to practise the plan of spray-inhalation; his instrument, according to the engraving given by Dr. Abbotts Smith, being nothing more nor less than the spray-producing apparatus suggested by Dr. Richardson for benumbing the surface with ether-spray, any difference subsisting being found in the substitution of the fluid to be used for inhalation in place of the ether contained in the bottle for rapid evaporization. The same writer above named also produces a sketch of an equally simple instrument, devised by Mr. Maunder, which, possessing less of the characters of an atomizer for inhalation than the several forms of apparatus above referred to, promises to be serviceable, particularly in laryngeal diseases.

Assuming that a spray-producer projects backwards into the fauces a cloud of spray, the question arises, Does this spray find its way into the larynx and lungs? Those who have examined into this matter, reply that such fine spray does not only reach the larynx and the bronchi, but the minute divisions of the latter also, and even the air-cells of the pulmonary tissue; and in support of this assertion they appeal to experiments upon the lower animals, and to the results of clinical observation in the human subject. Those who would acquaint themselves with the

investigations made on this point will have their labour much facilitated by the very excellent and complete summary presented by Lewin of the researches carried out by every French and German experimenter of importance since the question was first debated. But if unprepared to follow the German author through his extended analysis, they may peruse in Dr. Beigel's treatise a briefer outline of the observations *pro* and *con.*; though we question whether Dr. Beigel has suceeeded in putting the arguments in favour of the penetration of spray into the vesicular tissue of the lungs in such a shape as will convince the reader of the validity of his proposition, viz., "It is, therefore, confirmed beyond doubt that atomized fluids enter the respiratory tract, and penetrate into the very cells of the lungs; that, therefore, by means of inhalation, remedies most appropriately and successfully may be applied to the organs of respiration" (p. 26).

He quotes experiments on the lower animals, chiefly on rabbits, which show that inhaled spray has reached the larger bronchi; but that it may reach the small bronchi and air-cells is a proposition maintained only by an appeal to analogy, viz., that of the entrance of the fine dust of coal and other mineral matters within the respiratory passages of men exposed to its inhalation, sufficient at least to set up diseased action in the vesicular tissue itself.

Dr. Beigel writes thus:—"Now, if such be the case (*i. e.* of the penetration of small particles of solids into the lungs), it would *à priori* be very difficult to perceive why the same process should be impossible, if those particles be liquid instead of solid. But a conclusion *à priori* is quite superfluous, since positive evidence of the penetration has been afforded" (p. 25).

Respecting this line of argument we would suggest to Dr. Beigel that it is not conclusive. Because black masses of denser matter proved to contain coal-dust, and surrounded by more or less indurated tissue, "made up of dense, closely-arranged fibroid tissue," are to be found in the lungs of coal-miners, it does not follow that such particles of dust have at a previous period penetrated within the air-cells themselves, for it is assuredly equally as probable that they have got no further than the small bronchi, and there have set up the morbid action of which the indurated masses bear witness. We say that the inference in question does not necessarily follow from the pathological facts cited by Beigel, although we willingly admit that Lewin has pretty satisfactorily shown by experiment that fine coal-dust really may get within the air-sacs (Lewin, p. 89 *et seq.*). Nor again does the analogy of the penetration of mineral dust within the pulmonary tissue

prove that very finely divided spray can likewise effect an entrance as far within it, for the laws governing fluids are presumably adverse to this intimate penetration. The spray particles are disposed to coalesce on the surfaces on which they impinge, and also to unite with the moisture on the mucous lining of the respiratory passages, and, in a general manner, to be more seriously impeded in their inward progress by the ciliated epithelial lining of these passages. Lastly, the "positive evidence of the penetration" within the air-cells (as implied in the context and asserted in the proposition advanced) is certainly not supplied by Beigel; consequently, so far as his arguments and statements are concerned, Beigel has no ground for indulging in the angry tirade which appears at p. 16, against those who have not the like faith with himself in atomized fluids. Indeed, he seems to confound objectors against the doctrine of the vesicular impenetration of spray with antagonists to the use of spray inhalation, though it is clear enough that physicians may refuse to admit such penetration, and yet at the same time most readily and successfully resort to inhalation.

For the process of spray-inhalation the sitting posture with the head somewhat inclined backward is preferred as facilitating inspiration, and consequently the further penetration of the spray. The time chosen for it is either before taking food or some considerable time after meals. The tongue must be allowed to lie as flat as possible in the mouth with its tip resting against the lower incisor teeth. The depth of the inspiratory process must be regulated by that of the parts to which it is desired the spray should reach; the breathing, for instance, being shallow when the pharynx is subjected to the medication, and deep when the pulmonary tissue is to be reached by the spray. To hold the nose or to shut off the spray by any contrivance from it, as advocated by some writers, Lewin declares to be quite needless, observing that a downy feather held before the nose indicates no current of spray into its cavity, provided no impediment is offered to free inhalation through the mouth.

Both Lewin and Beigel refer to the difficulties encountered often when inhalation is attempted for the first time, particularly when patients are nervous; and both, again, devote a chapter to the consideration of the duration, the number of inhalations, and the doses of the medicinal agents employed, the remarks of the latter writer appearing to be pretty much of the nature of an abstract from the published work of the former. The time during which inhalation is to be continued must necessarily depend on the strength of the solution employed, and on the nature of that solution, as well as on the construction of the apparatus used,

and the distance at which the patient is placed from the point at which the spray is thrown off. Lewin has measured the strength and penetrating power of the several instruments he has used, and Beigel also recounts the results which he obtained with Siegle's apparatus; but no general rule can be collected from these painstaking experiments and calculations. Indeed, the physician must in every individual case be guided by his own observation, aided by what he has learned by previous practice. With respect to the number of inhalations in a day, Lewin advises as a rule only one, or at most two, in chronic cases, and three to four in acute disease; and he makes it a practice to examine patients with chest disease not only by percussion and auscultation, but also by means of the laryngoscope, before ordering inhalation.

What medicines may be administered by way of spray inhalation, and in what quantities, is conveniently summarised by Beigel in his sixth chapter. The list of such drugs is considerable; but the last contained in it appears to be a novelty introduced by Dr. Beigel himself, viz. cod-liver oil, which can also, as he tells us, "be turned into spray, if an apparatus is used with rectangular tubes, the openings of which are wider than usual, and a larger flame than commonly necessary is kept up." At best we cannot look upon this as an agreeable way of administering the oil, for the collection of the condensed spray about the mouth and fauces must assuredly be nauseating; and, on the other hand, we cannot agree with Dr. Beigel in believing that cod-liver oil so inhaled "would prove much more convenient and beneficial than when that substance is internally taken;" for we have no proof of any particular efficacy in lung diseases of cod-liver oil when brought into contact with the morbid tissues; indeed, observation as well as hypothesis concur in proving cod-liver oil to be essentially an alimentary material, operating beneficially when duly digested and appropriated by the system.

The remainder of the two treatises by Lewin and Beigel is occupied with a history of inhalation as applied to special diseases, illustrated by numerous cases. Dr. Beigel notices the application of spray inhalation first in diseases of the larynx and trachea, including laryngeal hyperæsthesia, œdema glottidis, croup, and diphtheria, and afterwards in diseases of the bronchi and larynx, including bronchitis, asthma, emphysema, hæmoptysis, phthisis, gangrene of the lung and convulsive cough. He has thought fit to preface his illustrations of the application and advantages of spray inhalation in these several diseases by a descriptive sketchy account of their symptoms and pathology; a modicum of information certainly needless to the

medical reader, who may presume to know what is intended by the terms œdema of the glottis, laryngitis, bronchitis, asthma, and the like, and one, therefore, calculated only to encumber and unduly augment the size of a work addressed to the profession, and valuable only for what special information it contains concerning inhalation.

Trousseau successfully applied a pulverized solution of tannin in two cases of œdema of the glottis, and the value of spray inhalations in laryngeal affections which are so little amenable to general treatment, might *à priori* be admitted, and is abundantly established by all writers upon throat diseases. We can, moreover, concur with Dr. Beigel in the inference drawn from cases on record, "that, by means of inhalation, we are often enabled to rid the patient, not only of troublesome symptoms, but of really inveterate diseases, which have for a long time resisted different kinds of treatment (p. 97).

In cases of croup and diphtheria Beigel recommends not only spray inhalations, but also the inhalation of oxygen,

"Not to cure the disease, but to guard the patient from the consequences of suffocative attacks which frequently prove fatal, and to place him in such a position as to enable him to apply other remedies. Oxygen, under such circumstances, has in some measure the same effect as tracheotomy, whereby we intend to evade (?) a particular part of the respiratory tract, in order to gain time till we possibly master the inflammation as well as the formation of the pathological products represented by the croupous membrane."

Probably owing to his incomplete knowledge of the English language Dr. Beigel treats what is commonly called a "cold" as synonymous with acute bronchitis. But, apart from this erroneous identification of two conditions, he seems to entertain opinions regarding the characters of bronchitis at variance with those generally held. For, although consolatory to the sufferers from acute bronchitis, it will scarcely appear credible to the physician that "a few inhalations of any astringent solution, alum, tannin, iron, will suffice to cure it in a short time." Nor will the wide and loose statement be admitted by medical men, that "chronic bronchitis is of a much more grave and serious character (*i. e.* than the acute form), and one of those diseases which obstinately resist medicines." Nor, again, is the pathology of this malady more satisfactorily set forth in the following extract: "Its characteristics are hypertrophy and thickening of the mucous and muscular membrane of the bronchi, whereby these membranes are deprived of elasticity, and, therefore, rendered prone to excavations."

Under the term asthma Beigel treats only of the spasmodic

form, "an affection of the vagi nerves," attended by spasmodic "contractions of the muscular fibres of the small bronchial ramifications and of the lung vesicles," in the treatment of which, although pulverized fluids are auxiliary, the inhalation of oxygen, in doses of two gallons twice a day, constitutes the principal therapeutical agent. In emphysema, likewise, oxygen inhaled occupies the foremost place among its numerous reputed remedies.

Hæmoptysis is adduced as a condition in which the value of spray inhalations has been especially demonstrated. A pulverized solution of the tincture of sesquichloride of iron is the agent mostly resorted to in suppressing it, and Beigel quotes a case to prove the intricate penetration of the iron into the pulmonary tissue when inhaled in the manner referred to. It was that of a soldier suffering from Bright's disease, with dropsy and hypertrophy of the heart, and mitral insufficiency, who was suddenly seized with violent hæmorrhage from the lungs. He was made to inhale the solution of iron, and in two minutes the hæmorrhage was stayed. "But it had consumed the last powers of the patient, who died two days after under suffocating phenomena." Sero-sanguineous fluid occupied the right pleural cavity, and several blood-clots were infiltrated in both lungs. "Dr. Holm examined the clots, and in all parts of the tissues of the lungs discovered much larger quantities of iron than is generally contained in the blood." In this case we may remark that though the inhalation of the pulverized solution by a prostrate, fainting man, just recalled to consciousness by being sprinkled with cold water (as detailed) during a space of two minutes, must have been hurried and imperfect, it may be granted that some small quantity of the spray may have entered the respiratory tract, sufficient, moreover, to be detected after death, as an amount in excess of the normal proportion of iron in the blood and lungs, yet we are sceptical respecting the conclusion drawn therefrom, that this cloud of spray actually had the power to stop the hæmorrhage. Such gushes of blood but once occurring, are ever and anon met with where no styptics have been administered; and in the case cited it is clear that an amount of unsuppressed internal hæmorrhage went on subsequently to the ejection of the blood from the mouth.

After some unnecessary general reflections on phthisis, Beigel proceeds with a brief notice of the application of inhalations in that fatal disease. The inhalation of oxygen he believes might prevent or delay the outbreak of consumption in children predisposed to it, but in the advanced stages of the malady he honestly can promise no more advantage from "inhalatory treatment" than the relief of distressing symptoms, the improve-

ment of nutrition, and the perhaps possible prevention of further deposits. Among the conditions for the relief of which he values inhalation, are the distressing cough which deprives the patient of his night's rest, excessive expectoration, attacks of suffocation and morning vomiting, as generally induced by the efforts made to dislodge the sputa.

The section on gangrene of the lungs and that on convulsive cough are little to their purpose as illustrative of the advantages of inhalation, the former containing the narration only of a case of advanced phthisis with fetid breath and sputa; the latter only two cases of hooping-cough, borrowed from foreign periodicals. Indeed, with regard to the last-named disease, Beigel admits the failure of inhalations in his own practice, on account of the difficulty with children in their administration, and indicates a preference to hypodermic injections, with which, so far as his experiments have gone, "no remedy will be able to stand in comparison, as to the convenience of administration and the certainty of the results" (p. 195).

Lewin's history of the application of inhalations in disease is much more extended than that attempted by Dr. Beigel, and includes pathological dissertations on the various lesions passed in review. The first chapter is occupied with hæmorrhage and its causes occurring in the organs of deglutition and respiration. Hyperæmia, inflammation, and ulceration, and, in the case of the larynx, new formations, are the enumerated causes of hæmorrhage from the parts in question. Of these ulceration is the most important by reason of the variety of causes inducing it, which are all detailed and classified by Lewin in the extensive chapter referred to. This review of a symptom—hæmorrhage from the respiratory passages in its relations to the tissues and morbid processes from which it may proceed—has considerable value in itself; but, nevertheless, we may raise the same objection to it as we did to the pathological sketches in Dr. Beigel's treatise, viz. that it is not wanted in a work which the student takes up for the purpose of informing himself of the methods of practising and the indications for using inhalation, and not for dissertations on pathology.

The eighth section of this chapter on hæmorrhage is, however, to the purpose. In it are described the medicines for, or, as Lewin writes, the materia medica of inhalation, and the principal indications for their use in suppressing hæmorrhage. Cold water is first mentioned, and after it follows solution of sesquichloride of iron, tannin, and alum. Various metallic salts and vegetable products, *e.g.* catechu, kino, and extract of rhatany, are alluded to as worth a trial. In the instance of the solution of iron, Lewin first recounts its physiological action on animal

tissues, and then proceeds to show where its operation by inhalation is likely to be beneficial, and where inoperative or detrimental. The same course he pursues with regard to tannin and alum, the other two principal hæmostatics, and in conclusion adds the doses commonly given.

Of forty-seven cases of hæmoptysis the hæmorrhage was stopped in thirty-six after the first inhalation; in the remaining eleven inhalation was repeated several times before its arrest. In the greater number—but what that number was he has failed to note—he had no history of subsequent attacks of hæmoptysis. Lewin very justly observes that styptics cannot close up the sources of future bleedings, nor can stay the course of tubercle or the over-action of a hypertrophied heart. In by far the larger proportion of his cases he employed the ferruginous solution, though in sixteen he used tannin and in two alum. In thirty-eight of the forty-seven cases of hæmoptysis he ascertained the cause to be, in twenty-nine tubercle, in four cardiac disease, in two vicarious of suppressed menstruation, in three overstraining. Thirty-nine of his patients were men and only eight women. Five cases are fully reported to demonstrate the virtues of the inhalatory treatment pursued.

The second chapter is occupied by a pathological history of inflammatory disorders of the pharynx and larynx, and their results. A painstaking attempt is made to distinguish and classify the several forms of inflammation according to the tissue or part affected. Six forms of pharyngitis are described, and in the case of the larynx the author adds to the ordinary nosology by describing various lesions of its walls and of its several anatomical elements. The second section of this chapter is occupied with an account of the medicines employed, their indications, contra-indications, and doses for inhalation; whilst the third chapter is a clinical record of cases in illustration of the contents of the one preceding.

The fourth and concluding chapter is devoted to the pathology and inhalation-therapeutics of diphtheria.

In Lewin's practice diphtheria has proved a very curable disease when treated sufficiently early, before the larynx is involved, and whilst the false membrane is limited to the pharynx. He applies locally chromic acid diluted with four to ten parts of water, as an antiseptic tending to disintegrate also the false membrane in its process of deoxidation; and to prevent the reproduction and to heal the morbid state of the mucous membrane, he administers by spray-inhalation solutions of tannin, of alum, and of sesquichloride of iron, &c. Where the disease has invaded the larynx, inhalation, in his opinion, offers the most direct and efficacious remedy available.

In importance, as a history of inhalation and a record of its practical application, Lewin's treatise occupies the foremost place among books on the subject. It contains an account of much original experiment and research, of extensive clinical observation, and of the labours of others in the same field of inquiry. Dr. Beigel's volume may be regarded as a *résumé* of the present state of knowledge respecting inhalation. As such it is calculated to be very welcome and useful, for it is the only book published in England which can pretend to any claim to adequately instruct the profession in the subject of which it treats. The other two productions of the press quoted at the commencement are of the fugitive pamphlet sort, to be considered as a sort of literary trial-balloons, thrown out to see if the current of opinion sets fairly for their makers to follow on with larger, or, as sometimes happens, to attract the public gaze to the makers of the new literary bubbles. Dr. Abbotts Smith deserves some credit for the matter he has collected; but of the pamphlet last in the list, which required, it seems, two weighty M.D.'s to manufacture and launch forth into the world, we will say no more than that it contains just about the quantum of second- and third-hand information which an instrument-maker is accustomed to collect and print in illustration of the advantages to be derived from a new or improved instrument in which he is particularly interested.

Review VII.

Lectures on some of the Applications of Chemistry and Mechanics in Pathology and Therapeutics. By H. Bence Jones, A.M., M.D., F.R.S., &c. 8vo, pp. 314.

The book before us is one of the most important, probably, indeed, *the* most important contribution to medical literature which has emanated for a long time from the chemical school of pathologists. Twenty years ago the rapid advances of organic, and particularly of animal chemistry, were revolutionizing our ideas of disease, and promising to give us at least a tolerably firm basis for our therapeutics. The fair promise in that respect, which seemed to dawn through the brilliant generalisations of Liebig, and which had even been foreshadowed by our countryman Prout, was not, indeed, fully kept; but some of the acquisitions of that tide were never lost by the succeeding ebb of scepticism. Pathological anatomy and histology absorbed

for a time almost all the labour and thought of our more zealous and ambitious students; and our knowledge of the nature and operations of disease was thereby greatly extended; but the more we knew of our enemy the less confidence we felt in the weapons with which we had to encounter it. If we retained any trust in them, it was due to such men as our author, men who had always reasons to assign for the faith that was in them, and who, bridging over the chasm between chemistry and pathology, sought to direct the resources of the one in accordance with the light yielded by the other.

"For the last century," says he (pp. 7, 8), "physiology and medicine have been chiefly occupied with the determination of the structure of the organs of the body in health and disease; even up to the present time the microscopical variations in the form and substance of the different parts of the body yield remarkable discoveries, but the inquiry into the conservation of energy in the body promises results which will unfold the mystery of health and of disease."

These are high hopes; but at present no more than hopes and expectations. He proceeds, however, to state briefly the few important facts which constitute the advances we have actually accomplished in this direction, the effects in relation to conversion and manifestation of force, of rest and motion, of heat and cold, and of food. Though he does not commit himself to either the old or the new view of the history and uses of azotized food, his words (pp. 10, 11) betray a leaning towards the latter.

"Food acts both chemically and mechanically; chemically, by furnishing: 1st, latent force; and 2nd, material for the formation of the organs by which that force is made active. . . . Both carbonaceous and nitrogenous elements can be acted on by oxygen, and can thus change their latent into active force. Hydrogen, carbon, nitrogen, sulphur, phosphorus, iron, and lime, enter into the composition of the organs by which the conversion of latent into active force is effected. Ultimately, more or less completely, these elements are themselves acted on by the oxygen, and thus no distinction exists between respiratory and plastic food. Professor Liebig's division of food, however, gave the first impulse towards the clear comprehension of the two great chemical actions of oxidation and nutrition which take place in each particle of the body. I hope to prove that extreme variations of these actions constitute disease."

After the programme thus enunciated, the first and second lectures, "On the Chemical Circulation in the Body," and "On Fluorescence in the Textures," strike the reader at first sight as somewhat irregularly placed, whereas the remainder of the book

is digested into a very orderly arrangement. Several reasons might, however, have been assigned for their location in the early part of the book. In the first place, the discoveries detailed in them threw a sort of preliminary and general illumination on the whole breadth of therapeutics; in the second, they will constitute, if confirmed, as there seems little reason to doubt they will be, at least in part, some of the greatest triumphs of animal chemistry, as applied to the foundation of a rational system of therapeutics, triumphs of which Dr. Bence Jones may be justly proud, and which he may well put forward to the front, when he is about to fight the battle of his favorite science.

The lecture on the chemical circulation briefly narrates the results of certain experiments undertaken by the author, with the help of Dr. Dupré, in order to test the rate of passage of crystalloïd substances into and out of the textures of the body, the method of spectrum analysis being employed, and chloride of lithium and carbonate of lithia being in most cases the crystalloids selected, by reason of the absence of lithium in any notable quantity from the normal human tissues, and the ease with which its spectrum may be recognised. There are two series of experiments; one on guinea-pigs, from which it appears that a moderate dose of lithium injected into the vessels reaches every organ except the lens in about four minutes; the second series was made on the lens in the human subject, advantage being taken of the opportunities afforded by the operation for cataract. A dose of twenty grains of carbonate of lithia having been taken, lithium appeared in the cataract within two or three hours. It would seem that several days elapse before the lithium thus absorbed is entirely got rid of. This great rapidity of the chemical circulation may, perhaps, be sufficiently accounted for by diffusion through the walls of the capillaries; but it appears to us that it lends additional probability to the views and observations of Dr. Carter, of Leamington, who finds a system of vessels much smaller than capillaries, and pervious to liquor sanguinis, but not to blood-discs, in various tissues of the body.

Alkaloids, as we shall see presently, are believed by the author, on experimental evidence, to be diffused into the textures in the same way, if not quite at the same rate, as the true alkalies. And he supposes the same to be true of the vegetable acids. "The action of alkalies out of the body," says he, "on the different classes of substance of which we are built up is sufficiently clear. The alkali disturbs the equilibrium of the elements by its affinity for acids; it promotes oxidation. The chemist can have no doubt that the same action takes place in

each particle of the texture to which the alkali is carried." "Acids when they have passed into the textures must have exactly the reverse action to alkalies; by lessening the alkalescence of the serum and tissues, vegetable acids must tend to stop the oxidizing process." After discussing briefly the probable actions of metallic salts when they have been in like manner introduced into the textures, and restating his opinion that the experiments indicate the existence of another and a greater and a more strictly chemical circulation, in addition to the mechanical or animal circulation of the blood, he concludes the chapter with [p. 27] "two most important inquiries."

"First, whether substances that diffuse into this larger circulation act as they would do out of the body under somewhat similar circumstances upon the different substances with which they come into contact in the different textures; either promoting the formation of new compounds, or giving rise to decompositions in the substances that are present in the tissues. And, secondly, whether the chemical force which may have been latent for ages in the mineral and vegetable substances that can enter by our food and medicine into this larger circulation, may be so given out in the textures as to increase or diminish those actions of oxidation, motion, sensation, and nutrition, which almost, though not altogether, constitute that assemblage of correlated actions which we sum up in two words—Animal Life."

The next lecture, entitled "On Fluorescence in the Textures," is taken up with the details of the discovery in the animal body of a fluorescent substance closely resembling quinine, but apparently not absolutely identical with it. This substance has been called *animal quinoïdine*. The therapeutical bearings of this discovery, if it should be confirmed, will probably, as before remarked, be of great importance. The history of the passage of quinine through the tissues comes out in the narration of the experiments. It would seem that the quantity in the tissues is at its maximum in somewhere about three hours after a dose is taken, and that the whole has not been eliminated three days afterwards. It is curious that the quinine seems to enter the nerves in less quantity than any other of the tissues examined, except the lens and humours of the eye, whose non-vascularity accounts for their comparative exemption.

In the third lecture the author begins the consideration of the "diseases of sub-oxidation," with the subject of diabetes mellitus, as being the best example among human diseases of a purely chemical derangement of health. He seems to indicate the absence or deterioration of a hypothetical sugar-ferment as the probable cause or chief cause. Believing that healthy urine always contains sugar, he speaks of excess of farinaceous or

saccharine food as causing temporary diabetes. We think he makes this statement rather too positively. The writer of the present notice once consumed about twenty ounces of honey at a sitting, with the view of testing the effect on the urine of this excess. The honey was taken on an empty stomach, and without any admixture except that of water and about two ounces of milk. The consequent phenomena were drowsiness, dull headache, and the passage of a moderate quantity of acid urine loaded with lithates. The indications by Trommer's test were obscure; but no sugar could be found by other tests, including fermentation. The bowels were unaffected.

The author makes two stages of the disease: in the first, vegetable or food-sugar alone is not oxidated, and it is possible by careful dieting to cause the disappearance of glucose from the urine; in the second, animal sugar also is more or less unchanged, and an anti-farinaceous diet has no effect. To this we must demur. If the second condition always involved the first, as the greater includes the less—if, in other words, it were always a second stage of the first—an anti-farinaceous diet ought always to have the effect of diminishing the glycosuria, though not of abolishing it; but there are cases (very few, according to our experience) in which it seems to have no effect at all, and these are not always the worst cases; they may constitute a separate *variety*, but not a *stage* of the disease. We agree, therefore, with Dr. George Harley in making at least two varieties, though we are not sure that his explanation of them is correct.

Some of the author's cases are extremely interesting, especially those of acute diabetes. He seems to regard phthisis as a mere result of imperfect nutrition of the textures implicated.

In treatment he of course favours alkalies and iron, on theoretical grounds, as promoters of oxidation; and the utility of opium, which is an opponent of oxidation, is but sparingly admitted. It is spoken of as lessening the quantity of water passed; but there certainly are cases in which the amount of sugar is also diminished under its use. This part of the lecture contains an admirable account—by far the best we have seen—of the dietary proper for diabetics, including a voluminous *carte* of permissible dishes by Professor Bouchardat, after reading which one is disposed to think that the best prescription for a wealthy diabetic would be a French cook. The whole subject of the treatment is handled in a masterly way.

In the next lecture ("On Acidity, or the Acid Diathesis") the pathological views of the author are further developed. Like almost all the book, it is written in so terse and succinct a style, and contains such a mass of important fact and ingenious

theory, that to give anything like an analysis of it in the course of a review would be impossible. All we can do is to select a brick here and there as a sample of the material of the edifice.

Excess of acidity, or the acid disease, may shortly be defined to be a want of free oxidation and an insufficient escape of the products of imperfect combustion."

He disbelieves the existence of Scherer's acid fermentation of the urine. In speaking of the symptoms of undue acidity, he says, "Slight degrees of acidity hardly irritate the skin; but itching, nettlerash, eczema, and herpes, are the outbursts of an over-acid state." It is a curious fact, which was brought out in the course of a recent discussion in a London medical society, that some dermatologists are not yet aware of this important truth.

He divides the treatment into dietetic and medicinal, and proceeding, we should say, rather deductively than inductively, says the best diet for diabetes is the best for over-acidity. He allows, however, that though the farinaceous and saccharine substances do give rise to various acids in their progress through the system, a smaller restriction on their employment will give greater results in this disease than in diabetes; and, moreover, that fatty substances, such as cream, fat, and oil, which in diabetes are so useful, in over-acidity frequently disagree and cause heartburn. The conclusion he therefore arrives at is, that small quantities of lean animal food, including fish, flesh, fowl, game, and eggs, with brandy and water, constitute the best anti-acid diet.

"Exactly the opposite diet," he continues, "was advised at the dawn of animal chemistry; on the ground that the ash of vegetables containing alkalies, these would neutralize all the acid that should arise from the starch and sugar. But take, for example, potatoes. The amount of acid-producing substances in them is from 20 to 30 per cent.; the amount of alkali at most will not reach to 1 per cent. Or take bread: the ash of bread would neutralize at most 1 per cent. of acid. The amount of acid-producing substances in bread is 46 per cent. Any other fruit or vegetable would give equally good evidence against a vegetable diet in over-acidity."

This looks plausible, but surely a fallacy underlies it. Following up this line of reasoning, one would expect to find the urine of the herbivora giving evidence of a preponderance of acid in their systems; but it does nothing of the kind—quite the contrary. Dr. Jones seems to us to have momentarily forgotten, when he was writing these lines, that even if the whole 25 per cent. of starch in the potato must pass through a stage of existence in the system as an acid or acids, there is no

necessity for supposing that the whole of it must be in the same stage at once. The same modicum of fixed alkali may, therefore, serve to neutralize successively the whole of this large quantity of acid. Again, he allows (p. 95) that "there can be no doubt that albuminous substances, whether of vegetable or of animal food, give rise to uric acid when an excess is eaten; and when uric acid is imperfectly oxidized it divides into oxalic acid and urea; also (p. 107) that "all the albuminous substances of animal or vegetable food, before they are thrown out of the body, pass through the stage of uric acid; and again, that "alcohol itself gives rise to acid;" so that his own standard diet, just mentioned, is not free from objection from the oxyphobic point of view, for except under such circumstances and with such habits as those of the hunters of the Rocky Mountains, it is hardly conceivable that any one should long continue to supply the demands of oxidation with azotized food alone without taking it in excess.

The medicinal treatment is considered by the author under the heads of "Removal" (by emetics, Turkish baths, &c.) and "Neutralization."

The next lecture, "On the Oxalic Diathesis," is very interesting. The formation of the mulberry calculus is dwelt upon as a signal instance of a mechanical complaint of the most grievous and distressing nature, arising out of a chemical disorder, which but for this possible consequence would have little or no importance at all. A noteworthy practical point is that the presence of oxalate of lime in the urine may be suspected when sudden changes are observed in the quantities passed in successive periods of twenty-four hours. The treatment for the diathesis and for the resulting mechanical disease, so far as it can be treated without the aid of the surgeon, is pretty much the same, and founded mainly on the principle of promoting oxidation. Lessening the amount of lime introduced into the system in the drinking water is proved to be advantageous by experiments detailed at p. 101. The formation of the crystals is found to be checked also by the popular method of treatment by mineral acids. At pp. 103, 104, are some pregnant remarks on these acids as therapeutic agents. "Hydrochloric acid," we are told, "rightly used, promotes digestion, nitric acid secretion, sulphuric acid constriction."

"And first, regarding hydrochloric acid. As this is more especially the acid of the gastric juice, it seems reasonable that it should be prescribed more particularly when there is feeble digestive power; after long illnesses, when animal food begins again to agree, it should be taken almost immediately before each meal of

animal food. Thus the greatest immediate use can be got out of hydrochloric acid."

We would gladly have had the author's reasons for his method of giving the acid more at length. We have ourselves always administered hydrochloric acid immediately or speedily *after* a meal, and have believed ourselves to have acted on sound chemical principles; for, in the first place, if the dose be given but a few minutes before the food, some portion of it will have been absorbed before it can begin to act; and in the second, the presence of adventitious acid in the stomach may probably lessen the secretion of gastric juice, which, if given when the secretion has already to a great extent or wholly taken place, it cannot, of course, notably affect. Dr. Wilson Fox, who has some good remarks on the use of hydrochloric acid,[1] says—"immediately before, or during, or directly after meals." Nitric acid Dr. Jones recommends to be given an hour or two before food, that, being absorbed, it may act on the system. He speaks of it as, perhaps, promoting oxidation as well as secretion. This should, we think, have been further explained, as it forms an apparent exception to one of his leading principles, viz. that acids are antagonistic to oxidation. That nitric or nitro-muriatic acid does sometimes prove useful in the treatment of gout is a noteworthy fact in this connection.

In several succeeding lectures the author carries on the consideration of the diseases of sub-oxidation, including the uric acid, xanthin, and cystin diatheses, and their developments.

As to the sources of urea, he adheres, without question or qualification, to the doctrine of *luxus consumption*, which may, indeed, we suppose, be now regarded as pretty securely established.

In speaking of the secondary (mechanical) diseases, viz. gravel and stone, which arise from excessive formation and excretion of uric acid, and of the tertiary ones, such as pyelitis, which follow again upon them, he mentions two or three remarkable and instructive cases. One of these (p. 113) seems to prove that a stone may remain in the kidney during fifty years of active life. In another, the lodgment of a mulberry-stone, not half a grain in weight, in the ureter, caused absolute stoppage of urine for nine days, the passage of the stone being followed by complete recovery. Except a case published by the late Dr. Alexander, of Wooler, we do not remember an instance of recovery after ischuria or retention so protracted. As to the treatment of gravel—

"The first question is regarding its solubility. The solution of

[1] On 'Dyspepsia,' pp. 116-17.

uric-acid gravel in any alkaline fluid that can be borne in contact with the mucous membrane of the urinary organs is scarcely possible. . . . . Acid gravel has not been dissolved in the kidneys or bladder by medicines; still, my friend Dr. Roberts, of Manchester, is confident of success—more so than I am."

Herein, by "medicines," he means remedies taken by the mouth; of the ultimate success of some plan for dissolving uric-acid stones and gravel in the bladder he has little doubt; his own ingenious electrolytic method, easy and effectual when tried on a stone in a glass vessel, has not hitherto been successfully applied within the bladder; but "at present progress is stopped more by the mechanical than by the chemical difficulties of the problem.

The interesting chapter on gout contains the following episode relating to its cousin rheumatism. We quote it partly for the plausible and charitable theory of rheumatic gout—charitable, we mean, towards those unhappy practitioners, among whom we must confess ourselves included, who, not altogether without a guilty sense of disloyalty to the dictates of modern science, have retained rheumatic gout as a sort of haven whereunto to resort in cases of doubtful diagnosis:

"Rheumatic inflammation so closely resembles gouty inflammation in all its local and general phenomena, that no positive differentiation between the two diseases can be made, except in the materies morborum. The rheumatic poison has not yet been isolated; that it differs from uric acid in its solubility, and probably in its volatility, is shown by its not forming calculi, not depositing itself visibly on the joints, escaping by the skin, and giving rise to local symptoms when the secretion of the skin is checked by cold and wet. In the analysis of sweat as yet only one acid is known that at all approaches to uric acid in composition—sudoric or hidrotic acid ($_5CH_8NO_6H_2O$?). It may be that this is the rheumatic poison, and that an excess of it in the blood causes the rheumatic diathesis, and its local oxidation a rheumatic attack. But whatever the acid, there is no reason why the rheumatic and gouty acids should not coexist in the same blood, causing a rheumatic-gouty diathesis; nor why a local oxidation of both acids should not take place at the same time in the same part, constituting an attack of rheumatic gout. Certainly there is no positive proof as yet that this happens; but animal chemistry is still very young, and will speak much plainer on this and many other subjects when grown up."

The treatment of gout is divided into specific and expectant. Under the former head the action of colchicum is carefully considered, and the theory that it may act directly on the nerves of the capillaries at the seat of inflammation is regarded with some

favour. The author thinks little of the eliminative virtues of colchicum. The balance of experimental evidence is certainly against its possessing any power to increase the excretion of uric acid (Parkes 'On Urine,' p. 172); but we are, nevertheless, disposed to attach some weight to the beautiful series of comparative experiments on diuretics by Hammond, according to which colchicum differed from all other diuretics in increasing the organic solids of the urine. With respect to diet, Dr. Jones says, tersely enough, that "a minimum of albuminous food should be taken in order to produce the least uric acid; and a minimum of carbonaceous food in order to allow the uric acid to be oxidized as much as possible." In the calculations of the amount of food necessary to supply waste and its chemical composition (p. 141), there is an error, probably typographical, which it may be well to point out. One ounce of cooked meat is said to contain seventy-seven grains of nitrogen and ninety-two grains of carbon. Now, an ounce of cooked meat does not contain half that amount of nitrogen; and even if we suppose perfectly dry lean meat to be spoken of, though the nitrogen might thus be brought, perhaps, just within the range of possibility, the carbon would err just as much in the other direction. The proportion of nitrogen to carbon in lean cooked meat should, of course, be somewhere about three and a half to one. The error we have pointed out does not, however, affect notably the subsequent calculations. Into the practical details of the dietary for gout the author does not enter; for the beef-tea and arrowroot in the text are meant, we apprehend, to serve merely as algebraical a^s and b^s. Beef-tea is probably objectionable in quantity in this class of diseases, as containing a great deal of those products of retrograde metamorphosis which seem already to superabound in the gouty blood.

There is a particularly good account of the long train of "mechanico-chemical and chemico-mechanical diseases and infirmities which may follow on the original chemical error in gout." After a short account of those varieties of deficient oxidation, which are known to us only by their producing xanthin and cystin in the urine, the first part of the book concludes, after a short discussion of the part played by the nervous system in controlling or regulating oxidation, in the following words:

"It may be said that the nervous force, without the intervention of any action on the blood-vessels, can exalt or depress the conditions necessary for oxidation, and increase or diminish the ordinary action in the capillaries and in the cells which are nourished by the liquor sanguinis. Without hesitation the vitalist adopts this view. The chemist, on the contrary, will admit it only when animal chemistry

is so far advanced that it can be proved that the quantity in which the active substances are supplied, and the rate of removal of the products of the action, are clearly insufficient to account for the increased or diminished oxidation which in these different circumstances is found to occur."

The second part of the work, entitled "On local Chemical Disorders arising from Sub-oxidation, and their Mechanical Results," is chiefly occupied with the consideration of the effects of extreme cold, and of the causes of fatty accumulation and degeneration. These causes or conditions are said to be three, viz., 1st, excessive supply of fat or other food capable of forming fat; 2nd, no excess of supply, but insufficient removal by oxidation; 3rd, insufficient oxidation of nitrogenous substances, giving rise to fatty products of decomposition. The subject of phosphorus poisoning, in which the active agent is shown to be phosphoric acid, and that of poisoning by other mineral and vegetable acids, is gone into at length, with the view of showing that these acids bring about acute fatty degeneration by destroying or disqualifying the blood-discs, and thus checking oxidation, the check being evidenced by the lowering of the temperature. The only flaw we can see in the chain of narrative and reasoning is this, that in those acid-poisoned animals which died quickest the fatty degeneration was most apparent; and in the cases of poisoning by sulphuric acid it was so in a patient who lived only three hours. Now, the amount of carbon which ought to be oxidated in the human system during that period scarcely exceeds an ounce; and if fat were formed only to a corresponding extent, it is scarcely credible that it could very notably affect the chemical constitution of the viscera. The reasons why the liver is of all organs the most prone to suffer from fatty deposit are exceedingly well put.

Part III is devoted to some of the chemical disorders arising from peroxidation. In the first two lectures the author confronts the great problem against which every pathologist in his turn has dashed himself in vain—that of the nature and starting-point of inflammation. He visibly rises to the task, and clearing away the vascular and nervous theories as irrelevant to a process which can take place in cornea and cartilage, briefly and clearly lays down his own view as follows:

"That it is closely related to the natural actions going on in the body is now fully recognised. Thus it has been stated to be 'a modification of nutrition,' 'a destruction of the equilibrium in the molecular attractions in the body, and of the reciprocal changes between the textures and the blood.' But these expressions are

dark and fruitless compared with the clearness you will obtain, if I can convince you that inflammation in its first origin usually is an exaggeration or excess of the ordinary oxidizing action that appears in each part of the body, and that this increased chemical action sets up secondary mechanical derangements varying with the structure of the textures in which the inflammation is set up, and reacting on their chemical repair . . . . . It has its origin in the causes which produce the natural heat of the body. The oxidation rises to a peroxidation; it is a purely chemical wrong, producing almost immediately a secondary mechanical derangement—increased motion of the blood-globules—from which excessive motion an obstruction of the blood-vessels arises. Hence proceed increased tension, increased pressure, and increased effusion of lymph from the liquor sanguinis. The effused fluid varies in its composition with the amount of pressure and oxidation. Lastly, from the increased heat, and the increased circulation, and the increased effusion of lymph, an entire change in the nutrition of the part follows; and these actions are so related, and so react the one on the other, that it is difficult exactly to estimate the part which each one separately plays in producing the result."

There is a grand simplicity in this view of inflammation which is very attractive at first sight; and, for ourselves, we must confess that further examination has made us regard it with still more favour. We must refer the reader to the work itself, that he may see the arguments by which it is supported, and the way in which the modifications of the process and its results, in particular organs and tissues, are shown to be just such as might have been predicted on the grounds set forth.

Next follows, by way of exemplification of the application of this doctrine of inflammation to the most complex cases, as well as of the inter-dependence of chemical and mechanical disease, a sort of brief treatise upon Bright's disease, or rather Bright's diseases. The division of this group of affections adopted is threefold, and may be represented thus:

Peroxidation of the arteries	=	So-called amyloïd or fibroïd disease.
„ „ matrix	=	Interstitial nephritis or cirrhosis.
„ „ tubes	=	Tubular, or so-called desquamative nephritis.

The account of the first of these affections is, perhaps, not so clear as those of the other two, and liable to more exception; but, in truth, that part of the subject is the most obscure, and the basis for theorising on is not so far advanced. The circulation of stimulant agents, that is, practically, of alcohol, is stated to be the usual cause of interstitial nephritis; not, however, we apprehend, to the exclusion of alcohol as a cause of the first

variety also, since if a stimulus of this kind is capable of setting up peroxidation in the interstitial tissue, it may be supposed to be equally capable of doing so in the walls of the vessels with which it primarily comes into contact. The history of a dose of alcohol is very well given; the author holds that it is partly oxidated and partly excreted. Tubular nephritis is ascribed to increased chemical action, brought on by the necessity of excreting foreign matters, such as scarlatina-poison, and perhaps some substances that should pass off by the perspiration. The old theory that scarlatinal nephritis is simply a desquamation analagous to that of the skin is justly rejected. A distinction is drawn between primary or acute and secondary or anæmic dropsy, the former being ascribed to increased chemical action set up in the cellular tissue by the diffusion thereinto of urea and its progenitors. The treatment recommended, which flows naturally from the principles laid down, presents no striking peculiarity. He allows the use of various diuretics in anæmic dropsy, but prescribes all except digitalis in the primary stage.

The next lecture, "On Fermentations and Fevers," is full of acute observation and ingenious and daring theory. He extends the idea of zymosis almost as widely as Dr. Farr does the name, but with better reasons to show for doing so. Thus he holds ague-poison to be a ferment, and takes true syphilis (for he acknowledges the binary doctrine) as the type of a class of zymotic diseases in which errors of oxidation are slight, while qualitative and quantitative errors of nutrition chiefly mark the action of the poison. He draws analogies between syphilis and smallpox, among which the following is an interesting and, we believe, a true one (p. 234).

"The protective power of the alteration is in a slighter degree extended to the progeny through the germ and spermatozoon, so that a race partly protected by inheritance may suffer less from these diseases than a purer race, whose textures are free to undergo the full change which constitutes the disease."

Naturally, as he adopts the ferment rather than the specific virus theory, he has no objection to acknowledge *de novo* or spontaneous origination.

"It is in vain now," he says, "to ask what circumstances at the end of the fifteenth century produced the first modified albuminoïd matter which gave rise to the first true syphilitic poison. In cancer, which bears a distant resemblance to syphilis, although the spontaneous generation of the first cancer-cell is daily occurring in some predisposed texture, we are as yet quite unable to say what produces the first modified particle of matter which multiplies and communicates its composition to adjacent predisposed textures by contact, and

is carried by lymphatics and blood-vessels to every part of the body, and affects the nutrition of each part with which it comes in contact, provided the textures are in a condition to propagate the cancer-cells."

Part IV is on the results of errors of chemistry in the development and repair of the body, a vast subject which the author does not profess to do more than touch upon, and which he does not consider to be ripe for systematic treatment. As in other parts of the work, however, he selects a few examples to illustrate his views, or to show the nature and importance of the subject. Thus he briefly discusses, as instances of subtrophic and pertrophic action, potency of the foramen ovale and imperforate hymen; and, as examples of what may be called nutritive therapeutics, the use of iron in anæmia and of phosphate of lime in rickets.

The fifth part is still more briefly dismissed; it relates to mechanical diseases and the secondary chemical diseases they produce, and contains a good account of the results of a typical "medical accident," rupture of the aortic valve.

Following the author, we have left too little space for the last part or appendix, which treats of the therapeutic actions of mechanical and chemical forces. It is not satisfactory to attempt to give an outline of an outline, even when the latter, as in this case, has as much of character and suggestiveness as such an outline can possibly have. The first principle is, that the law of the conservation of energy entirely does away with every supposition that food or medicine can create or annihilate any force. Then follows a division of therapeutic agents in accordance with the pathological canons laid down in the body of the work. Thus we have A, direct promoters, and B, indirect promoters (through the vascular system) of oxidation; and C, direct retarders, and D, indirect retarders of the same; then, in corresponding order, E, direct, and F, indirect promoters of nutrition; and G, direct, and H, indirect retarders of the same. A includes oxygen, iron, alkalies, iodine and mercury. Of B it would be premature to positively announce an example. Under C is included "that most important list of remedies which formerly constituted the medicinal part of the antiphlogistic treatment," *e. g.* vegetable salines, dilute acids, and preparations of lead (which the author calls the antithesis of iron). We do not exactly see the coherence of the author's views as to vegetable salines, as he allows that when oxidized to carbonates they may add to the alkalescence of the blood, and thus actually promote oxidation. Lead is supposed to be a hinderer of change from its properties as a precipitant of many organic substances. We are disposed to think the localization of the

action of lead in particular muscles, its attraction to the gums, &c., antagonistic in some degree to Dr. Jones's views on diffusion. It has been suggested by another reviewer that these views may be correct with reference to substances that nominally do not form part of the organism, such as lithia, but not with respect to others; but this qualification would not go far enough to account for the peculiarities of the action of lead. We cannot see why lead should not diffuse equally into the flexor and extensor muscles. Again, under division D, the tolerance of opium established in confirmed opium-eaters is conclusive to our mind aganst a purely chemical view, such as the author would adopt, of its action on the nerves. Opium is regarded by him as the beneficial agent in the old-fashioned and hardly yet defunct calomel and opium treatment. This section winds up with one of his clear and concise definitions—"Antiphlogistic treatment," says he, "is the retardation of chemical action."

Under E he includes iron, bone-earth, and cod-liver oil; but denies to the phosphates any special power of aiding in the formation of phosphorized nerve-substance, on the ground that the phosphoric acid will never lose its oxygen by reduction. He makes no mention of the hypophosphites. G includes a long list of substances, such as lead, and probably several other metals, carbonic acid, and organic acids in general; and under H come strychnia, veratria, morphia, digitaline, &c., whereunto Dr. Jones adds the salts of potassium.

Finally, before turning away from the contemplation of the leading principles of his science of therapeutics, he thus asseverates his belief in them and in their future prevalence:

"Instead of being, as formerly, blind wielders of heavy clubs, that may cure the disease or kill the patient, or instead of being, as at present, 'judicious' or injudicious 'bottle-holders,' physicians at some future time will estimate exactly the effect of the increased or diminished action of any one force upon all the other forces concerned in the production of general or local disease; and by adding to the resistance of one or more forces, or by liberating more energy by means of the powers that are latent in food and medicine, they will restore that equilibrium of action in the body upon which our health depends."

Review VIII.

1. *On Diseases of the Stomach, the Varieties of Dyspepsia, their Diagnosis and Treatment.* By S. O. Habershon, M.D. Lond., F.R.C.P., &c. London. 1866. Pp. 258.
2. *On the Diagnosis and Treatment of the Varieties of Dyspepsia, considered in relation to the Pathological Origin of the different forms of Indigestion.* By Wilson Fox, M.D. Lond., F.R.C.P., &c. London. 1867. Pp. 243.
3. *The Indigestions or Diseases of the Digestive Organs functionally treated.* By Thomas King Chambers, Honorary Physician to H.R.H. the Prince of Wales, &c. Second edition. London. 1867. Pp. 337.
4. *A Treatise on the Functions of Digestion; its Disorders, and their Treatment.* By F. W. Pavy, M.D., F.R.S., &c. London. 1867. Pp. 217.

Of the authors, the titles of whose works are given above, two, Dr. Chambers and Dr. Habershon, are already favourably known by their writings on the disease in question. Notices of their former contributions to the obscure class of morbid phenomena commonly arranged under the head of Dyspepsia are to be found in the volumes of our Review for 1856 and 1863. Nor is the topic one recently taken up by Dr. Fox, who, even when a student in Berlin, made the morbid conditions of the stomach a subject of research, the results of which he communicated to the Medico-Chirurgical Society of London in 1858. Lastly, of Dr. Pavy, we need hardly inform our readers that he, too, early in his career, made digestion a special subject of research, and distinguished himself as an original inquirer.

Similar as the works are in their general intent, each has a distinctive character of its own. Of this some idea may be formed from the designations of the chapters into which they are divided. Thus, Dr. Habershon first treats of the "changes of digestion at different periods and conditions of life;" secondly, of "the general sympathy of the stomach in disease;" thirdly, of the "symptoms of the disease of the stomach;" fourthly and fifthly, of the general treatment, and of the remedies for indigestion, and their abuse; next, of dyspepsia of various kinds, as the atonic, congestive, inflammatory, hepatic, rheumatic, and gouty, renal, mechanical, sympathetic, fermentative, duodenal,—these comprised in ten chapters, followed by two concluding ones on ulceration of the stomach and cancerous disease of the organ.

Dr. Fox's arrangement of his subject-matter is comprised under fewer heads. Commencing with an introductory chapter on the nosological classification of dyspepsia; he next treats of "the general symptomatology of the stomach;" thirdly, of the general symptoms and causes of dyspepsia; fourthly, of the disease in its atonic form; fifthly, of the neuroses of the organ; sixthly, of acute gastric catarrh in inflammatory dyspepsia; seventhly, of chronic inflammatory dyspepsia or chronic catarrh of the stomach; lastly, of diagnosis.

The titles of the chapters into which Dr. Chambers's work is divided are altogether different from the preceding. Thus, after three chapters of a discursive kind, comprised in an explanatory introduction; a chapter on the "indigestion of various foods;" the third, on the "habits of social life leading to indigestion;" in the seven which follow he discourses on abdominal pains, vomiting, flatulence, diarrhœa, constipation. and costiveness, nerve disorders connected with indigestion, concluding with a list of cases indexed according to the subjects they profess to illustrate.

Dr. Pavy's arrangement, according to the title of his work, is partly physiological, partly pathological. The following are the headings of the several sections:—"General remarks on digestion and indigestion—Prehension and ingestion—Mastication—Insalivation—Deglutition—Difficulty of swallowing or dysphagia—Gastric digestion—Vomiting—Eructation—Rumination—Perverted appetite—Pain and other morbid sensations connected with the stomach—Flatulence—Heartburn—Waterbrash—Acidity—Intestinal digestion—Colic—Enteralgia—Intestinal flatulence—Tympanites—Diarrhœa—Constipation—On artificial digestion as a means of dissolving meat for producing an article of nourishment for the invalid.[1]

To do justice to these works, an ample analysis of each would be required, with such comments as parts of them might need, a task which would require more space than we have at our disposal.

All four are highly creditable to their authors, and we have no doubt will be well received by different members of our profession. Dr. Habershon's and Dr. Chambers's will probably be much read, especially that of the latter, by the advanced practitioners; whilst Dr. Fox's will prove of most service to the senior students. The same remark applies to Dr. Pavy's, with this addition, that a special interest is imparted to it from its

[1] This article Dr. Pavy has had made by the action of a digesting fluid—pepsine and hydrochloric acid and water in due proportions. He considers it greatly superior in its nutritive qualities to the extract of meat, prepared by Liebig's process. We hope to hear more of it and of its success.

containing the results of his own researches, especially of that explanatory of the remarkable phenomenon first noticed by John Hunter—the digestion and destruction of the stomach by its secreted fluid—of which a brief account is to be found in the October number of our Review for 1864.

They may be viewed in some measure as complimentary to each other; the one supplying more in detail the information which the others either omit, or pass over briefly. Were we requested to recommend a selection of works on dyspepsia, taking the word in its most comprehensive sense, we do not know that we could do better than name those which are now before us; Dr. Fox's as affording a pretty comprehensive sketch of the different views which have been taken from time to time by the highest authorities in medicine relative to the nature of dyspepsia, and which are taken at present of the many problems on doctrinal questions involved in this most obscure and intricate subject; Dr. Habershon's, as affording a summary of what is best known at the present time of the diseases of the stomach and their treatment, whether functional or organic, whether depending on lesion of the organ itself, or on sympathy through the medium of nerves with other diseased organs; Dr. Chambers's for the clinical teaching it affords by means of a collection of well-selected cases excellently described, and accompanied by a sagacious running commentary; lastly, Dr. Pavy's, for the clear and felicitous manner in which, in connection with the treatment of the disease, he has described the structure and functions of the several organs concerned and their secreted fluids.[1]

Each of the authors is an hospital physician, each a teacher in one of the respected medical schools of our metropolis; we have a right to expect from them the most approved and recent information on any disease on which they may undertake to enlighten their brethren. In this instance, we think, we may honestly say, each has performed his part well and will cause no disappointment.

During our perusal of the volumes we marked many passages

[1] The author speaking of pepsine makes the important remark, that what is commonly sold as such is inert. Of good pepsine, he states, ten grains to the ounce of water with its requisite portion of acid (twenty drops of strong hydrochloric) forms a strong digestive solution.

He describes how an artificial digestive liquid may be obtained: the pulp of a fresh stomach—that of the pig—may be scraped off the mucous surface, or the mucous membrane itself cut off in pieces may be infused in cold or tepid water for from twelve to twenty-four hours. The strained infusion is then to be treated with the acid. He points out that the dried mucous membrane answers as well for making the infusion—dried at a moderate degree of warmth, and kept well exposed to the air, or ground to powder.

for comment, but we abstain for the reason already assigned, and hardly knowing where we should stop were we to begin.

Will Dr. Chambers pardon us if we advert to one expression in his excellent work which has not pleased us; it is that relative to Sir Astley Cooper, as having "wasted his life in routine work," and found, when attempting retirement, "it was too late to educate the mind to anything else." Now, surely this distinguished surgeon and original inquirer does not deserve to be so spoken of. Even up to the very end of life, he was zealously occupied in research. We remember well the last time —a year or two before his decease—that we had the pleasure of breakfasting with him, how earnest he was about what then engaged his attention, and the many preparations he brought from an adjoining room (where he had an artist employed) to illustrate his remarks. In the eleventh volume of our Review an obituary notice is to be found of this ornament of our profession, which we would recommend to the perusal of any of our readers who, like Dr. Chambers, do not appreciate Sir Astley Cooper's claims to our gratitude; or, failing that volume, the obituary notice of him in the fourth volume of the 'Abstracts of the Philosophical Transactions.'

Review IX.

On Wakefulness. With an Introductory Chapter on the Physiology of Sleep. By William A. Hammond, M.D., late Surgeon-General of the United States Army, &c. Philadelphia. 1866. Pp. 93.

In this little volume, as it is designated by its author, we have a happy example of theory and practice combined; and viewed in this light, we consider it not the least valuable of Dr. Hammond's contributions to medical science.

The contents of the work are distributed in four chapters, each of which we shall briefly notice.

The first, on the "physiology of sleep," he prefaces with some interesting remarks on functional action and waste, on assimilation and reparation with rest, no organ being an exception, more especially the brain, with this qualification, however, that its rest is not absolute, only comparative. After passing in rapid review the various hypotheses which have been advanced to account for sleep, the conclusion he arrives at is that it does not depend on increased but on diminished pressure on

the brain; not on a greater supply or flow of blood through the organ, but on the contrary; and in enunciating this, he very properly lays emphasis on the distinction that should be made between sleep and stupor—the latter the effect of abnormal pressure, or of access to the organ of venous in place of well aerated arterial blood.

In taking this view of the cause of sleep, our readers must be aware that Dr. Hammond has been anticipated by a physiologist of our own country, Mr. Durham, of the results of whose researches a short notice is to be found in the April number of our Review for 1861, page 332. To him our author gives full credit, remarking when commenting on the labours of others, as follows:—"But the most philosophical and most carefully digested memoir on the proximate cause of sleep which has yet been published is that of Mr. Durham," adding, "Although my own experiments in the same direction, and which will be hereafter detailed, were of prior date, I cheerfully yield all honour which may attach to the determination of the question under consideration to this gentleman, who has not only worked it out independently, but has anticipated me several years in the publication, besides carrying his researches to a much further point than my own extended."

On Dr. Hammond's observations and experiments it may suffice to remark, that they are very satisfactory and further elucidate the cause of sleep: they may be referred to with profit. They warrant him, we think, in enunciating as a law, that whatever is capable of lessening the quantity of blood in the brain, is also capable of inducing sleep, and this without exception; and that the use of sleep is the comparative rest of the *sensorium commune*, and the recovery by rest and assimilation of what the organ has lost in the performance of its great and vital function. Amongst the conducing causes he points out the following:—

1st. Heat, as determining the blood to the cutaneous surface and the extremities, with a dilatation of their vessels.

2nd. Cold, that moderate degree which in healthy and vigorous persons, he assumes, has a similar effect, as indicated by "the ruddy complexion and warmth of the hands and feet," in contradistinction to intense cold, which, as is well known, produces stupor and is fatal to life.

3rd. Diminution of power of attention,—that too so well known,—however produced.

4th. Excessive loss of blood by lessening the quantity of blood in the brain and enfeebling the propelling force of the heart.

5th. Debility, acting in a somewhat similar manner.

We are of opinion that a sixth might be added, viz. gentle carriage exercise. We have commonly found it to have the effect of rendering the pulse slower; and it is known by many to have a soothing influence conducive to sleep.

The second chapter is on the "pathology of wakefulness." It is his main subject.

In every instance of primary insomnia he holds that it depends on an increase of the quantity of blood circulating in the brain, either absolute or relative; the former in cases of vigorous general health, the latter in cases in which the system has been reduced below par, with temporary increase of activity in the cerebral circulation. He describes some striking examples of each kind. Of the first, he gives a case in which sleep was restored by means of a large dose of bromide of potassium (thirty grains); a medicine which Dr. Hammond has found by experiments on dogs and other animals to have a decided effect in diminishing the flow of blood through the brain. The patient was a robust man, a broker, intent on and over-excited by speculation in the money market. Of the second kind, was a lady, reduced to a deplorable state of weakness by loss of blood, associated with many mental anxieties. In this instance a stimulating mode of treatment was employed and with the desired effect, and this speedily by the use of whiskey at bedtime after a warm bath.

In the third chapter "the causes of wakefulness" are discussed. In accordance with the assigned pathology, Dr. Hammond maintains that whatever is capable of increasing the ordinary amount of blood circulating through the brain may occasion wakefulness. The following he points out as most deserving of notice, especially as they are more or less under the control of the individual.

1st. Long continued or excessive intellectual action, or any powerful emotion of the mind; the former, besides being productive of insomnia, being often attended with mental hallucinations.

2nd. Positions of the body, such as tend to impede the flow of blood from the brain, and yet not obstruct its passage through the arteries, productive of hyperæmia, and thereby of a state of erithism and insomnia.

3rd. An increased flow of blood to the brain from certain articles taken of food or medicine, such as coffee, tea, alcohol, opium, &c.; the latter, if not given in large stupifying doses, and not even then, according to Dr. Hammond, provided the blood circulating through the brain be duly oxygenated by respiration.

4th. Functional derangements of certain organs, productive of an increased amount of blood in the brain, such as hyperæsthesia

of the skin, or of any other organ of sense, disordered menstruation, irregular or deficient action of the heart, with coldness of the extremities, indigestion, &c.

By way of illustration, and for the purpose of comment, the author relates many interesting cases, most of which came under his immediate observation. Owing to our limited space we can only thus refer to them. Those accompanied by hallucinations are most striking and especially deserving of attention.

The fourth chapter is on "the treatment of wakefulness." The therapeutical measures which he proposes are of two kinds; first those which have a soothing influence on the nervous system, or distract the attention, diminish the action of the heart and bloodvessels, or correct irregularities in their function, lessening thereby the amount of blood in the brain. Second, those which produce a similar effect, either directly, mechanically, or specially upon the circulating organs.

Belonging to the first class, he enumerates monotonous sounds, gentle undulating movements, gentle friction of the surface of the body, &c., means which, in slight cases, are often effectual, but in cases of severity are generally nugatory.

Belonging to the second class, he comprises as of most import, hygienic measures tending to improve the general health of the afflicted. Amongst these, he first considers food, his remarks on which are very judicious. He lays it down as a rule, that people are underfed, especially women, who use "slops" to the neglect of solid nutritious viands, thereby lowering the tone of the system and promoting local congestions, and if in the brain, a resulting wakefulness. For such cases, *i.e.* limited to the asthenic form of insomnia, stimulants in moderation are recommended with nutritious food. He has found, in many cases of this kind, coffee, a strong cup, taken three or four nights in succession, beneficial. Regular "physical exercise" he estimates so highly, that he thinks no lasting good can be expected without its aid, and, as a requisite, taken in the open air, to the extent of producing a slight feeling of fatigue. According to our experience, we may add, that walking at a slow measured pace has a lowering effect on the pulse like that which we have experienced from gentle carriage motion. The warm bath, by determining the blood from the head and calming nervous irritability, he esteems as a valuable means of procuring sleep. The application of cold water to the head, he, of course, restricts to sthenic cases of insomnia. He states that he has often used it as cold as ice could make it. In a tropical climate, we may here incidentally mention, the soothing influence we have experienced from sponging the head at night, or dipping it in water and laying it with the hair wet on the

pillow on going to rest: it conduced then to sleep as much, we think, as the prevention of coldness of feet in bed does in a cold or temperate climate. The effects of position in its efficiency in severe cases which have come under his charge, he thinks, afford "additional confirmation of the correctness of the theory that the condition of the brain in such cases is one of hyperæmia."

Of medicinal agents, the one he most confides in is the bromide of potassium, on account of its effects, as already mentioned. Hyoscyamus, as an hypnotic, he prefers to opium. When either is used, he properly insists that care should be taken to give them in sufficiently large but not in excessive quantities, for reasons pretty generally understood by medical practitioners. Tonics, he is of opinion, are commonly indicated, especially quinine and iron. Spasmodics, such as valerian, assafœtida, &c., he does not commend.

When insomnia is owing to functional derangement of distant organs, measures of relief, he points out, must be primarily directed to them, to afford lasting relief of the ailment. When long-continued and severe mental exertion has been to blame, it need hardly be remarked, that nothing can effectually avail until the individual consents to use his brain in a rational manner. Relaxation at proper intervals is essential, which may be aided much in the restoration of healthy sleep by travel, when practicable, and change of scene.

The importance of the subject treated of, especially at the present time, when the brain is so often over-worked by those inordinately intent on money speculations, to which there are so many temptations, has led us to be more particular in our notice of the work than its length, not its merits, might seem to justify. It would be fortunate for mankind were its contents widely known, and the warnings in it taken to heart, and the advice given by its accomplished author followed.

REVIEW X.

Intestinal Obstruction. By WILLIAM BRINTON, M.D., F.R.S. Edited by THOMAS BUZZARD, M.D. Lond. London, 1867. Pp. 136.

THIS work is distinguished in a remarkable manner for the careful, laborious, and original research on which it is based.

Before Dr. Brinton engaged in the inquiry denoted by the title of the volume, it was a received doctrine that the intestines are capable of an inverted or antiperistaltic action. We are informed by the editor that as early as the fourth year of his student life, he had begun the study of the process by which fæcal matter is vomited, and that following it up he contributed a paper to the Royal Society, in 1848,[1] in which among other results he adduced evidence in proof of the vomiting in question being owing not to anti-peristalsis—an hypothesis, he set aside entirely—but to the existence of two currents, a peripheral of advance and an axial of return, from equal lateral pressure downwards and diminished resistance in the contrary direction.

This as a principle he further explained in the Croonian Lectures which he delivered before the Royal College of Physicians in 1859; and it constitutes an important element in the work now before us, which is a reproduction of those lectures, as we learn from the preface of the lamented author, for the most part re-written and amplified.

In the first lecture Dr. Brinton has, we think, been eminently successful in enunciating the laws which govern the process of intestinal obstruction generally. After pointing out how the hindrance of the passage of the intestinal contents may depend on two, and in some sense contradictory causes, viz. a failure of propulsive energy on the one hand, or an excess of resistance to such energy on the other, he enters on the pathology of the subject with the statement of a typical case characterised by fæcal vomiting. This most important symptom he explains on the principle already announced, illustrating his explanation by diagrams. The other symptoms, according to the several stages, he discusses *seriatim* and in a very lucid manner.

In the second lecture, the author considers the chief varieties of intestinal obstruction from a strictly clinical aspect, so as to distinguish the different members of the group from each other, exclusive of "ruptures," these pertaining to surgery. The

[1] Its title was "Contributions to the Physiology of the Alimentary Canal." It was not published in the 'Transactions of the Society;' but a very full account of it was given in the 5th volume of 'Abstracts.'

results of his researches, founded on an astonishing number of necropsies are collected in a table, which we give, but for want of space without any of the remarks and valuable observations which precede it.

In the third lecture the author enters upon the important subject, the treatment of intestinal obstruction, prefacing it by some interesting remarks, which we are tempted to quote, both as a good example of his terse style and logical manner, and as affording a clear *précis* of what had been before advanced.

"In the two preceding lectures" he says "we have successively considered (1) the chain of phenomena common to all intestinal obstructions; and (2) the symptoms characteristic of its chief varieties. In the first we found that any mechanical obstruction of the bowel causes an accumulation of its contents above the obstructed part; that this accumulation provokes such a peristalsis as ensures their mixture, and, strictly speaking, their reflux; and that increasing distension finally brings about paralysis and inflammation of the intestine, ending in the collapse and death of the patient. . . In the second we estimated the relative frequency of the several forms of obstruction usually met, and pointed out that their symptoms (referable chiefly to the nature and situation of the obstacle), generally furnish an accurate diagnosis, even in the earliest stages of any given case." He adds: "It is in the substantial accuracy of these two conclusions that all I have to say respecting the treatment of intestinal obstruction essentially depends. Not only would it be difficult to mention any group of maladies which better illustrates the unity of the science and the art of medicine; but, I would add, as the key to the following remarks, that specific insight, and accurate and early diagnosis, have a scientific if not unusual value in respect to both branches of treatment. If the process of mechanical obstruction cannot be distinguished from that of enteritis, the physician may search in vain for principles to guide his administration of food or remedies. And if one form of obstruction cannot be distinguished from another, the surgeon can scarcely venture to operate with any reasonable chance of success."

The following is a summary of the treatment which the author considers most advisable in the different forms of obstruction.

"In intus-susception of the large intestine, repeated injections of liquid into the rectum, so as to distend the bowel to its utmost dimensions. In stricture of the large intestine, the institution of an artificial anus above the obstacle. In obstruction from bands, diverticula, &c., mostly affecting the small intestine, gastrotomy, and division of the chord-like cause of strangulation, a procedure which, if interrupted by unforeseen impediments, may further require the institution of an artificial anus in the most distended part.

Intestinal Obstructions.

Frequency—1 in 280 deaths from 12,000 promiscuous necropsies).
Varieties—Relative frequency per cent. (from 600 necropsies of obstruction.

Intus-susceptions.	External (bands, &c.).	Parietal (strictures, &c.).	Torsions.	
43	32	17	8	= 100

Intus-susception, varieties of, per cent.	Ileo-cæcal.	Iliac	Jejunal.	Colic.	
	56	28	4	12	= 100

Remaining Forms of Intestinal Obstruction.

Part of the intestine obstructed.	Lesion.	Ratio of Sexes. Male.	Ratio of Sexes. Female.	Average age.	Bowel affected per cent. Small.	Bowel affected per cent. Large.	Average duration in days.	Per centage of each group.	Per centage in the two groups.
Chiefly (95 per cent.) of small intestine	Bands, adhesions	13	15	36	94	6	6	33	60
								9	
	Diverticula ilei	5	2	22	95 ?	5 ?	6	18	
	Vermiform appendix	1	1	22	91	9	6	9	
	Ruptured mesentery	2	1	35	100	...	5	16	
	Other peritoneal lesions	...	...	...	...	...	...	(2½ omentum.)	
	Gall-stones	1	1	57	100	...	5 ?	9	
Chiefly (88 per cent.) of large intestine	Strictures	3	2	44	8	92	23	68	40
	Tensions	13	10	54	24	76	9½	32	

"In obstruction by stricture, however, a tobacco enema should be administered at least once; a measure which should be repeated if need be in obstruction by bands, and especially by gall-stones. In all cases, opium and supports to be freely administered from the earliest stages of the malady. The bulkier liquid constituents of the food to be given as sparingly as possible by the mouth, but administered freely per anum. Distensive enemata to precede all operations, if only as a means of aiding or assuring diagnosis. Where vomiting is excessive, nourishment to be also injected into the rectum in small and frequent doses.

"After recovery, all food which can introduce indigestible substances into the intestine should be carefully avoided; the bowel having sometimes undergone changes of calibre and arrangement, such as permit substances easily transmissible through the healthy canal to cause fatal obstruction."

Our readers will perceive how different this system of treatment is from that too often followed before, when attempts were made to overcome the obstruction by purgatives, mercury, bougies, &c. His comments on these unscientific means are of a very forcible and convincing kind, such as must carry conviction, we think, to every reflecting mind; and we cannot commend too much the various cautions he offers. Adverting to the superiority of the mode of treatment which he advocates, he rises into a strain of eloquence it is delightful to read, and in reading to sympathise with. We trust no apology is needed for quoting it.

"And though any statistical comparison of the two plans at present quite fails us, I venture to anticipate, from personal observation, that such a comparison would afford striking proof of the superiority of the treatment I have advocated. I say more, I hazard the prediction that it will hereafter be proved so—not because I have long advocated it—not because its every detail has been matter of careful consideration, gradually ripening into strong conviction—not even because the drift of professional opinion has evidently for some years past shown signs of setting into this channel—but rather because I believe it to rest on a scientific basis, upon physiological and pathological foundations both wide and deep; because it seems to me to offer an illustration of the immortal law '*Natura enim non nisi parendo vincitur*,' and to show that in a disease often incurable, always dangerous, Providence has still confided to us the lives of our fellow-creatures: teaching us how, by studying his own body, man may often remedy one of its stormiest diseases, just as by studying inanimate nature, he may, with means no less simple and apparently inadequate, avoid the whirlwind, guide the avalanche, put back the glacier, attract the fertilising rain, and control the devastating flood. Comparing the duties we have to perform with some of these tasks and with others which, though strictly medical, the individual cannot

undertake, and the social organisation called government is only beginning to count amongst its functions. The directness and readiness of the means we wield in this, one of the most agonizing and deadly of all diseases, may well mingle gratitude with those feelings of reverence and responsibility which animate the incessant toil, and the scarce less incessant distress for others of the physician's life."

The only addition made to the work by the editor is a detail of three well-marked cases of intestinal obstruction, treated according to Dr. Brinton's plan, in which opium was largely used, and all of which happily recovered.

With the very high sense we entertain of the merits of Dr. Brinton's work, we cannot recommend it too strongly to the attention of our readers. Had he written no other, we think he would have established a claim to the investing of his name amongst the benefactors of mankind and the improvers of medical science, &c.

REVIEW XI.

On Diseases of the Lungs and Air-passages; their pathology, physical diagnosis, symptoms, and treatment. By HENRY WILLIAM FULLER, M.D. Cantab. Second Edition. 1867. Pp. 534.

THE notice prefixed to the present edition of Dr. Fuller's work states that the former was exhausted within two years from the date of its publication; a fact which furnishes, we think, sufficient proof that a want existed for such a book, and that the want has been well met by the author. Towards the end of our review of the first edition we hinted that a division of the work into two or more volumes might accord with the interests both of readers and booksellers. Our suggestion has been accepted, and in the present volume we find Parts III and IV of the former, comprising diseases of the heart and great vessels, omitted and reserved for separate publication, whilst Parts I and II on 'Diseases of the Lungs and Air Passages,' revised and amplified, present a bulk not much inferior to that of its predecessor. Our former review and the general recognition of the merits of the book exemplified by its rapid sale preclude the necessity for a lengthened notice of this edition. We, however, take the opportunity of making our readers acquainted with Dr. Fuller's views on one or two points of considerable interest, but of no less difficulty.

That deviations from the natural standard in the *quality* of the respiratory sounds in the direction of hollowness and intensity, together with increased vocal resonance, are sufficiently accounted for by changes in the density of the pulmonary tissue, as taught by Laennec, was for a long time the only and still is the prevailing doctrine. Skoda, however, ignoring the effect of consolidation in rendering more audible sounds produced in the larger air-passages, has endeavoured to explain the phenomena of bronchial breathing and broncophony in diseased conditions by the theory of consonance. One of the chief facts tending to support this or some analogous theory is that of the occasional intensification observed in the voice sounds over vomicæ, dilated bronchi, &c. It is evident that no alteration in the density of the pulmonary tissue could absolutely augment the voice sound, rendering it when heard "through the stethoscope applied to the chest-walls as loud if not louder than the voice heard in the same manner over the larynx or trachea." Such a fact, although incapable of shaking the validity of Laennec's original statement, that sounds are conducted better by condensed or infiltrated lung tissue, proves at least that this law will not account for all the variations in the quality and pitch of the vocal and respiratory sounds heard through the stethoscope at the bedside. Dr. Fuller's remarks on this subject seem worthy of the consideration of those interested in the theory of auscultation. Dr. Fuller shows that the increased sound-conducting power of condensed or consolidated lung is not as Laennec believed simply in relation to the change from a non-homogeneous to a homogeneous substance. He believes that he has proved by direct experiment that the nature of the consolidating matter plays an important part in altering or raising the conducting power. It is not sufficient that the effused material should render the lung-tissue homogeneous merely, it must further be of a vibratile or elastic character. He says:

"In speaking of consolidation and consequent increased homogeneity as a cause of increased vocal resonance, I would confine the observation to those cases in which the effused matter is of an elastic vibratile nature; for the presence of non-elastic material in the lung may actually diminish or altogether abrogate its voice-conducting power. This may be tried experimentally by injecting one lung with size or some other elastic material, and another with tallow or other non-elastic matter, and then listening through a stethoscope applied to one part of the lung, whilst a second person speaks through a stethoscope applied to another part of the same lung. The differences observable between the voice-conducting power of the lung, according as it is solidified by one material or another, will then be strikingly

apparent. I have repeated this experiment with a variety of materials; and I entertain no doubt that the varying sound-conducting power of different lungs, apparently equally condensed, is attributable to the varying elasticity of the matter to which their consolidation is due."—Note, p. 109.

Skoda has gone so far as to assert that healthy lung tissue will conduct sound further than hepatized, and has, therefore, rejected Laennec's theory *in toto* for that of consonance. Dr. Fuller denies the validity of Skoda's experiments, and rejects, in the great majority of instances, the theory of consonance, but he believes that other causes besides the simply raised conducting power of infiltrated or condensed tissue must be accepted as producing the phenomena of intensified thoracic vocal and respiratory sounds. In ordinary cases, in connection with consolidation, he believes these phenomena are referable to three causes, viz. non-diffusion of the vocal vibrations consequent on the obstructed condition of the air-cells and the terminal bronchi; increased reflecting power of the bronchi; and, lastly, to augmented conducting power of the lung. In those instances, however, in which the vocal resonance heard over vomicæ or dilated bronchi is intensified or altered in pitch, Dr. Fuller, following Dr. Walshe, accepts consonance, unison resonance, and echo, as possible sources of the altered sound. At the same time, he thinks that of the three the first is the most rare. To echo he is inclined to refer the intensification of the voice in certain cases of consolidation, to unison resonance that observed in emphysema, and he thinks both combine to produce the effect in certain cases of vomicæ. The laws which regulate the production of consonance are not such as to admit its frequent occurrence in the lungs. Holding this opinion, Dr. Fuller questions the value of Skoda's distinct class of "consonating râles," and advises the student to dismiss the term from his vocabulary. Such râles have he believes at the most a problematical existence; they are undistinguishable from other râles, and possess little diagnostic value.

With regard to sounds described by authors as emanating from the lung substance itself—such, for instance, as the fine crepitation accompanying inspiration, heard sometimes at the base of the lungs posteriorly in healthy persons who have been long reclining on their back, and the pulmonary crumpling described by M. Fournet—Dr. Fuller is sceptical. The former he believes to be true crepitation due to the presence of secretion in the air-vesicles; the latter, he thinks, is a form of pleuritic friction. He, however, believes there is one rare and peculiar variety of crepitation produced by the presence of serosity in the areolar pulmonary tissue. He distinguishes it from ordinary crepitation by the circumstances that the crepi-

tations are less numerous, less regularly and rapidly evolved, and that they are heard just as strikingly during expiration as inspiration. Whilst the period of its occurrence and its duration correspond with the fine bubbling sound of capillary bronchitis, the minuteness of the crepitations, the peculiar dryness of their character and the absence of expectoration distinguish them from any sound produced by the presence of fluid in the air-passages. A somewhat similar sound is heard over the sternum when the mediastinal cellular tissue is infiltrated with air or serosity. Dr. Fuller has heard this sound when the tissue named has become implicated in the course of pericarditis.

The peculiar sound heard by Laennec in emphysema, which he describes as "*râle crépitant sec à grosses bulles ou craquement,*" is believed by the author to be "a sound of vibration connected with the extremely viscid and tenacious bronchial secretion which ordinarily accompanies vesicular emphysema. He so far discredits it as to deny it a place in the list of peculiar râles and rhonchi. In referring it to the presence of bronchial secretion, we think that Dr. Fuller coincides with one of the most recent writers on emphysema, Dr. Waters, of Liverpool, who believes that the râle described by Laennec is not dry, but is produced by the presence of a certain amount of fluid in the finer air-passages, and is but little distinguishable from an ordinary subcrepitant râle.

The chapter on succussion, amphoric resonance, and metallic tinkling contains several passages of interest. The question whether succussion sound can be elicited under any circumstances of thoracic disease except that of the coexistence of air and fluid in the pleura is set at rest by the author. He states that he has met with succussion sound—produced by shaking the chest—in three instances at St. George's Hospital, where post-mortem examination revealed an enormous tubercular cavity, but no pleuritic effusion. Such instances are of course uncommon, and they would be easily distinguished from those in which splash is produced in the pleural cavity by the limitation of the area over which the sound is heard, and, according to the author, by the fact that in the case of tuberculous cavity "the resonance rarely acquires a metallic quality at all equal in intensity to that which usually accompanies pleural succussion sound." On the question as to whether for the production of amphoric echo in a cavity it be essential that a fistulous opening into a bronchus exist, Dr. Fuller agrees with Skoda, Barth, Rogers and others, that such communication is not necessary. He believes "that if the air in the cavity be separated from moderate-sized bronchus merely by a thin layer of pulmonar

tissue, the sounds existing in that bronchus may pass into the air of the cavity with force sufficient to excite sonorous vibrations therein accompanied by amphoric resonance." In like manner, he agrees with Skoda that the existence of a fistulous opening into a bronchus is not essential to the production of metallic tinkling. Dr. Fuller believes that this phenomenon is "a mere reverberation or echo of certain sounds reflected by the tense and indurated walls of a large hollow space within the chest;" that it is dependent for its peculiar character on the nature and tension of the walls, and on the original sonorous vibrations. In his experience the occurrence of metallic tinkling is exceedingly rare. He asserts that amongst thousands of pulmonary cavities he has examined, he has only met with it in a single instance, and in that case the cavity was larger than a full-sized orange, adherent anteriorly to the parietes of the chest, bounded by smooth tense walls, and contained a remarkably thin homogeneous pus. Skoda denies also that fluid is necessary to the production of this phenomenon. Dr. Fuller, however, maintains that the presence of fluid thrown into active vibration is essential to the production of true metallic tinkling, but that fluid is not necessary if that term be applied, as he believes Skoda applies it, to high-pitched amphoric echo.

There are numerous other observations on auscultatory phenomena scattered through the first part of the book by which we think Dr. Fuller has proved himself a careful and independent thinker and observer. We are not always convinced by him, but we welcome a writer who forms an independent opinion on his own experiments and bedside observation. We see that in the second part he retains those, in the present day, courageous views on the subject of the treatment of pulmonary hæmorrhage, to which we previously adverted. Small and repeated bloodlettings of from six to ten ounces are considered permissible when local congestion is excessive. Dr. Fuller, however, adds—and we certainly agree with him—that in most cases dry cupping will supersede the necessity for venesection.

In the chapter on pulmonary consumption we notice that the author is disposed to accept M. Villemin's experiments on the inoculation of tubercle; but, like most other English physicians, he is an opponent of the doctrine of the possibility of phthisis being communicated by contagion or infection, and he is sceptical as to the beneficial effect of pregnancy in arresting the disease. But throughout the book we find subjects on which two opinions may be held discussed in a philosophical tone, which will commend itself to the medical reader. We may add that, as a class-book for the student, Dr. Fuller's work is eminently well arranged and comprehensive.

PART SECOND.

Bibliographical Record.

ART. I.—*Acupressure an excellent Method of arresting Surgical Hæmorrhage, and of accelerating the Healing of Wounds.* By WILLIAM PIRRIE, Professor of Surgery in the University of Aberdeen, and WILLIAM KEITH, Senior Surgeon to the Royal Infirmary of Aberdeen. London: 1867. Pp. 190.

THIS book is a seasonable and welcome contribution to the literature of Acupressure. It is even more valuable in some respects than Sir J. Simpson's treatise, since it gives the more matured experience of practical surgeons who have themselves made trial of the method in almost all kinds of cases. Sir J. Simpson's book, besides being somewhat tinged with the natural enthusiasm of an inventor for the darling creation of his brain, was a little unsatisfactory in the cases produced, many of which were of so trivial a nature as to be worthless in the way of testing the method, while nearly all of them were secondhand. Moreover, Sir J. Simpson's book was, we think, of an inordinate length, a fault from which Dr. Pirrie's even is not entirely free. At least we fail to see the object with which a chapter on the healing of wounds is introduced, which is little more than a reprint from Mr. Paget. One thing, at any rate, may be taken to be settled by the present volume, viz. that acupressure is a perfectly reliable and efficient method of arresting hæmorrhage in surgical operations. Thus, numerous cases of amputation of the thigh and one at the shoulder-joint are given in this work. The mere fact of acupressure being a reliable method does not, as Professor Pirrie justly observes, prove that it has any superiority over the ligature, which is also a reliable method. But Professor Pirrie and Dr. Keith do very much prefer it for the reasons thus stated by the former in all cases where immediate or rapid union is desirable.

"First—It is not only the easiest of application, but the quickest method yet devised for arresting bleeding. That the vessels in a

large amputation can be acupressed in a much shorter time than they can be ligatured, I am perfectly satisfied.

"Second—Another advantage resulting from shortening the time necessary for the arrest of hæmorrhage is the diminution of the risk of the occurrence of suppuration and other distressing results of the higher grades of inflammation in the stump. I have often thought we are too apt to forget that living tissues are resentful of even slight injuries, and that we are not sufficiently careful to use the sponge as seldom and as gently as possible. The risk from frequent touching of the parts must be diminished by thus shortening the time required for the suppression of hæmorrhage.

"Third—The use of the ligature is attended with an insuperable obstacle to obtaining perfect examples either of immediate union or union by primary adhesion without the formation of some pus. I have never allowed myself to call any case a perfect example of either of these two methods of healing where a single drop of pus was seen. Neither of these two methods of healing, in this sense, can be perfect in any case where the ligature is used. The immediate effects of the ligature—the changes by which its removal is rendered possible, and its presence acting as a seton in the wound—render more or less suppuration at the points of deligation and in the tracks of the ligatures inevitable. I never saw, in the experience of any surgeon or in my own, where the ligature was used, a perfect example of either of the two desirable methods of healing without any suppuration. I believe such a case never was and never will be seen.

"On the other hand, acupressure, if properly performed, and not too long continued, does not cause any condition which must be followed by suppuration; and I have seen in my own experience and in that of my colleagues many examples of wounds after capital operations where acupressure was used, where healing was effected in some by immediate union, and in others by primary adhesion, without a single drop of pus. Acupressure, no dressings, and perfect repose of the whole of the wound, are invaluable means for the accomplishment of these gratifying results. Acupressure being reliable warrants its use; and these gratifying results alone seem sufficient to justify its preference to the ligature.

"Fourth—For arriving at a just appreciation of acupressure, one of the most important points to be determined is, the effect of its use on the frequency of pyæmia, which is admitted by all to hold a high place in the causes of death after great operations. It is only by the careful observation and record of a long series of cases that this question can be definitely determined. It will, however, be generally admitted that whatever promotes either immediate union or primary adhesion diminishes, and that whatever induces suppuration increases the risk of the occurrence of pyæmia.

"As yet there has not been a single instance of pyæmia in any case where acupressure has been practised by my hospital colleagues or by myself.

"In the forms of acupressure in which the wire is used, if the

wire be drawn with unnecessary and improper tightness, and in these methods, especially if the acupressure be continued for an undue length of time, division and strangulation of arterial coats, and suppuration may be made inevitable. I have heard of cases elsewhere of acupressure having been continued for a considerable number of days. In such circumstances the suppuration and other untoward events ought not in fairness to be regarded as objections to acupressure, but as results of errors in the manner of its performance and the time of its duration. In my experience of acupressure no fact has struck me more than the slight degree of pressure required for the perfect arrest of circulation through an artery, provided the pressure be direct and continuous. It is important to keep this fact impressed upon the mind, that unnecessary and injurious constriction may be avoided. As to how early it is safe to relieve arteries from compression, in these early days of acupressure, I believe it is impossible to give perfectly reliable information. The table of my own cases given at page 134, of Dr. Fiddes's at page 135, and of Dr. Keith's in a future part of this work, will show at a glance what has been the practice of the surgeons of Aberdeen on this point. It is unquestionably safe at the periods stated in the tables, but my strong impression is that future experience will show that the acupressure may be with safety removed at much earlier periods than has as yet been ventured upon.

"Fifth—Acupressure requires a much briefer sojourn of the foreign body in the wound—it is safer and far less irritating in consequence of the obliterating foreign body being of a metallic and not of a textile nature—and in acupressure that division and strangulation of the arterial coats does not take place which is inevitable in deligation.

"Sixth—It is a great comfort to a patient to be assured in a very few hours after an operation that all foreign matter is removed from the wound; and as far as that goes, that all interference and suffering are at an end. This is a comfort a patient enjoys after acupressure, but not after deligation.

"Such are the principal reasons for my regarding acupressure as preferable to deligation in certain circumstances in which both are practicable."

We would beg our surgical colleagues in London to weigh well these dicta of such distinguished surgeons as Messrs. Pirrie and Keith. The method has been extensively tried in London and elsewhere, but the opinions of surgeons still differ much about it. From the little that we have ourselves seen and done in acupressure, we should be inclined to suspect that most, at any rate, of the failures charged against acupressure have really proceeded from want of familiarity with the method, and from consequently using too much pressure or torsion. When surgeons become more familiar with the method, and especially as surgeons come into practice who have not been brought up

in different habits, we confidently expect to see acupressure come more and more into use, and we believe that it will be found one of the most real and most important advances of modern surgery. To no one after its inventor will this advance be more due than to the Aberdeen surgeons.

ART. II.—*Die Lehre von der Mania transitoria für Aerzte und Juristen dargestellt,* von Dr. Med. R. V. KRAFFT-EBING, Arzt an der Gr. Bad. Heil-und Pflegeanstalt Illenau. 1865.

The Study of Ephemeral Mania, arranged for Physicians and Lawyers. By Dr. KRAFFT-EBING, attached to the Asylum at Illenau. 1865.

THIS monograph, on a subject but little studied or written on in this country, is from the pen of a physician belonging to the Asylum at Illenau, in the Duchy of Baden, one of the most celebrated establishments on the Continent, and, also, one of the best conducted. The author has consulted the works of those who have recorded such cases in English and foreign periodicals, and has produced a valuable essay on the subject, having collected widely scattered details into a complete whole, and made some useful generalisations.

The term *mania transitoria* is usually confined to a somewhat rare class of cases in which violent excitement comes on suddenly in otherwise healthy people, and disappears after a few hours, leaving no traces. Such outbreaks are often connected with personal violence, and thus the question of their proper interpretation has a medico-legal bearing, which gives them additional importance. It is chiefly the continental writers who have recorded such instances, but in the 'Psychological Journal' (1863), the subject is well treated, and Dr. L. Lindsay has recently published a case[1] of which we give an abstract illustrating the class:

A cook in a gentleman's family had been unable, owing to some squabbles, to see her mother for many months. On the forenoon of the day of her illness she had had an opportunity of visiting her, having been sent to town, on a message, by her mistress, but, being unsuccessful in her attempt, came home in a state of great hysterical excitement, which gradually increased during the time she was preparing dinner, until she at last became frantic, and, dashing down the dinner, rushed to a window, broke the panes, and began weeping violently, protesting that the dinner was wrongly prepared, and that she was

[1] 'Edin. Med. Journ.,' November, 1865.

looked down upon by her fellow-servants. She remained in a state of the greatest violence from two to three hours, during which time she struggled violently to escape from the house; but after having a strong dose of Liq. Opii Sed., she became calm and rational, and next morning went about her work as well as usual.

It is, of course, of importance to diagnose these cases correctly, as they do not, generally, require asylum treatment, and great social injury may be done to an individual by an over hasty removal.

One of the chief points of distinction in cases of transitory mania is, the suddenness of the attack, the premonitory signs of ordinary mania being absent. This was noticed in fourteen of the eighteen cases adduced by Dr. Ebing, and in the remaining four the symptoms were not very definite.

There is, also, one character common to the delirium in these cases, viz. that it is of a painful and depressing nature. The patients have a fear of approaching death, danger, or calamity, think they are persecuted or insulted, and have illusions of sight or hearing, which are generally the cause of the acts of violence committed; so well marked, indeed, are these characters that the name of "melancholia activa transitoria" is often used as a synonym.

There is, generally, increased heat and vascular action of the head and face, hard and frequent pulse, injection of the conjunctivæ, and quickened respiration. Sometimes there is constipation and gastric disturbance.

As regards the duration of the attack there is some difference, but it may be stated, as from two to six hours, and after a good sleep the patient, as a rule, wakes quite well. The tendency to return is very slight, only three out of the eighteen having had a second attack, while in these a permanent recovery was effected, and this constitutes another distinctive character of this form of mania.

The attacks are not *per se* dangerous to life, and, as regards health, the prognosis is always favorable, but in the excited stage it is very needful to take care that the patient does no injury to himself or others. Removal to an asylum is uncalled for and injurious.

Of the 18 cases given, 15 were men and 3 women, and 12 of the total number were between 20 and 30 years of age. Hereditary taint does not increase the liability at all, and the exciting causes are, most frequently, passion and violent emotions, such as anger, fright, or distress. Transitory mania is sometimes confused with epileptic mania, but the delirium is of a different character in the two, being far more boisterous in

the latter, and of much longer duration, while the history of epilepsy would make it still more clear.

It has, also, some resemblance to a state of ecstasy, but in this condition there are convulsive and hysterical movements, and men are rarely affected.

The excitement following painful dreams—a sort of night-mare—has been thought to resemble this form of mania; but the short duration of the former condition, the retention of memory, and rousing to perfect consciousness, instead of a relapse into sleep, are sufficient to establish the difference.

In a medico-legal point of view it is important to recognise these cases, inasmuch as acts of violence, of a more or less grave character, are often perpetrated in the course of them, and as a direct consequence of the symptoms (the patients sometimes attacking others, under the idea they have insulted or injured them); or, on the other hand, real criminals may shelter themselves under the device of a feigned access of furious mania. Such cases involve difficulties not always easily solved, and have been fruitful of controversy.

In discriminating these points, the author of the essay in question make the following remarks:

"In genuine *mania transitoria* the patient retains his recollection up to the moment of the attack, and then loses it completely; in sham attacks, on the other hand, a man will take care to have a witness, and will deny all knowledge of preceding events, but there he goes too far, for he will not know where his recollections should end, and where they should begin again. He will deny the knowledge of events which occurred sooner or later before the attack, will agree to things which never happened, and pretend to know nothing of events affecting him, will get angry and be unsteady in answering questions, and he would probably make preparations to accomplish the deed or endeavour to efface any traces of it, or lead on another track. He will, in vain, simulate the ignorance which properly belongs to a man who has committed an act of violence in unconsciousness, for his look and demeanour are sure to betray him."

We have said enough to indicate the general course and bearings of this state. The curious inquirer will find more ample information by referring to Dr. Ebing's essay, to Ideler's 'Gerichtliche Psychol,' or Virchow's 'Archiv.,' vol. viii, amongst other works.

ART. III.—*Practical Dissections.* By RICHARD M. HODGES, M.D. *Philadelphia,* 1867. Second Edition. Pp. 286.

THIS little book appears, as far as we can judge (not having had the opportunity of testing it by use in the dissecting room), to be well calculated to fulfil its purpose, that of giving the student the assistance otherwise imparted orally by a demonstrator. It is compressed to the extreme—too much so, in our opinion—and several points in its plan hardly commend themselves to our judgment as testifying to a rather low style in the students to whom the work is addressed. We speak especially of the introduction of a very ridiculous "memoria technica" for assisting Dr. Hodges' readers to remember what surely any man with a head on his shoulders, and the most ordinary industry could remember, without any such assistance. Thus, American medical students appear to need to be reminded by Bo, the first two letters of the word "Boston," that the *B*iceps muscle of the thigh formed the *o*uter ham-string; and by Bi, the first two letters of the word "Biceps," that the *B*asilic vein (which is near the biceps flexor cubiti) is the *i*nner of the two veins of the forearm.

Again, Dr. Hodges rather encourages his students to lazy dissection by constantly pointing out that this or that part cannot be exposed in ordinary dissection; some of which things, we think, can be very satisfactorily demonstrated by a good dissector, working sedulously on a good subject.

But a few such drawbacks, notwithstanding, we consider this book creditable to Dr. Hodges, and can easily understand that students who do not wish to dissect more thoroughly might prefer it to the English manuals, Ellis or Holden. Whether this does not tend to encourage superficiality may be a question. For a beginner, Dr. Hodges' work would, no doubt, answer better than those we have named, as being less complicated and more easily intelligible, while its division into "daily portions" is certainly convenient. For more advanced dissections, Mr. Ellis's work remains still, to our thinking, unsurpassed.

ART. IV.—*On Echinokokker i Hjernen.* Af Dr. VALD. RASMUSSEN, *Hospitals-Tidende,* Kjöbenhavn d. 12 Decbr., 1866.
On Echinococci in the Brain. By Dr. VALD. RASMUSSEN.

IN the number of this Review for October, 1866, we gave a tolerably full analysis of some of Dr. Rasmussen's valuable contributions to helminthology. A brief notice of the more

recent paper by the same author above referred to may constitute an appropriate appendix to our former article.

The occurrence of entozoa in the great nervous centres in man is rare, and is confined to what have been known under the collective name of "hydatids." Of these, the only two forms observed in the human subject are the echinococcus and cysticercus, of which the first exercises by far the greater influence on the functions of the brain, and almost constantly induces death, while the cysticercus, especially when it occurs singly, seldom manifests its presence by any morbid phenomenon, and is only accidentally met with after death. This is due to the different development of the two hydatids; the growth of the one being unlimited, while the cysticercus attains only a comparatively small size.

Dr. Rasmussen relates the case of Peter Petersen, aged thirty-four, admitted into hospital on the 17th of September, 1866. His symptoms commenced nine months previously, without any assignable cause, except exposure to cold while engaged in farming work. There was a feeling of numbness in the right arm, soon followed by a similar sensation in the right leg. Five or six months later violent headache set in over the whole upper part of the head, with considerable impairment of memory and some indistinctness of speech. There was some distortion of the face, the right cheek being flaccid, and the angle of the mouth slightly pendent. The tongue deviated to the right, the uvula to the left. The patient's speech was much affected. He complained of difficulty in swallowing, which was not, however, perceptible to the bystanders. The urine contained no albumen. The patient died comatose on the 11th of October. On examining the head, the arachnoïd stretched between the second and third frontal convolutions on the left side was of about the natural breadth; in a limited spot of about a finger's breadth it was rather whitish, and slightly thickened. On cutting through the membrane at this point a cavity was found quite filled with a very tense, whitish, elastic vesicle of rather more than the size of a duck's egg, which, on being punctured, gave exit to an abundant, clear fluid, in which were found only some small whitish flakes and numerous small bodies like grains of sand; on incision, the edges curled outwards, and the wall of the vesicle had on the whole all the characters belonging to the cuticle of echinococci. The echinococcus was nowhere adherent to the walls of the cavity in which it lay, but was taken out with the greatest ease. In the echinococcus-cyst extremely numerous, free, well-preserved scolices were found on microscopical examination. No entire brood capsules were seen, nor were daughter-cells in any stage met with.

The author points out that this case presents no slight interest in both a helminthological and a clinical aspect. In the first point of view, as an instance of a single echinococcus-cyst containing scolices, hitherto thought to be extremely rare, but of which he has met with three out of five cases observed by him. The *size* of such cysts in the brain varies greatly; very rarely does it much exceed that described in the foregoing case, though Abercrombie quotes an instance where, in a boy aged eleven, the cyst contained twenty-one ounces of fluid. Most frequently echinococci do not coexist in any other organ. In the brain their most usual *seat* is the membranes. Sometimes, but rarely, it is in the lateral ventricles; lastly, as in the above case, it is in the cerebral substance itself. In this instance the cyst had evidently been developed from the surface of the brain, and thence forced its way into the substance of the organ.

The author observes that as the echinococcus scoleciparíens is derived from the tænia echinococcus of the dog, the patient in the above case must have swallowed mature ova of the same with his food or drink; and he proceeds to show how the embryo *may* have worked its way through the aortic system to the locality where it became developed. He remarks upon the absence of a proper capsule in echinococci in the brain, it being mentioned in all cases reported with any accuracy that the excavation in the substance of the organ is lined with a thin layer of connective tissue, which may be considered as an "actual cyst," and is described as a "cellulo-fibrous" vascular lamina. Dr. Rasmussen suggests that these cysts so described may have been, like that in his case, the pia mater, "whose true nature, by reason of its deep situation, or of careless preparation, or from some other cause, has been mistaken."

The effects of echinococci in the brain are in general those of pressure. The disease usually occurs in middle life, between the twentieth and fortieth years. One of the earliest and most constant symptoms is headache, gradually increasing in frequency and severity. This is accompanied with attacks of vertigo and with rheumatic-like pains in the body, especially in the extremities, as well as occasionally with obstinate vomiting. Next come lesions of mobility, articulation, sight, and hearing. The organic functions are generally undisturbed, constipation, in addition to the vomiting already mentioned, being the only symptom to be mentioned in connection with the process of digestion. The duration of the disease, *from the occurrence of the first symptom,* seems not to exceed four years, in some cases it was only a few months; the mean may probably be stated as a year. The result is almost always death.

ART. V.—*On Anthracosis or Coal Miners' Phthisis.* By J. WARBURTON BEGBIE, M.D. (Reprinted from the 'Glasgow Medical Journal.') 1866. Pp. 22.

THIS is a very interesting paper on what is now a somewhat rare disease. The author has met with two well-marked examples, which form the basis of his remarks on its etiology and pathology. In regard to the origin of the carbonaceous deposit, two views have hitherto existed:—1st, that the source is external, and that the carbon is inhaled and impacted in the ultimate bronchial ramifications and air-cells (Thomson, Carswell, Mackellar, and others); and 2nd, that the source is internal, the deposit originating as hematin, and undergoing transformation (Virchow). Dr. Begbie brings forward a third view, which certainly embraces, more completely, the condition of the case than either of the former theories. Perhaps we may best express it by giving, in his own words, the conclusions to which a very careful survey of the facts have led him. They are as follows:

"1. Anthracosis is primarily determined by the inhalation of carbonaceous particles.

"2. That, in the instance of the coal miner, while capable of being produced in various ways, the chief exciting cause is the inhalations of the very impure atmosphere, occasioned by the burning of oil lamps. It would appear that the long-continued inhalation of a very *dusty* atmosphere may, under certain circumstances, engender the same condition.

"3. That, when once the deposition of carbon in the pulmonary structure has taken place to any extent, and the true function of respiration is thereby interfered with, there occurs a tendency which gradually increases to the arrestment of carbon or carbonaceous pigment in the lungs, and its removal then from the blood.

"4. That the presence of black pigmentary deposits in the bronchial glands, the pleura, and less frequently the peritoneum and mesenteric glands, makes it probable that there may, in many cases of anthracosis, be some peculiar process of carbonaceous absorption as well as deposition of carbon.

"5. That, in this view, the opinion as to the black pulmonary deposit being the result of transformation in hematin, although supported by so distinguished an observer as Virchow, cannot be considered as so readily reconcilable with what we know of the natural history, and especially the etiology of the disease."

To all who feel interested in this curious lung disease, we commend Dr. Begbie's essay as a very important and valuable contribution to our knowledge of the subject.

ART. VI.—*Chemistry, Inorganic and Organic, with Experiments and a Comparison of Equivalent and Molecular Formulæ.* By C. L. BLOXAM, Professor of Practical Chemistry in King's College, London. London: 1867.

THIS comprehensive chemical manual possesses so many merits that we regret that it is impossible to speak of it in terms of altogether unalloyed praise. Our objections to the volume do not relate to its plan, nor to the accuracy of its data, and of its descriptions of phenomena. What we complain of is, the author's use of what he calls equivalent formulæ in representing equations, &c., and his recurrence to the Fahrenheit scale and the grain weight. The author is himself conscious of the "obstinate conservatism" of these peculiarities of the book, and, indeed, no one knows better than himself that many of his formulæ and equations are simply impossible. And although Mr. Bloxam thinks his method simple for the young students for whom he chiefly writes, yet we consider the book liable to convey erroneous impressions to them. In concluding this brief notice we should be doing a grave injustice to the book if we did not freely acknowledge that, in some particulars, it is a most valuable manual. We single out for special praise the numerous descriptions of appropriate lecture experiments, with their beautiful woodcut illustrations.

ART. VII.—*Hospitals, Infirmaries, and Dispensaries: their Construction, Interior Arrangements, and Management, with descriptions of existing Institutions, and remarks on the present system of affording Medical relief to the Poor.* By F. OPPERT, M.D., F.R.C.P.L., &c. London, 1867. Pp. 218.

THE title-page of this volume is amply expressive of the intents of the author. The work comprises an account, not only of the principal hospitals of this country, but also of the Continent and America, the more important of which are elucidated by plans. It is divided into two parts with appendices. The first part relates to the construction of hospitals, their administration, and that of charitable institutions, and to special hospitals; with two appendices, one on the institutions for training midwives, the other entitled, "useful weights and measures." The second Part contains a description of hospitals and infirmaries, with two appendices, one relating to hospital

tents, marquees, and huts; the other, following an account of the French hospitals—descriptive of the other institutions under the Paris administration; concluding with remarks on the Metropolitan Poor Bill.

The illustrations are as many as fifty-eight. Under the literature of the subject, as many as eighty-eight works are named, of which, we are informed, fifty-three have been specially consulted by the author. An index is added.

From what we have read of the work we have formed a very favorable opinion of its contents, so much so that we think it will become a standard, and will be of much utility. Considering its nature, especially its descriptive part, relating to foreign hospitals and their localities, in all parts of the world, it would be too much to expect absence of all errors. In turning over the pages we have seen one, we may remark, requiring correction. The author, after giving an account of the many and large hospital establishments in Malta, adds,—"We might feel surprised that so many large buildings are required for the poor of Malta, as the island contains only 6000 inhabitants; but I am informed that half of these are paupers, and the number of female paupers is especially large; further, we might wonder why all the buildings are placed close together. But this is considered preferable, on account of centralisation." Now, the fact is, that the population of Malta, one of the densest on the face of the globe, exceeds 100,000. It is, indeed, a poor population, abounding in beggars, but that half of the inhabitants are paupers is very wide of the truth. Nor was there that centralisation adverted to when we were acquainted with the island, a few years ago, and we have our doubts that it exists at present.

The author's suggestion, following his judicious remarks on the Metropolitan Poor Bill, we extract, being, as we think it, deserving of being widely known and well considered.

"In conclusion" (he says), "I venture to suggest that a commission of competent medical men, should—if not appointed by authority—meet, of their own accord, to decide on the following questions respecting the system of medical relief in London:

1. "How to manage the affairs of dispensaries, workhouses, infirmaries, hospitals, and special hospitals, so that they do not interfere with each other's interests.

"2. Which of these institutions should be used as a means of medical education?

"3. Which institutions should become a means for training midwives on the same principles as the Hebammen Institute? How long should the instruction last, and who should compose the board of examiners?

[1] So called in Prussia; a kind of midwifery college in which lectures are given, &c., with clinical instruction.

"4. How to effect a partial change in the present system of admission into hospitals.

"5. If a popular treatise could be prepared, containing special instructions on ventilation, for the nurses and house-surgeons.

"6. Should night-nurses exist?[1]

"7. To prepare tables of statistics respecting zymotic diseases for hospitals, workhouse infirmaries, and special hospitals, and to determine the necessary water-supply per head, per day, by careful investigations made on a uniform plan.

"8. To district the metropolis dispensaries, and for the visitation of the poor in a more efficient manner.

"9. To consider if it would be advantageous to form a pauper pharmacopœia for the use of the new dispensaries."[2]

ART. VIII.—*Guide for using Medical Batteries; showing the most approved apparatus, methods, and rules for the medical employment of Electricity in the Treatment of Nervous Diseases.* By ALFRED C. GARRATT, M.D., &c. Philadelphia, 1867. Pp. 180.

WE are informed by the author in his preface that this little volume is intended to be a practical guide to the medical and surgical uses of electrical apparatus, and that it is a condensation of a portion of his larger work, 'Medical Electricity and Nervous Diseases.' What we have read of it, we have no hesitation in saying, has impressed us strongly of its merit. The account given of electrical instruments, those of the most approved kind, is very full, and is well illustrated by figures from electrotype plates; and an interest, moreover, is imparted by the historical notices introduced, showing the progress of electrical science and its application to medicine.

This descriptive and explanatory portion in varied detail occupies nearly one half the volume, constituting the contents of the first chapter.

In this part of his subject we find a statement made by the author, not that it is of much consequence, of an erroneous kind, viz. of his having seen the remains of a voltaic battery of the Cruickshank form, in the laboratory of the Royal College of

[1] This seems to us an unnecessary question. We agree with the opinion of the author expressed, where treating of nurses, that night nurses should not, as is too often the case in this country, be of an inferior grade, and receive less pay than day nurses.

[2] The author, in a former part of the work, remarks—"The Poor-law Medical Officers on the Continent, as a rule, only prescribe the medicines, which are made up in the city by the chemists, who are paid by and give a discount to the board, or the medicines are dispensed by functionaries paid by the board. A special pauper pharmacopœia usually exists."

Physicians and Surgeons in London, which had been provided for Sir Humphry Davy, and by means of which he, and more recently Faraday, had made their splendid discoveries. We need hardly remark, that what Dr. Garrett saw must have been in the laboratory of the Royal Institution.

The second and remaining chapter is devoted to the methods and rules for the medical and surgical employment of electricity, and is also very comprehensive, marking well the sound judgment and accurate science of the author, the former especially as exemplified in the many cautions which he gives respecting the use and abuse of the agent. His view of its uses is clearly indicated in the following quotation:

"*General propositions*—The first question that arises here is, where, and when, and for what practical indications, may electricity as a therapeutic be considered as indicated. By standard works on therapeutics, as Wood, Stillie, and others, we are justified in the following general propositions:

"1. To *stimulate* peculiarly and especially by a given appropriate form of electricity, parts in which sensation, or the normal power of motion, may be defective or wanting; as in paralytic conditions of the muscles or muscular power, or of general sensibility, or of the special senses.

"2. To *correct* deranged sensations, which includes the quelling of pain and distress; to tone the chords and muscles, as well as to relax excessive contractions, as in some forms of neuralgia, rheumatism, &c.; to alter morbid nutrition by electrolysis and catalysis; to effect through its chemical agency a local coagulation of the blood, as in aneurism and varicose veins; and to renovate simply exhausted nerves and vital forces, and to cauterise nerves and sanguineous growths.

"3. To operate *revulsively* on the nerve-centres through the medium of the skin-nerves, as in various internal and profound affections, even including some chronic inflammations, congestions, and organic plethora and rheumatisms; thus, to promote digestion, or any of the secretions; or peristaltic action, absorption, and circulation; or to equalise animal temperature, as from the head to the feet; as also the nutritive and depurative processes, especially when electricity is directed to the flagging vital properties; also to awaken a keener susceptibility of the system to the action of other medicine.

"4. To awaken the system generally, as in asphyxia, syncope, and from the poisonous effects of narcotics, and to recover from extreme exhaustion, and to arouse from suspended animation."

Having a high sense of the merits of his work, we think it deserves a place in every medical library, the knowledge which it imparts both as to soundness and exactness being precisely of the kind most needed by the practitioner, who purposes to avail himself of electricity as a therapeutic agent; to use the words

of our author, not as a "*cure-all,*" but, as he insists, "one of the most valuable aids in certain diagnoses, and one of our most controllable and precious remedies;" a sanguine opinion this indeed! which we hope the future may verify.

ART. IX.—*Insanity in its Medico-legal relation. Opinion relative to the testamentary capacity of the late James C. Johnston, of Chowan County, North Carolina.* By WILLIAM A. HAMMOND, M.D., &c. New York. 1866. Pp. 72.

THE subject-matter of this volume, a case of monomania with presumed lucid intervals, giving rise to the question whether a will made by the individual was valid in law, is ably discussed by its learned and accomplished author.

Dr. Hammond adduces a great mass of evidence from the works of the most approved authorities in support of his opinion that lucid intervals, so called, may occur with persistence of mental disease, the organic cause remaining uneradicated, as, if we may use the expression, a smouldering fire ever ready to break out into flame.

The author's *résumé* will give our readers a good idea of this interesting case and of the argument employed. We prefix his definition of insanity, which is one of the best we are acquainted with, viz. "a general or partial derangement of one or more faculties of the mind, which, whilst not abolishing consciousness, prevents freedom of mind or of action."

"1. That there is a form of insanity known as monomania, which is characterised by a perversion of the understanding in regard to a single object or a limited series of objects.

"2. That one of the most prominent features of this species of insanity is a morbid feeling of hatred to friends and relatives, and a disposition to do them injury.

"3. That it is especially a symptom of monomania to imbibe delusions which exercise a governing influence over the mind of the affected individual, and force him to the commission of acts which in a state of sanity he would not perpetrate.

"4. That the monomaniac has power to conceal his delusions, and to arrest the paroxysms of delirium to which he may be subjected.

"5. That the testator, James C. Johnston, was affected with monomania: that he had conceived a dislike to his relatives; that he was subject to delusions in regard to them; that he was, in consequence, not free to make a will such as he would have made had he been sane; and that he had power to conceal his delusions and to control his paroxysms of delirium.

"6. That there could not possibly have been a lucid interval when

the will was written, signed, acknowledged, and reaffirmed, because all these acts" [acts described in the volume] "show a continuance of the delusions under which the testator laboured. Insane acts can never establish lucid intervals.

"7. That, therefore, he was not possessed of testamentary capacity; and that, consequently, the paper dated the 10th of April, 1863, and re-affirmed the 13th of September of the same year, is not his last will and testament."

What was the decision of the court is not stated; but in a similar case, that of a Mrs. Thwaytes, whose will was contested by her relations, the judgment pronounced by Sir James Wilde accorded precisely with the opinion of Dr. Hammond, as to the lady's incompetency. We quote from the 'Times' of the 7th of August the following rule in the words of the judge: "A person who is the subject of monomania, however apparently sensible or prudent on all subjects and occasions other than those which are the special subjects of his apparent infirmity, is not in law capable of making a will."

ART. X.—*Tapeworms (Human Entozoa): their Sources, Nature, and Treatment.* By T. SPENCER COBBOLD, M.D., F.R.S., &c. London, 1866. Pp. 83.

THIS little work is such as we might expect from an author who has treated the great subject of entozoa so ably. Limited to the most formidable genus of these parasites, and that the genus, the natural history of which has been so well elucidated, it is admirably adapted to enlighten all those persons who have not made helminthology their special study; and whilst informing them of the dangers to which they are exposed,—how they may become the hosts of these treacherous and formidable enemies of the human race, not neglecting to point out how they may be best avoided, or, if present, got rid of.

Brief as the work is, it is not without original matter, contained in an appendix of a few pages. Therein Dr. Cobbold gives the results of some of his latest researches, tending to prove that the ox is as often the habitat or host of the entozoon as the pig, and that measly beef is as common as measly pork, if not more so, as he has found the tapeworm which infests the former, *tænia mediocanellata* of more frequent occurrence than that which infests the *tænia solium*. The manner in which he has demonstrated this important point is an excellent example of experimental research; and considering the means by which it was accomplished, of the benefit also of grants of money for scientific inquiry, in this instance, we are pleased to

think, made by the council of the British Association at the recommendation of the physiological subsection. The details of the experiments are to be found in the fourteenth volume of the 'Proceedings of the Royal Society.'

In concluding, the author points out the possibility of getting tapeworms from eating mutton, hitherto considered safe in relation to this danger. He states that he has on three separate occasions found their embryos—small hydatid-like cysts—in joints of mutton brought to his own table, the exact species remaining to be ascertained; he expects it will be found to be his *tænia lophosoma*.

The value of this little work is enhanced by a classified and detailed list of the species of entozoa at present known to infest the human body, amounting, exclusive of external parasites, to as many as forty-one.

What a happy circumstance it would be for our race were it exempt from this mysterious evil! Though we cannot expect that this will ever be the case entirely, yet we may reasonably hope that, as knowledge is diffused, and means of prevention observed—all of these practicable—it will be to a great extent accomplished. Of the preventive means most efficient, the one most within our reach, and which cannot be too strongly inculcated, is the avoidance of underdone meat, keeping in mind that the temperature required for the meat being well done is fatal to the entozoa; in accordance with which, as is well known, those people who use well-cooked dishes are far more exempt from entozoa than those who indulge in raw or half-dressed viands.

ART. XI.—*A Dictionary of Science, Literature, and Art, comprising the definitions and derivations of scientific terms now in general use, together with the History and Descriptions of the Scientific Principles of nearly every branch of human knowledge.* Fourth Edition. By W. T. BRANDE, F.R.S., and the Rev. G. W. Cox, M.A., assisted by contributors of eminent scientific and literary acquirements. London, 8vo. In three volumes.

WE again introduce this Dictionary to the notice of our readers, chiefly to notify its completion, a matter of congratulation to those of them who have subscribed to it as it has appeared from time to time. Its publication has extended over a considerably longer period than was contemplated, "having been (as stated in the advertisement to the third volume) delayed, in part, by difficulties inseparable from the vast range of the work, but chiefly by the lamented death of Professor Brande, under whose

care the first edition of this dictionary was published six-and-twenty years ago."

The advertisement proceeds to render a merited tribute to the memory of Professor Brande, in which every one will readily concur who knew the venerable chemist either personally or by his works. Previously to his decease he had "in great part revised the articles which related to his own subjects; but his editorial duties have since his death devolved on the present editor," the Rev. G. W. Cox, an able and well-known writer and excellent classic. The list of contributors includes the names of men eminent in the branches of knowledge upon which they severally have undertaken to write. Although we have had occasion to point out, in previous notices of this comprehensive work, various defects and errors in certain articles, particularly in those relative to medicine and to the history of microscopic organisms, yet, as a whole, it is one of great merit, and contains many papers of the highest value for their completeness and accuracy.

ART. XII.—*The Forms, Complications, Causes, and Treatment of Bronchitis.* By JAMES COPELAND, M.D., F.R.S. New Edition. London, 1866. Pp. 165.

COUGHS and cold under the grey sky and pluviose atmosphere of the British Islands make up a very large proportion of the ills of the flesh to which we islanders are subjected. Consequently it is of the very first importance to us to know how to guard against and resist these evils of climate, and also, where all our precautions have failed us, to know how to successfully treat them. This argument of the frequency of and the large mortality from bronchitis and chest disease generally, is a well-worn one by writers on those subjects, and, so far as it goes, a fair enough one. But unfortunately it is often made the apology for the production of books which have nothing to tell us that has not been better told before, and which may be suspected to be written rather for practice than from practice.

This suspicion will never arise respecting a work produced by Dr. Copeland, who writes from the experience of half a century, well weighed and well matured. In his opinion, resting on bibliographical knowledge possessed by very few, bronchitis has not received the attention from writers which its importance deserves. Had we been asked on this point, we should have expressed a very different opinion, and said that bronchitis has been well studied, that it is generally a well-understood complaint, and that it has found many writers

to discourse upon it. However, we will not dispute the dictum of so competent an authority as Dr. Copeland is on this matter.

In the 'Dictionary of Practical Medicine,' published in 1832, a very elaborate history of bronchitis appeared, of which the present treatise is, for the most part, a reprint. It is, however, as Dr. Copeland implies, something more; for he refers to it as containing the results of uninterrupted observation continued since that date. At least, if it do not contain much new matter, it may be taken to represent the ingathering of thirty-four added years of experience in practice. It may be accepted as the most complete monograph on bronchitis that we possess, describing in detail the forms, the diagnosis and prognosis, the causes and the morbid changes induced by it, the prevention and the treatment of this disease, both when acute and when chronic. A supplementary chapter treats "of certain morbid conditions of the respiratory organs sometimes consecutive of bronchitis," viz. congestion of the bronchi and lungs, bronchorrhœa, dilatation and ulceration of the bronchi.

The subject-matter of the book is treated in a highly practical manner, and the question of treatment comes in for a larger share of consideration than is usually allotted it in most medical works of the present day. Dr. Copeland has not followed in the wake of those who ignore the so-called antiphlogistics in the treatment of inflammatory diseases. The remedial measures he suggests in sthenic acute bronchitis are those of a generation well nigh extinct. Blood-letting, general or local, is to be employed, in healthy, vigorous subjects, as a means of shortening the disease, and of rendering other remedies more effectual; and after it are to follow in due course antimony or ipecacuanha, singly or combined, and diaphoretics. The antiphlogistic doctors come in for the following sharp critique:

"At the time when I first wrote upon bronchitis, general and local bloodletting was often required early in acute bronchitis, but now, and for many years past, it has been very generally anathematised; and, although the asthenic diathesis and character of diseases have so long prevailed, yet I believe that this treatment has been sometimes neglected where a cautious recourse to it was required, owing to the generally prevailing opinion against it, an opinion entertained by many quite incapable of forming a correct judgment as to the circumstances either requiring it or contra-indicating it, and as to the manner in which the adoption or the neglect of it may influence the course and the termination of the disease."—P. 89.

These observations are repeated at pp. 122, 123, and somewhat expanded. The use of inhalations meets with his approbation: "The inhalation of emollient and medicated vapours is occasionally of much benefit in the sthenic form of the disease,

but chiefly in its first and second stages;" and the author further discusses the value of the inhalation of or fumigation with stimulating vapours. These, he believes, have often been used in too concentrated a state, and it is this that has led to their disuse. They have not been fairly and sufficiently tried; "and although the cases have been few in which these substances have been used by me, yet sufficient evidence of advantage has been furnished to warrant a cautious recourse to them" in chronic bronchitis. Of inhalations also of the fumes of the balsams, of the terebinthinates, of the odoriferous resins, &c., he remarks that, from what he has seen of their effects, they are of much service in the chronic forms of bronchitis" (p. 135). They are, moreover, enumerated among the remedies for dilatation and ulceration of the bronchi.

This little work by Dr. Copeland will recommend itself especially to the working medical man who wishes to discover the modes of treatment pursued in the multiform varieties and complications of bronchitis occurring in practice, and to learn which of them have the good opinion of a physician who has occupied so prominent a position in the medical world during an almost unprecedented period.

ART. XIII.—*The Climate of the South of France, and its varieties most suitable for Invalids; with Remarks on Italian and other Winter Stations.* By CHARLES T. WILLIAMS, M.B. Lond. 1867. Pp. 90.

TIME and space will not permit us to do more than to give a passing notice of this work. It is divided into six chapters, plainly and pleasantly written, and not too long, embracing the following subjects, viz. the "general advantages" of the climate; "physical aspects," including thermometrical, hygrometrical, and other meteorological phenomena; "health resorts of the region," especial notice being taken of Hyères, Cannes, Nice, and Mentone; "medical aspects;" "effects of the climate on health and disease;" "hygienics of consumption in the south of France;" and "winter stations of Italy." Under these various headings the reader will come across much that is valuable and interesting, embodying, not only the results of the author's own observation, but also the experience of several authorities and experts whom he has wisely consulted. We hope, and we doubt not, that many a traveller in search of health will read this brochure of the recently elected physician to the Hospital for Consumption and Diseases of the Chest. Its possessor will find in it a very amiable companion, and one full of many a good and practical piece of advice.

PART THIRD.

Original Communications.

Art. I.

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

(*Concluded from* vol. xxxix, p. 496.)

The same influence of age which characterises the different forms of hereditary hæmorrhage in early life, as well as other diseases of the vascular system which are necessarily attended with hæmorrhage, and many of which are more commonly developed later in life, can often be traced in hereditary insanity and in various hereditary affections of the brain and nervous system. It has, for example, been long known and very generally admitted that the different forms of insanity are liable to be thus hereditarily limited by age, and cases have been recorded which show that this limitation may occur at almost any period of life. In support of this statement, it may be mentioned that nearly all the principal authorities on the subject have published cases in which the disease has been so limited, many of which have been already cited to illustrate the influence of sex, which is commonly conjoined with it; and although this limitation by age as well as sex is often independent of any preference connected with the transition periods of reproductive development in either sex, yet there are many cases in which the restrictive influence of age is associated with some developmental epoch. Dr. Leubuscher,[1] for example, after informing us that "there are many curious cases recorded, in which the disease broke out in the children exactly at the same periods of life in which it had manifested itself in the parents, and, likewise, where it occurred at the same age in various collateral branches of a family," proceeds to state that coincidences in the time of the attack of hereditary insanity may be most easily explained by the influence of sexual development, and that the following proposition may be established, namely, "that the outbreak to an hereditary disposition to insanity

[1] "Remarks on the Hereditary Transmission of Insanity;" 'Journ. Psychol. Med.,' vol. i, 1848, pp. 274-5.

is especially connected with those processes of development, as the occurrence of puberty, childbirth, and the climacteric period." But it is doubtful whether the cases in which this preference is shown are either so common or so well marked as some writers on insanity would lead us to suppose. For no age, as Dr. H. G. Stewart[1] has remarked, is exempt from hereditary mental disease, and therefore in many of the cases which have been recorded there could have been no connection between the occurrence of insanity and any such transition period in development. This has even been noticed in cases in which the insanity seems to be specially connected with some sexual epoch in development, as, for example, in cases of erotomania, in which there is sometimes hereditary limitation by age, without any preference of this kind being shown. For when, as Dr. Guislain[2] more particularly has noticed in cases of senile erotomania, inquiry is made into the family history, it will be found that "a sister, a brother, an uncle, an aunt, have been mad, and very often at an advanced age." To the same influence of age must also be ascribed some of those cases of insanity in families, in which two or more brothers or sisters are affected, and which are commonly described as cases of insanity by imitation. Such, for example, as a case which has been observed, in which two sisters, unmarried, the one aged thirty-eight years, and the other thirty-five years, both became insane at the same time, and suffered from delusions of the same character; or a somewhat analogous case, in which two sisters, one aged thirty-eight years (who was unmarried), and the other aged thirty-six years (who was married), both became also insane at the same time, and suffered from delusions of a similar character. In such cases the influence, if any, of imitation appears to be altogether subordinate to hereditary influence, although it must be admitted that there are occasions when imitation seems to have an important influence on the development of the disease; as, for example, in the occasional occurrence of epidemic insanity, which has been so ably and so fully investigated by M. Calmeil and M. Brierre de Boismont, and in some of the insanity which was consequent on the outbreak of the so-called religious revivals in our own day. It would appear, however, that some writers assign less importance to imitation than to fear in the development of insanity as a family defect; for Dr. Leubuscher[3] remarks that just as "the fear of an epidemic is the surest means of being attacked by it," so the "consciousness that one descends from insane parents, and bears the germ of insanity within one's organism, acts as a powerful incentive to the outbreak of the disease." It is obvious, however, that as "the germ of insanity" here referred to must have been the

[1] 'Journal of Mental Science,' April, 1864, p. 62.
[2] 'Leçons Orales sur les Phrénopathies,' 1852, t. i, p. 177.
[3] Loc. cit., p. 276.

result of hereditary influence, its subsequent development, whether aided or not by imitation or by fear, may with certainty be referred to the influence of age.

In the large and varied class of convulsive diseases which, according to Churchill, Bouchut, Trousseau, Barthez and Rilliet, Baumes, and many other authorities, are very commonly associated with hereditary influence, restriction by age is frequently well marked. In illustration of such restriction may be cited a case mentioned by Willis,[1] in which all the infants in a family died from this cause at the age of three months. Gaussail [2] refers to a case observed by Raymond de Vieussens of epileptic convulsions in a male infant, which began at the age of seven months, and were repeated seven or eight times a day during a period of two months, and which were followed by imbecility. The father had suffered in the same way from convulsions during infancy, and two other infants in the family had died from them. Barthez and Rilliet [3] mention the case of "two little girls born of a mother with strongly marked hysteria being, the one and the other, and at nearly the same age, seized with a violent attack of eclampsia." Guersant and Blache [4] mention the case of a woman who was extremely passionate during pregnancy, and who lost three infants successively from convulsions soon after birth. Such cases as the preceding must be familiar to all who have had their attention much directed to the diseases of children. Whilst in other cases of less frequent occurrence the hereditary influence of age has been of a more strongly marked character, and has been observed to prevail to a far greater extent. An example of this has been mentioned by Trousseau,[5] who, after informing us "that it is not unusual for parents, and especially for mothers, subject to convulsive attacks in their infancy, to give birth to individuals who in their turn are similarly affected," cites a remarkable case recorded by Dr. Duclos, of Tours, in which a family composed of ten children all had convulsions in their infancy. The mother, who herself had suffered from frequent attacks of eclampsia up to the age of seven years, belonged to a family also of ten children, six of whom had died of convulsions. A corresponding development of convulsions may also occur without the mother having been similarly affected at any period of her life, as in the following illustration, for which I am indebted to Mr. Curgenven, who has kindly favoured me with his notes of the case. The first nine children of a woman who had never suffered from convulsions,

[1] Cited by Gaussail, 'De l'Influence de l'Hérédité sur la production de la Surexcitation Nerveuse,' 1845, p. 119.

[2] Ibid., p. 121.

[3] 'Traité Clinique et Pratique des Maladies des Enfants,' 2nd edit., 1853, t. ii, p. 473.

[4] Cited by Churchill, 'Diseases of Children,' 2nd edit., 1858, p. 108.

[5] 'Clinique Médicale,' 2nd edit., t. ii, 1865, p. 115.

though belonging to a family in which the disease was hereditary, had been born alive at the full period. Of these children, five were girls, of whom two were twins, who both died within five days of their birth, of marasmus; two died during childhood, of scarlet fever and measles respectively; and one girl is living and healthy. *None of these female children had convulsions.* The four other children were boys, two of whom died of convulsions in infancy; and the other two, who are now living and healthy, suffered very much from convulsions during infancy. In the four succeeding and last pregnancies the fœtuses died about the eighth month; and it is evident (Mr. Curgenven informs me), from the sensations of the mother immediately preceding the death in each case, that they all died of convulsions. With reference to the death of the last fœtus, the mother stated that its movements began very early, and were both frequent and strong; that immediately before its death, and whilst she was sitting quietly on a sofa, the movements became very violent, and ended with a sudden plunge, which caused her to feel very faint. The three preceding fœtuses had died suddenly in a similar manner at the eighth month. *They were all males.* The mother of this family of thirteen children, four of whom were thus born dead at the eighth month, had had ten brothers and sisters, all of whom, with the exception of one sister, had suffered from convulsions during infancy. Two of her brothers and four of her sisters had died of convulsions between the ages of three and fourteen months, and two other brothers, who lived to grow up, had also suffered during their infancy from frequent attacks of the same disease. In this case it may be noticed that the hereditary limitation by sex had, in the subsequent part of the family history, a considerable influence on the development of the disease, for, besides the occurrence of convulsions in several infant members of the same family, as well as in those belonging to collateral branches, the disease, after having become associated with the restrictive influence of sex, moreover affected four successive male fœtuses, and was apparently the cause of their death at about the eighth month of intra-uterine life.[1]

In passing from the period of infancy to childhood, it may be observed that the convulsive attacks, which in the earlier period of life are distinguished by the term eclampsia, and in the later period of life are known as epilepsy, continue to be subject to the same restrictive influence of age. For Romberg,[2] after stating that "it

[1] This occurrence of convulsions in the fœtus-in-utero, besides being an occasional cause of its premature death, may also be referred to as the cause of some of those congenital deformities which are associated with permanent and abnormal contraction of certain muscles, and which in many cases have been observed to be subject to hereditary influence.

[2] 'A Manual of the Nervous Diseases of Man,' translated and edited by Edward H. Sieveking, M.D., Sydenham Society, 1853, pp. 193 and 208.

is undeniable that an hereditary constitution exists," since "in some families almost all the children are attacked with eclampsia," subsequently adds that "the predisposition (in cases of epilepsy) afforded by a certain age is of much more consequence" than fright on the mother, which has been popularly alleged as an apology for the occurrence of this disease.

With respect to other forms of convulsive disease, it may be useful to direct attention to the frequency with which chorea has been observed in two or more members of the same family at the same age, the occurrence in these cases being generally limited, as is usual, to one sex, the female, and a preference being shown for some age between eight and fourteen years. Dr. Churchill[1] has expressed some doubt "whether we ought to consider chorea as an hereditary disease; although [he adds] Coste, Young, and Constant, have each met with an example, and Stiebel has known families in which every child was attacked at a certain age." There need, however, be no hesitation in admitting, not only that chorea is an hereditary disease, but that sometimes, when subject to the hereditary restriction of age, it may be independent of its usual preference for the above-mentioned period of life. For in one of the cases recorded by Duchamp,[2] in which two children (brother and sister) in the same family suffered from the disease, the attack began about the age of two years; and in another case, cited by the same writer, in which two children (sisters) in the same family suffered from the disease, the attack began at the age of seven years.

The same hereditary influence is also noticeable in many cases of local as well as of general affections of the nervous system. In cases, for example, of stuttering, which, like chorea, appears to be very commonly acquired by imitation, it is deserving of notice that the preference which is usually shown in both affections for one age and for one sex is probably less due, in many of the families affected, to imitation than to hereditary influence. For stuttering, as regards age, is very rare in early childhood, and habitually disappears in old age; and, as regards sex, it has been noticed that it is almost exclusively limited to males. Among the cases of this description in which hereditary limitation of a well-marked character has been observed is one mentioned by Dr. Graves,[3] in which all the male children for three generations had this infirmity of speech, but none of the females in the family suffered from it. In such cases as these it is evident that, even if it must be admitted that the affection is less due to hereditary influence than to imitation, the latter influence must to some extent be regarded as an hereditary instinct, liable to

[1] 'Diseases of Children,' 2nd edit., 1858, p. 100.
[2] Maladies de la Croissance,' 1823, pp. 21 and 23.
[3] Clinical Lectures on the Practice of Medicine,' 2nd edit., vol. i, p. 575, 1848.

be limited in either case to a certain period of life and to a different sex, in consequence of which the females in some families suffer from chorea, whilst the males do not; and in other families the males stutter, whilst the females are gifted with fluency of speech.

In cases of infantile trismus, which is a disease specially limited to a very early period of life, it may be remarked that, although frequently due to endemic or some other and external cause, such cases are occasionally associated with hereditary influence. So likewise in phrenoglottism, or spasm of the glottis, which, besides being limited in a similar manner as regards the seat of the attack to one locality, is almost necessarily also limited to one age, the effect of hereditary influence has been very distinctly recognised by many writers on the subject. Dr. Churchill,[1] in citing the authority of Ley (*Laryngismus stridulus*, p. 71) on this point, informs us that the disease "certainly is often observed in several children of the same family successively;" and the same fact has been specially noticed by Bouchut,[2] who, in his description of this disease, refers to its "successive appearance amongst *all* the children of the same family, of which Rullmann, Kopp, Marshall Hall, Toogood, &c., have reported several examples." Among other illustrations of the hereditary occurrence of this disease which have been recorded is a case observed by Davies and Henrich, in which five children of the same family were seized with spasm of the glottis; and a case observed by Powell, in which out of thirteen children born of the same parents only one escaped the disease.[3]

The influence of age in limiting the hereditary occurrence of what may be strictly defined as cerebral disease has been referred to in the earlier part of this paper in connection with hydrocephalus as a congenital defect; and in again directing attention to its effect on this class of diseases it will be useful to notice the exactness of the limitation by age in some of the cases which have been observed, in which the limitation seems to be more strictly defined when it is associated, as it not unfrequently is, with limitation by sex. This occurred in a case which came under my own observation some years ago, in which the wife of a journeyman gilder lost all her sons from disease of the brain (tubercular meningitis) at the age of five and a half years; whilst her daughters, by escaping the disease, survived the age which had been uniformly fatal to their brothers. It was ascertained that there had been ten children in the family, five of whom were boys, who had died in this way at or within a few days of the same age. The first-born child in this family, a girl aged fifteen years, has had a persistent squint in the left eye since the

[1] 'Diseases of Children,' 2nd edit., 1858, p. 237.

[2] 'Practical Treatise on the Diseases of Children and Infants at the Breast,' translated by P. H. Bird, F.R.C.S., 1855, p. 161.

[3] Cited by Barthez and Rilliet, 'Mal. des Enf.,' t. ii, p. 527, not

age of twelve years, and she has been much troub'ed with worms, but not otherwise affected in health. The second child, a boy, died at the age of five and a half years from tubercular meningitis. The third, fourth, and fifth children, all boys, died at the same age from the same disease. The sixth child, a girl aged eight years, has had a squint in the right eye since the age of seven years, but she is otherwise free from disease. The seventh child, a girl aged six years, and the eighth child, a boy aged three years, are both free from disease. The ninth and tenth children are twins, a boy and a girl, aged one year and a half, and they are both free from disease. The mother in this case has been under medical treatment during the last few months for phthisis, and it is to be noticed, with reference to the squint in two of her daughters, that she has had, since the age of three years, a squint in her right eye, which was caused by exposure to the sun. In connection with such cases as the preceding, it may be remarked, with reference to the frequent occurrence of limitation by age, that it was this hereditary character of the disease which specially attracted the notice of Coinde,[1] who, after observing that it is unusual for a single child in a family to be attacked with hydrocephalus, and that he has had under treatment "many whose father and mother had had brothers or sisters who had died from it," very justly states that it probably depends on the fact that "like constitutions are the result of a similar organization, and that from thence the same diseases ought to be reproduced at the same age when there shall be a concourse of circumstances fitted to develop this constitutional identity."

It has been stated by Romberg[2] that "an hereditary tendency is observed less frequently in paralysis than in other nervous diseases;" but this statement, so far as it may lead to the inference that hereditary influence is of very exceptional occurrence in this class of cases, cannot be admitted as correct; for on careful inquiry it will be found that such diseases are not only often hereditary, but that when so they are likewise often subject to the controlling influence of age. This is very noticeable in cases of locomotor ataxy and in that form of paralysis which is accompanied by granular or fatty degeneration of the voluntary muscles, respecting which diseases there is still much difference of opinion amongst writers on the subject, both as to their exact nature and also the proper nomenclature for some of the cases on record. The paralysis in these cases, as in the hereditary hæmorrhages of early life, is usually limited to one sex, two or more brothers in the same family being attacked, whilst their sisters remain exempt; and in most of the fully recorded cases there has also been noticed a very strict limitation by age. A careful analysis of these cases has proved that this influence of age cannot

[1] 'Memoire sur l'Hydrencéphale,' 1817, p. 68.
[2] Op. cit., vol. ii, p. 256.

be referred to any decided preference of the disease for one period of life more than another: for although M. Aran[1] deduces from eleven observations that the paralysis belongs to adult life, the average age at the period of attack in his series of cases being thirty-five years; and even Dr. Duchenne,[2] who has devoted special attention to these paralytic affections of the voluntary muscles, states that both "progressive fatty muscular atrophy" and "progressive locomotor ataxy" are in like manner diseases of adult age; yet from the more extended observations of Dr. Roberts,[3] including 105 cases (of which only twenty-eight are considered by Dr. Meryon[4] to be referable to fatty degeneration of the voluntary muscles), it seems that the paralysis is very apt to make its appearance at an early age, even in childhood, and this agrees with what has been observed by myself and others in some unrecorded cases of the disease.

Apart from the distinctions which appear to have been correctly, though somewhat finely, drawn between locomotor ataxy and other forms of paralysis which have been classed with it, the following may be cited as illustrations of the limitation of such cases by age.—1. A case recorded by Dr. Meryon,[5] in which a daughter and a son, belonging to the family of a surgeon at Swanage, consisting of sixteen children, and of which they were the seventh and eighth in order of birth, began to suffer from the disease at the age of *seven years*. The remaining fourteen children are described as perfectly healthy. In this case there was limitation by age only, and not by sex.—2. A case observed by Dr. Buller,[6] of Southampton, in which two brothers, the sons of a clergyman, were attacked between the age of *eight and nine years*, and both eventually became quite helpless and died. There were two sisters in this family who were both free from the disease, as were also the parents.—3. A case mentioned by Dr. Meryon,[7] and occurring in the family of a gentleman in Somersetshire, in which all the daughters, four in number, are healthy and strong, whilst eight sons have been successively attacked by the disease, six of whom are dead. The information respecting age in this case is incomplete, but from what is stated it may be inferred that all of the eight sons were affected at about the same period of childhood (*eight or nine years*), as in the last case.—4. A case observed by Dr. Duchenne,[8] in consultation with the late M. Maubec, in which a brother and sister were attacked, between the age of *ten and twelve years*, with "progressive, fatty, muscular atrophy," which in both

[1] 'Archiv. Gén. de Méd.,' 4th ser., t. xxiv, 1850, p. 207.
[2] 'De l'Electrisation localisée,' 2nd edit., 1861, pp. 476 and 606.
[3] 'An Essay on Wasting Palsy, (Cruveilhier's Atrophy),' 1858, pp. 74—99.
[4] 'Practical and Pathological Researches, &c.,' 1864, p. 210.
[5] Op. cit., p. 208.
[6] 'Med.-Chir. Trans.,' vol. xxxv, 1852, pp. 79—81.
[7] Op. cit., 1864, pp. 212-13.
[8] Op. cit., 1861, p. 480.

of them commenced in the facial muscles supplied by the fifth pair of nerves.—5. A case recorded by Oppenheimer,[1] in which the disease attacked two brothers at the age of *eleven years;* whilst two other brothers, three sisters, and the parents, remained free from it. Two uncles and two male cousins were in this case affected with the same form of paralysis in early life, but their exact age at the time of the attack is not stated. In this case it should be noticed that, in addition to limitation by age and sex, there is very strong evidence in favour of atavic transmission, similar in character to that which has not unfrequently been observed in cases of the hæmorrhagic diathesis, as well as in other diseases.—6. A case observed by a "physician of eminence," and recorded by Dr. Meryon,[2] in which the usual limitation by sex, with somewhat imperfect limitation by age, occurred in the male offspring of three sisters, and which it is interesting therefore to refer to here as an evidence of atavic transmission corresponding with what has been noticed in the last case. The first of these sisters had a numerous family, all of whom were healthy except one son, who began to suffer from the disease at the age of ten years. The second sister had two sons and one daughter; and the sons only, one aged fifteen years and the other fourteen years, were affected by the disease, which began in them at a very early age. The third sister had one son and two daughters; the son was very early affected by the disease, whilst the two daughters remained perfectly healthy. There was a fourth sister in the family, with only one child, a daughter, now grown up and quite healthy. It was moreover noticed in this case that, whilst three sets of male cousins suffered from the disease "very early in life," with one slight exception, if it could be so called, in which it appeared at the age of ten years, there was a marked peculiarity, consisting of a very uncommon development of the gastrocnemii muscles, which was uninfluenced at least by sex, for it was observed "in all the members of the mother's family." —7. Dr. Little[3] has recorded a well-marked case of the disease in two brothers, one of whom died at the age of fourteen years, and the other was still living at the age of twelve years; it was accompanied, but not preceded, by abnormal increase of bulk in the muscles of the calf.—8. Dr. Heptenmacher[4] has traced with great diligence, partly by means of oral report and partly by searching the ecclesiastical records, the hereditary history of this disease in the allied families of Boehm, Koch, and Stelz, and the result affords a very complete illustration of its atavic transmission to the male offspring through the female line, such as was noticed in Dr. Meryon's case above

[1] Cited by Dr. Roberts, op. cit., cases 90 and 91.

[2] Op. cit., 1864, pp. 205-6.

[3] 'On the Nature and Treatment of the Deformities of the Human Frame,' 1853, pp. 14—16, note.

[4] 'De Ætiologia Atrophiæ Muscularis Progressivæ,' Diss. Inaug., 1862, Berolini, pp. 15—17, and 29.

referred to, with also limitation by age; for all who were affected began to suffer from the disease in early boyhood, and not one survived to his twentieth year. It appears from the history of this case that two healthy sisters married into healthy families, and that the disease, which they transmitted, but did not share, showed itself only in the male descendants. The first of these sisters had two sons, who died from the disease, and two daughters, who were free from it, and who married into healthy families. The elder, having wedded the peasant Koch, of Dossenheim, had a healthy daughter, a son who died in his first year of infantile convulsions, as well as four other sons all affected with progressive muscular atrophy, three of whom are already dead, and the youngest, yet living, manifests the same kind of disease. The younger, having wedded the peasant Boehm, of Wieblingen, had many healthy daughters and three sons. The eldest of the sons (whose case is related by Oppenheimer) died of the disease; the middle one, Andrew Boehm, affords an excellent counterpart of his diseased brother; and the youngest, Jacob, affected when a boy with rickets, has very good health and powerful muscles. The family of the second of the two sisters referred to above consisted only of daughters, one of whom, having married a healthy husband, has a son, Philip Stelz, aged fourteen years, who from his fourth year has suffered from hip-joint disease (coxarthrocace) of both sides and progressive muscular atrophy of the shoulder and of the muscles which move the scapula. Besides those particularly mentioned, other members of the above-named families seem to have been similarly affected, although their relationship could not be distinctly referred to or traced, for Dr. Heptenmacher informs us that "a great many mothers also enumerate other kinsmen who have wasted away," apparently from the same progressive muscular atrophy. It remains only to be noticed that the disease followed exactly the same course in the two families of Koch and Boehm, but not in that of Stelz; and that "all the children descended from the Koch family perspired much, and mostly on the right side."—9. A case observed by Dr. Meryon,[1] in which the disease occurred in two brothers named Tyrrel, living at the village of Playden, in Sussex, both of whom were attacked at the age of *twelve years*, and became totally disabled in their arms and legs at the age of *twenty-five years*. There was a third brother in the family similarly affected, and who had died some years previously, but no mention is made of his age either at the time of the attack or at death. A sister in the same family had continued free from the disease.—10. A case, for which I am indebted to a medical friend, in a branch of whose family the disease occurred. Three sons, who were the third, fourth, and fifth children in a family composed of three sons and three daughters, were all attacked by the disease on approaching *the age of puberty*, and died in succession at

[1] 'Med.-Chir. Trans.,' vol. xxxv, 1852, p. 81.

fourteen, fifteen, and nineteen years of age. Of the three daughters, all free from the disease, the eldest, aged thirty-two years, and the second, aged thirty years, are still living; and the youngest died in infancy. Both parents are living (1864), the father aged sixty-three and the mother aged fifty-five years, and both are free from the disease; as were also the paternal grandfather, who lived to the age of seventy; the paternal grandmother, who lived to be upwards of eighty; the maternal grandfather, who was above seventy at the time of his death; and the maternal grandmother, who died at an advanced age.—11. A case recorded by Dr. Meryon[1] and Mr. Tatum[2], in which four sons belonging to a family of rank, composed of four sons and seven daughters (sometimes referred to as six daughters) suffered from the disease, whilst all the daughters remained free from it. The limitation in this case refers to the age at death, for three of the sons died in succession at the respective age of *seventeen, sixteen, and sixteen years;* whilst the fourth son, although still living in 1866, was suffering from the disease in a very advanced stage.[3] —12. A case recorded by Prof. Freidreich,[4] of Heidelberg, in which the disease occurred in three sisters and a brother, at the respective ages of *sixteen, fourteen, fifteen, and fifteen years.* —13. A case also recorded by Prof. Freidreich,[5] in which the disease began in a brother and sister at the age of *eighteen years.*—14. A case mentioned by Mr. Brodhurst at a meeting of the Medical Society of London, and of which I have since been favoured with his private notes, extending to the latter part of the year 1859. In this case the family consisted of nine children, four of whom, a daughter and three sons, were attacked by the disease at the age of *eighteen years.* One of the sons, who began to suffer from the disease at the age of eighteen years, died from apoplexy at the age of forty years, having had for two years before death symptoms of softening of the brain. A second son, who also began to suffer from the disease at the age of eighteen years, died at the age of forty-five years, having on two or three occasions had symptoms of apoplexy. A third son, affected with the disease since the age of eighteen years, was still living at the age of thirty-one years, when the last report of his case was made. The daughter referred to died of phthisis at the age of eighteen years, and in her the symptoms of muscular weakness had been noticed only a short time before death. Of the other children it appears that one son, the eldest, died from apoplexy at the age of forty-four years; another son died from hydrocephalus at the age of two years; one son, free

[1] Op. cit., 1864, p. 205; and loc. cit., 1852, pp. 73—84.

[2] Holmes's 'System of Surgery,' vol. iii, 1862, pp. 531-2.

[3] This son has since died, March 17, 1867, in the eighteenth year of his age.

[4] 'Archiv. Gén. de Méd.,' Dec., 1863 (paper abridged from 'Archiv für pathol. Anat.,' t. 36 and 37).

[5] Ibid.

from disease, was drowned at the age of twenty-three years; and, lastly, another son, who had emigrated to Ceylon when he was sixteen years of age, was still living at the date of the report, and had shown no symptoms of the disease. The condition of one member of the family has not been recorded. The parents in this case are described as still living and healthy, the father being thin and deaf, but not otherwise infirm. In reviewing the preceding case it will be noticed that whilst the attack of the disease under consideration was strictly limited to the age of eighteen years in four members of the family, the deaths of three sons, two only of whom are said to have suffered from this form of paralysis, are referred to apoplexy at the respective ages of *forty-two, forty-four, and forty-five years.*—15. A case recorded by Duchenne[1] of a man aged twenty-two years, in whom the disease began at the age of seventeen years. His great-grandfather, his grandfather, and his father, the eldest of the family in each generation, have been attacked by the same disease, which began in the first at the age of twenty to twenty-two years, in the second at the age of twenty-four years, and in the third at the age of seventeen years. "In all of these patients the muscular lesion has attacked successively the muscles which move the shoulder, then those of the arm, and lastly the flexors of the thigh on the pelvis and of the foot on the leg." In this case the progressive muscular atrophy, localised in certain muscles, was associated with considerable obesity.—16. A case mentioned by Dr. Eulenburg,[2] in which the disease appeared in two brothers at the age of *twenty-four years.*—17. A case recorded by Dr. Heptenmacher,[3] in which a peasant of Wilhelsfeld, named Nicholas Beckenbach, and aged fifty-seven years, suffered from the disease, which began at the age of forty years, in the thenar muscles of the thumb.[4] Dr. Heptenmacher informs us that the sister of the patient, an inhabitant of the same village, at present forty years of age, suffers from the same disease, beginning in the thenar muscles of the thumb. Their mother, who died at the age of sixty-four years, had been affected from her fortieth year with progressive muscular atrophy of the entire body, whose mother and three maternal uncles (out of four uncles) suffered from similar affections, and the grandson of one of these maternal uncles is now possessed by the same malady. "All these persons first perceived the affection

[1] De l'Electrisation localisée,' 2nd edit., 1861, obs. 102, pp. 465—468.

[2] 'Constatt's Jahresb.,' 1855, quoted by Dr. Roberts, op. cit., 1858, cases 44 and 45.

[3] Op. cit., pp. 11, 12.

[4] Trousseau ('Lectures on Clinical Medicine,' translated and edited, with notes and appendices, by P. Victor Bazire, M.D. Lond. and Paris, part 2, p. 282, 1867) states that in *progressive muscular atrophy* it is the rule, to which there are exceptions, for the muscles of the hand to be first involved; "those of the thenar eminence in the very beginning, those of the hypothenar next, and the interossei afterwards."

in the muscles of the thumb, and the chief part of them at that period *when the fortieth year of life approached.*"—18. In connection with the preceding case may be mentioned that of Jacob Beckenbach, aged sixty-three years (also recorded by Dr. Heptenmacher),[1] who is apparently related to the preceding family, as the surname is the same, and the disease began at a corresponding period of life. The case of Jacob Beckenbach is, moreover, of interest from the fact that his wife has a nephew (a sister's son), Beckenbach of Wilhelmsfeld by name, who is similarly affected.

Among the remaining hereditary cases which for the most part have been imperfectly recorded, and some of which appear to have a somewhat doubtful claim to be classed with the preceding forms of paralysis, a more or less complete limitation by age may, in a few of them at least, not improbably have occurred. As in a case recorded by Cruveilhier,[2] in which two children of the same family were affected, and in which neither the sex nor the age is stated; in the cases lately referred to by Mr. Skey,[3] who states that in two families "under his cognisance" the disease commenced in infancy (showing limitation by age, which was probably well maintained), and was strictly limited to the males; and in Dr. Meryon's[4] case of a brother and sister, offspring of the father's second marriage. Besides these there are many others interesting as hereditary cases, which are commonly described with more or less vagueness by French and other foreign writers under the general title of progressive locomotor ataxy; but it is difficult, and sometimes, indeed, impossible, to recognise the exact nature of the disease of some of the cases which have been recorded. Among them is a case observed by Virchow,[5] of a man aged twenty-one years at the time of the attack, whose father had previously died from the disease, in which there might have been limitation by age, and which is noticeable as a rare instance of its direct transmission with the usual limitation by sex. A still more rare illustration of direct descent through the female line occurs in a case recorded by Marius Carré,[6] in which a woman aged twenty-six years began to suffer from the disease at the age of twenty-two years. Eighteen members of her family were similarly affected, namely, her maternal grandmother, her mother, all of her mother's kindred to the number of eight, seven children, three of whom have died from the disease, and a cousin. In a case recorded by Bamberger,[7] in which two brothers were affected with

[1] Op. cit., pp. 12—14.
[2] 'Archiv. Gén. de Méd.,' 1853, p. 601.
[3] 'Lancet,' March 10, 1866, p. 258.
[4] Ibid.
[5] 'Virchow's Archiv,' Band. vii, quoted by Dr. Roberts, op. cit., 1858, case 94.
[6] 'De l'Ataxie Locomotrice Progressive,' Thèse de Paris, 1862, obs. 1.
[7] 'Oesterreich. Zeitschrift f. prakt. Heilkunde,' vi, 7, 1860, quoted by Heptenmacher, op. cit., p. 26.

the disease, it was ascertained that they were descended from a family in which, *on the maternal side*, similar affections of the locomotive organs were common. Dr. Heptenmacher[1] relates the case of a hospital pensioner named Schumacher, aged twenty-four years, who was suffering from this disease, and whose sister had died in the summer of 1860 from the same disease. Topinard[2] has cited the case of a tanner, aged forty-seven years, in whom rheumatic affections, which began at the age of nineteen years, simulated progressive locomotor ataxy. This patient had five brothers and sisters, older than himself, who were free from disease. His father, whose age is not stated, died of apoplexy, and his mother was hysterical. Three brothers and sisters of his father, as well as one of their male children, had all four died from general paralysis with insanity, but their age is not given. The same author[3] has mentioned a case of progressive locomotor ataxy in a civil engineer, aged fifty-two years, whose maternal grandmother and paternal uncle had died of hemiplegia, which is to be noted as an illustration of the double heritage of paralytic disease. Lastly, M. Aran[4] has related two cases, in one of which an agriculturist, eighteen years of age at the time of the attack, had lost two aunts from the disease; and in the other, a sea-captain, forty-five years of age at the time of the attack, had lost two maternal uncles and a sister from the disease, and had three brothers and two sisters free from it. In such cases as these there is little information to be gleaned beyond the fact that paralytic affections, varying, perhaps, to some extent in their pathology, may occur in different members of the same family, and sometimes, it may be presumed, at the same age. But whether such cases are consequently hereditary must occasionally be considered doubtful, for I have at the present time under observation a young man who was attacked with paralysis of the lower extremities at the age of eighteen years, and two of whose male relations, belonging to two preceding generations, were also affected with paralysis, one of them being his paternal grandfather, who had hemiplegia of the right side at the age of sixty years, from which he died at the age of sixty-two years; and the other being his paternal uncle, who had paralysis affecting his speech, also at the age of sixty years, from which, after a time, he recovered. In this case it is probable that the paralysis in the first and second generations was to some extent an hereditary affection, since it was strictly limited by age, although, as both of the patients were drunkards, there was the same exciting cause of disease in each; whilst the paralysis in the

[1] Op. cit., pp. 17—20.

[2] 'De l'Ataxie Locomotrice, et en particulier de la Maladie appelée Ataxie Locomotrice Progressive,' 1864, obs. 270, p. 394.

[3] Op. cit., 1864, obs. 168, p. 236.

[4] 'Archiv. Gén. de Méd.,' 4th ser., t. 24, 1850, p. 28.

third generation was not in any degree hereditary, but due to the mechanical injury occasioned by pressure on the cord from excessive curvature of the spine, consequent on scrofulous caries of some of the vertebræ in the dorsal region.

There is another form of paralysis which is altogether limited in its occurrence to infancy or early childhood, and which is usually of a far less serious description than the preceding, as there is not the same tendency in the disease to become progressive, although fatty degeneration of the muscular tissue occurs.[1] This affection, which is commonly known as the "paralysie essentielle" of French writers, has been noticed by Rilliet and Barthez, and by Duchenne; and it has been ably and recently described by M. Laborde,[2] who refers, however, in somewhat vague terms to "the hereditary transmission of the morbid predisposition of the nervous system," as shown in three of the fifty cases on which his observations are founded. The fatty paralysis in this disease, which is circumscribed to one muscle or to one group of muscles, is almost peculiar to a very early age; and in each of the three cases referred to by M. Laborde, two of which (obs. 21 and 41) occurred in boys, and one (obs. 36) in a girl, there was limitation by sex in the hereditary transmission from parent to child; but no allusion is made to the early condition of the parents, and the inquiry does not appear to have extended to other members of the family. The cases referred to by M. Laborde, so far as regards hereditary transmission, seem to be chiefly useful in showing the tendency to transformation in nervous diseases referred to by M. Trousseau[3] in his remarks on progressive

[1] "The disease," says Trousseau, ('Lectures on Clinical Medicine,' translated and edited, with notes and appendices, by P. Victor Bazire, M.D., part 2, p. 298, 1867), "which most closely resembles progressive muscular atrophy is the atrophic paralysis of infants, but the latter differs from the former in being complicated by an arrest of development of the bones of the limb whose muscles have undergone fatty degeneration."

[2] 'De la Paralysie (dite essentielle) de l'Enfance,' 1864.

[3] Op. cit., part 1, 1866, p. 150. Among the more noticeable illustrations of such transformation, referred to by M. Trousseau, are the three following cases:—1. The case of a physician at Rouen, aged forty-five years, who suffered from locomotor ataxy in a very advanced stage, and who had an uncle and aunt insane, one brother ataxic, and another (younger) who was hemiplegic. 2. The case of a gentleman who was ataxic, whose father committed suicide, and whose two sons suffered from peculiar nervous affections. 3. The case of a man, aged eighty-eight years, who, at the age of sixty-four years, fell into a state of melancholy madness, of which he was perfectly cured. He has had three children, two boys and one girl. The eldest son is of a desponding character, but otherwise quite sound in mind. The younger son was attacked with locomotor ataxy, and died mad. The son of this last, now aged thirty years, has remained up to the present time sound in mind, but has married within the last four years, and has a son an idiot. The daughter, deficient in intelligence and otherwise somewhat eccentric, has had two sons; the eldest died mad and paralytic, the second is almost an idiot. The man (referred to above, aged eighty-eight years) had a sister who became mad at the age of thirty years, leaving a son and a daughter. The son has been hemeralopic from his infancy, and is now epileptic; the daughter

locomotor ataxy, in which attention is directed to the fact that "in the family history of patients suffering from this disease there will sometimes be found cases of various nervous diseases, which may be ascribed to a common origin." In addition, however, to such transformation, these cases of infantile paralysis appear to illustrate also the occasional tendency to the anticipation of age in hereditary disease; and they should be taken in connection with a case of temporary paralysis, hereditary for three generations, which will be more fully referred to in a subsequent part of this paper.

There remain to be noticed those hereditary cases in which the paralysis has occurred as a secondary affection, and which are subject to some extent to the influence of age. For example, Dr. Leon Tripier[1] has lately directed attention to a rare form of paraplegia consequent on cancer of the vertebral column, and mentions a case (illustrative of the hereditary influence both of sex and age) in which a woman came under observation at the age of forty-two years with scirrhus of the breast and painful paraplegia, due to secondary cancer of the spine, and died at the age of forty-six years. Her mother, who had suffered from paralysis of the lower limbs, died at the age of forty-two years, but it could not be ascertained whether she also was cancerous. In other cases of paralysis, in which the disease, being associated with and indeed dependent on disease of the vascular system, must also be regarded as a secondary affection, the hereditary influence of age has been not unfrequently observed; as in the cases recorded by Sir Henry Holland,[2] of three brothers who severally suffered from hemiplegia about the same period of life; of three brothers and a sister who "underwent an attack of hemiplegia before the age of forty-five;" and of three sisters dying in succession at the age of twenty-four years from cerebral disease with epileptic fits. In these cases the limitation by age, both with and without a corresponding limitation by sex, was well marked. Lastly, there are to be noticed those secondary forms of paralysis which result from blood poisoning, such as occurs, for example, in cases of diphtheria and other acute specific diseases, in which it may occasionally happen that two or more members of the same family are similarly affected at an early period of life, as occurred in the

died amaurotic and mad, leaving also a son who already has had mental disturbance. In addition to the preceding cases may be cited the case of a commercial traveller, aged thirty-seven years, who suffered from progressive locomotor ataxy, and whose father had for the greater part of his life suffered from epilepsy, and died in a fit (Dr. Althaus, 'Progressive Locomotor Ataxy,' p. 5, 1866); and the case of a book-keeper, aged forty-seven years, under the observation of Dr. Roberts (op. cit., pp. 32—38), in which the disease began at the age of thirty years, in the ball of the right thumb, and in which it is stated that "his father was subject to fits (probably epileptic), and died of apoplexy."

[1] 'Du Cancer de la Colonne Vertébrale, et de ses rapports avec la Paraplégie douloureuse,' obs. 10, p. 67, 1867.

[2] Op. cit., p. 43.

case of temporary paralysis already referred to as hereditary for three generations. Such cases require to be distinguished from those pseudo-hereditary cases of paralysis of the upper extremities caused by working in lead, and those numerous cases of paralysis of the lower extremities resulting apparently from a vegetable blood-poison, which have been noticed in certain districts, as in the villages on the right bank of the Jumna, in India.[1] In cases of this description it has been noticed that men are much more liable to be affected than women, owing probably to differences in their social habits and position. Whilst, apart from the influence of age, it may be observed that the local prevalence of the disease in such cases where the determining cause can be clearly recognised, favours the supposition that some endemic influence has promoted the hereditary transmission in some of those cases of locomotor ataxy and allied forms of paralysis, as of the hæmorrhagic diathesis, which have been observed to prevail in some parts of Germany and elsewhere.

The same restrictive influence of age which has been shown to prevail in many hereditary affections of the vascular and the nervous systems can be recognised also in other diseases, in some of which the effect of such limitation is often well marked. This is specially noticeable in hereditary phthisis, in which it is common for successive members of the same family to die from the disease at the same age, whilst those who remain free from it for a certain number of years usually cease to be liable to its attacks. Dr. Pollock[2] informs us that he knows "of families whose members were successively swept off by acute phthisis as they arrived at the same age;" and in his subsequent remarks on age as "a modifier of phthisis," he states that "it is common for insurance companies to regard the proclivity to phthisis acquired by hereditary tendencies to have died out at forty-five." There is need, however, of more extensive and of more accurate information on this subject, for Dr. Pollock, in common with other authorities, has observed that, "although the greatest amount of danger is past at this age, many cases occur and much mortality prevails in more advanced life."

A similar limitation by age has been occasionally noticed in those diseases which are, to some extent at least, hereditarily allied to phthisis, such as rheumatism and diabetes. With respect to the latter disease, there have been some well-marked examples recorded of its occurrence as a family defect, without any reference to the influence of age, such as Dr. Clarke's[3] case, in which six children out of twelve, in a family residing at Nottingham, died of diabetes.

[1] "Notice of a Form of Paralysis of the Lower Extremities," by James Irving, M.D., 'Indian Annals,' 1860, quoted by Dr. Meryon, op. cit., 1864, pp. 150-1.

[2] 'The Elements of Prognosis in Consumption,' 1865, pp. 95 and 254.

[3] 'Edinb. Med. and Surg. Journal,' July, 1810, p. 274.

But besides imperfectly recorded cases of this description there are others in which a very distinct limitation by age has been occasionally observed, such as a case mentioned by Isenflamm[1] in which eight children in the same family died in succession, at the age of eight or nine years, of this disease; a case mentioned by Sir Henry Holland,[2] in which three brothers were affected with diabetes mellitus under the age of ten years; a case observed by Gintrac,[3] in which a brother aged twenty years, and a sister aged eighteen years, were attacked by the disease; and amongst other cases of a similar character which might be cited is one of hereditary diuresis, which has come under my own observation, and which there will be occasion to refer to more fully in connection with the atavic transmission of disease, for the case is chiefly noticeable for the fact that the limitation by age was associated with strict limitation by sex and by atavism. The hereditary influence of age has also been observed in furunculoid affections, in which, as is well known, the urine, without being excessive in quantity, is liable to become saccharine; and among the writers by whom this hereditary influence has been noticed in such cases is Dr. Whitehead,[4] who refers to the formation of boils "in all the children of a family at particular ages," one or other of the parents "being able, for the most part, to exhibit indelible blemishes left by similar inflictions."

A corresponding tendency to limitation by age has been observed in hereditary affections of the skin and its dependencies, including the organs of special sense, and some illustrative cases have been already cited, in which it was associated with limitation by sex. In the following case of cutis pendula, which is, moreover, interesting from the rarity of the affection, age presents itself as the ruling influence, to which both sex and atavism are, as it were, more or less subordinate. The case, which is recorded by Dr. Graf, of Königsberg,[5] is that of a man, a native of Russia, who in his forty-seventh year began to suffer from relaxation of the skin of the left side of the neck, and of the lower eyelids and the parts beneath them. "The malady so increased that in a few months the skin of these parts hung in the form of sacks. He inherited the disease from his grandfather, who was attacked in his forty-seventh year, and in whom the deformity was so great that he was obliged to confine himself entirely to the house. He died at an advanced age, leaving

[1] Quoted by Gintrac, 'Memoire sur l'Influence de l'Hérédité sur la production de la Surexcitation Nerveuse,' &c., l'Académie Royale de Méd., 'Mémoires,' tom. ii, p. 197, 1845.

[2] 'Medical Notes and Reflections,' 3rd edit., 1855, p. 44.

[3] Op. cit., p. 197.

[4] 'On the Transmission from Parent to Offspring of some Forms of Disease, and of Morbid Tastes and Tendencies,' 2nd edit., p. 44, 1857.

[5] 'Wochenschrift für die gesammte Heilkunde,' No. 15, 1836, quoted in 'Med.-Chir. Review,' vol. ii, p. 568, 1836.

one daughter and two sons. The disease showed itself in the daughter in her forty-fifth year, and she took refuge in a convent in consequence. In the eldest son it showed itself in his forty-third year. The youngest, although believed to be seventy-one years old, remained free from it, but transmitted it to his son above referred to." The same influence has also been well shown in a case of hereditary crimping of the nails, which has come under my own observation, and in which the defect, limited to the female sex, was developed about the age of fifty-five years. In this case a woman, aged sixty-eight years, who had formerly possessed nails which were well-shaped, smooth, and free from ridges, observed about the age of fifty-four or fifty-five years that they became longitudinally ridged; the furrows between the ridges, which were well defined and uniform, gradually increasing in depth so as to give the nails a very peculiar appearance, somewhat similar, in miniature, to the ridged surface of a double harp shell. Her sister, aged sixty-six years, has nails which, in the same way and at the same age, became ridged like her own; whilst her brother, aged sixty-four years, has smooth and well-formed nails. Their mother, who died at the age of ninety-eight years, had the same crimping of the nails for many years, but it is not known when the peculiarity in her case began.

The restrictive influence of age has often been observed in many other local peculiarities of structure, as, for example, in hernia, which, Mr. Kingdon[1] informs us, is hereditary in about fifty per cent. of the cases which have come under his observation; and he states that, so far as could be ascertained from an "analysis of the patients (5976) with inguinal and femoral hernia who were relieved by the City of London Truss Society during the year 1863, the two following laws of inheritance seem to hold, namely, limitation by sex and limitation by age."

In addition to such cases as the preceding, in which the disease is developed at the same age in different members of a family hereditarily subject to it, there are many others in which the influence of age can also be traced, although the limits of this influence are less strictly defined. In the following case, for example, of hereditary syncope, occurring in my own practice, the frequent recurrence of the fainting fits is referable to a reflex affection, which is limited to the females in the family, and affects them only during the child-bearing period of their lives. A married woman, aged twenty years, who is four months pregnant, has suffered from frequent attacks of fainting, which came on at the end of the first month of pregnancy, and have occasionally recurred as often as three times a day. She has three brothers and six sisters. None of

[1] "On the Causes of Hernia," 'Med.-Chir. Trans.,' vol. xlvii, pp. 295—321, 1864.

the three brothers, who are respectively aged thirty-two, twenty-one, and twelve years, have had any attack of syncope; but of the six sisters, the eldest, aged twenty-six years, who has been married four years, and has had no children, began to suffer from faintings at the age of eighteen years, and they have continued, sometimes daily, to recur ever since; the second sister, aged twenty-two years, and unmarried, has suffered in like manner from faintings which began at the age of twenty years; whilst the four remaining sisters, aged respectively sixteen, ten, eight, and six years, have been exempt from such attacks. Their mother, aged fifty-four years, and who married young, began to suffer from attacks of syncope almost immediately after her marriage, but they ceased to recur when she had passed the age for having children. Bronchocele, although usually associated with endemic influence, is in like manner sometimes hereditarily limited by age. This was very noticeable in the case of a family consisting of seven children, all of whom were attacked with bronchocele before they arrived at the age of puberty, the father having been also afflicted with it at an early period of life. The family resided at Crondall, in Hampshire, and were the only persons in the place who laboured under it.[1] And without any special reference to age, Dr. Crawford[2] has in like manner cited evidence to show that this disease "is often continued in families by inheritance;" as in a case which came under his own notice of "a woman with goître, whose grandmother, father, paternal aunt, and cousins, also had it, although they did not live in the same place, and no other person in their neighbourhood was affected with the disease."

It is important to notice in connection with this subject that the various modifications of disease are closely connected with the influence of age, and that it is often and to a great extent a determining cause of the so-called transformations which occur in the hereditary transmission both of non-malignant and of malignant disease. The substitutions which have been effected in the transmission more especially of malignant and benign tumours, are of comparatively frequent occurrence, and they occupy an important position in the history of hereditary disease; but they have, as yet, been very imperfectly investigated, and, notwithstanding the importance of the inquiry, they seem to have been glanced at rather than studied. Among the various cases of hereditary tumours of a non-malignant character which illustrate the influence of age, is the following, which has been cited by M. Broca,[3] to show "that certain tumours due to a disturbance of nutrition, spontaneous but entirely local, may be developed in the same organ, in many members of the

[1] 'Thomas's Practice of Physic,' 11th edition, vol. ii, p. 337.
[2] 'Cyclop. Pract. Med.,' art. "Bronchocele," vol. i, p. 325.
[3] 'Traité des Tumeurs,' tom. i, 1866, pp. 156-7.

same family." A lady affected with adenoma of the breast from the period of her first pregnancy to her death at an advanced age, had a family consisting of three daughters, all of whom were similarly affected about the same age. Dr. Murchison[1] has recorded a case of multiple fatty tumours in two sisters, which began to appear in the one sister at the age of sixteen years, and in the other at the age of twenty years. Their father, aged sixty years at the time of his death, had tumours similar in appearance and position the greater part of his life. M. Schiffner[2] has related a case of multiple neuromata in two brothers who died affected with the disease, one at the age of thirty-four years, and the other at the age of thirty-three years. Whilst Mr. Paget[3] has recorded a case of symmetrical osseous tumours in a father and son, which began in early childhood, and in which the influence of age was associated with atavism, for it was ascertained that, on the father's side, "four cousins—one female and three male—children of his mother's sisters," were similarly affected.

Among the remaining cases to be noticed in which the hereditary influence of age can be more or less fully recognised is cancer, which is apt, especially when it retains the same character and affects the same organ or tissue, to occur in successive members of the same family, either at the same or at a retrogressively earlier age. Both the limitation by and the anticipation of age have been so often observed in cancer, and they are so completely in accordance with what commonly occurs in the development of other forms of hereditary disease, as well as with many well-marked cases of the hereditary maintenance of health and the hereditary prolongation of life, that it is difficult to imagine how any one could be induced to refer to the restrictive influence of age in this disease as an argument against its hereditary transmission. Yet, notwithstanding the recognised fact that certain forms of cancer are peculiar to a middle or to an advanced period of life, it has lately been alleged by a writer on the subject,[4] who has had special advantages in observing this disease, that cancer ought not to be regarded as hereditary unless, as a rule, it affected the members of succeeding generations during the period of infancy; for "were it inherited [he remarks] it should be most common while the influence of parental structure and character is yet strong over the offspring, and it should diminish in frequency with the augmenting independence of the individuals. The cancer of infants should be the most numerous in the community, and the proportion of it should regularly lessen with advancing age." The subsequent admission by this writer, that "the reverse

[1] 'Edinb. Med. Journal,' vol. ii, 1857, pp. 1091-3.
[2] 'Mém. de la Soc. de Chir.,' tom. iii, p. 285, 1853.
[3] 'Lectures on Surgical Pathology,' vol. ii, pp. 244-5, 1853.
[4] 'The Antecedents of Cancer,' by Charles H. Moore, 1865, pp. 22, 23.

of this expectation is the fact," contains a conclusive argument in favour of the inheritance of the disease, and is moreover in accordance with the popular belief on the subject; for the overwhelming dread of cancer which prevails in certain families liable to its recurrence is chiefly due to the frequency with which it has been developed at a late period of life in the offspring of those who have died from the disease at a corresponding age. There is, however, both good as well as evil to be derived from this restrictive influence of age on the development of the disease; for although limitation by age serves to strengthen the hereditary character of cancer, yet the effect of this influence is so long postponed that it should, like the silver lining of a cloud, inspire hope rather than dread; since it is obvious that the inheritors of those who have died of cancer at an advanced age must in other respects have been more than usually favoured, so as to escape, not only in infancy, but also in youth and mid-age, those diseases which shorten life. It is satisfactory to observe that this hopeful view of the influence of age has been fully recognised by some even of those whose opinions on hereditary transmission are not altogether orthodox; and it may be useful, moreover, to notice that in a case of cancer lately recorded by M. Broca,[1] and which was hereditary for four generations, special attention has been directed to the fact that the sixteen persons who were affected by the disease lived altogether 783 years, which gives them an average life of forty-nine years; and that as the average life of the inhabitants of France is at present thirty-five years, and at the beginning of the century was only twenty-nine years, it follows that there was an average gain of from fifteen to twenty years in each of these cancer patients, compared with "the amount of life devolved to each Frenchman." With respect, therefore, to the inheritance of cancer being contingent on its recurrence during infancy, it will be sufficient in reply to state that, besides the evident impossibility of any direct repetition of hereditary transmission in the case of a mortal disease subject to such conditions, the attempt to establish a principle of this kind would be opposed to the developmental phenomena, not only of animals, but also of plants. For the time which it is often necessary should elapse before the evolution in succeeding generations of an hereditary disease, which appears in consequence at a given age, is needed also for the completion of those changes in the development of plants, in consequence of which each after its kind flowers at an appointed time. Hence, in accordance with a well-established principle in development, which induces either a recurrence to the normal type or an extinction of race, it may be urged that it would be quite as unreasonable to suppose that all transmitted disease generally, or that all the varieties of cancer

[1] Op. cit., tom. i, pp. 151-3, and 337.

specially, should, regardless of the age at which the parents had been affected, appear in the descendants during infancy, as it would be to accept the apocryphal hypothesis of Milton that, in the beginning, plants of all kinds burst into flower on the same day.[1] For, like the infantile development of the hereditary cancer of advanced age, such an unseasonable display of eccentricity in plant life would involve the necessity of our assuming that a large proportion of the plants belonging to the first generation could not have been perfect or typical, in thus flowering before the time, nor could there have been at this early period in the history of the world any proper seed-time or harvest; unless, indeed, we are prepared also to assume that afterwards, by a universal inversion of the order of nature, each plant after its kind has flowered only at certain times and seasons, in accordance with those peculiarities of race which, subject to variation dependent on endemic and other influences, have since become hereditary, and which it might reasonably be inferred that each would have derived from the first instead of from a subsequent, if not from an abnormal, generation of plants.

It must not, however, be expected that this or any other restrictive influence in development would be equally displayed in all cases without exception; for as disease is from first to last essentially connected with abnormal change, it would be but reasonable to suppose that a condition which is often exceptional in its origin should not also be sometimes exceptional in its course; and, therefore, in drawing attention to the fact that limitation by age is a firmly established principle in reproductive development, it must at the same time be noted that on many occasions, when there is even a strong tendency to this restrictive influence, some disturbing cause appears to interfere with the reproduction of the disease, in consequence of which the time for its development is either anticipated or deferred.

With respect to the anticipation of age in hereditary disease, there are some important facts connected with the subject which it will be useful to notice at this stage of the inquiry, for the occurrence of anticipation is not only an unfavorable sign as regards the relative soundness of the organisation in which a transmitted disease can thus appear before the appointed time, but the possibility of its occurrence leads to the inference that in some of those cases in which two or more members of the same family have been thus affected by the same disease at the same age, without any corresponding disease having been developed in the preceding generation, the difficulty of accounting for such an occurrence might sometimes be referred to the death of the parents previous to the age suited to the development of the disease; and that had the parents in such a case survived,

[1] "Then herbs of every leaf, that sudden flower'd."—'Paradise Lost,' book vii, l. 317 (founded on 2 Esdras, vi, 43, 44).

the disease might have shown itself in them. Whilst even without any such equality of age as regards the development of disease in different members successively affected in the same family, there is evidence to prove that this phenomenon of anticipation may also be an occasional cause of difficulty in tracing back the course of the transmission; for the offspring who have become affected at an unusually early, although not strictly the same, age, may in like manner have received the morbid inheritance from a parent or ancestor who has either been subsequently affected or has died previous to the age at which the recognised development of the disease might have occurred. That this is no fanciful assumption can be proved by the fact that in some diseases, and especially in insanity and consumption, children born before any symptoms of the disease have shown themselves in the parents have, as is well-known, shared by anticipation the morbid inheritance. With reference more especially to insanity, it is now very generally admitted that anticipation of age occasionally occurs in the hereditary transmission of the disease. Morel[1] informs us that it is a "fact which deserves to be distinguished," that "madness may remain in a state of incubation in the parents, and that the children may first be struck with the malady. All physicians to asylums are able to confirm this fact. It has occurred to them to receive first the children as insane, then the parents have been ultimately placed." Gintrac, Prosper-Lucas, Girou, Foville, and many other writers on hereditary disease (including some who have not yet been quoted in these papers), all agree on this subject, and cite cases in which it has occurred.

In an inquiry into the causes which may influence the progress of hereditary disease, it is obvious that this anticipation of age is important as one of the means by which the further transmission of hereditary disease in a family is checked; for when, as is not unfrequently the case, the development of the disease occurs at a retrogressively earlier period in each succeeding generation, there is a consequent tendency to its disappearance by extinction of race. The progress of hereditary phthisis, for example, is sometimes arrested in certain families by the disease occurring at so early an age that there cannot be any offspring to succeed to the inheritance. M. Recamier[2] relates an interesting case in which this anticipation of age, associated with limitation by sex, occurred in the family of an old lady who died of phthisis at the age of eighty years, and whose lungs were found, after death, studded with miliary tubercles; her daughter died of tubercular phthisis about the age of fifty years, and the daughters of this last patient died of the same disease before their twenty-second year. "I should have," adds M. Recamier, "but the difficulty of choosing in order to multiply analogous examples." The same

[1] 'Traité des Maladies Mentales,' note, p. 116, 1860.

[2] 'Recherches sur le Traitement du Cancer,' tom. ii, p. 38, note.

writer[1] relates a well-marked case of cancer of the stomach, which was hereditary for three generations, with corresponding anticipation by age associated with limitation by sex. In this case the male ancestor, who died of cancer of the stomach at the age of seventy years, had two sons, the eldest of whom died of the same disease at the age of thirty-eight years, whilst the other son, who emigrated to Brazil, was free from the disease at the age of fifty-one years. Among the numerous grand-children by the eldest son, the first born was a male, who from the age of twenty or twenty-two years suffered from obstinate affection of the stomach similar to his father; the second, also a male, died from cancer of the stomach at the age of twenty-two years; the third was a daughter, aged, at the date of the report, twenty-two years, whose stomach was merely so far affected as to be often "out of order;" and there is a vague account of stomach ailments in the remaining offspring, who were under age. It may be interesting with reference to this case to state that the hereditary occurrence of cancer of the stomach in connection with anticipation of age possesses some historical importance from the fact that the disease was fatal to Napoleon Bonaparte, who with his sister Caroline died from it at an age earlier than that at which it had proved fatal to the father, from whom it was derived.[2]

The anticipation of age in the recurrence of hereditary disease in the preceding class of cases is to some extent an indication of progressive degeneration; and when, as is not an unfrequent occurrence,

[1] Op. cit., pp. 48, 49.

[2] The evidence in favour of the frequent anticipation of age in hereditary cancer is more complete than in the case of any other hereditary disease; for, in addition to the cases referred to above, Mr. Moore (op. cit., pp. 18 and 19) informs us that Mr. Paget's observation that cancer appears "to increase in intensity with repetition," so as to begin "at an earlier age in a second generation than in that from which it was derived," "corresponds with what has been noticed at the Middlesex Hospital." This was observed to be the case more especially where the disease affected the same organ in persons of the same family; and in consequence of this a parent and child were sometimes found to be suffering from the disease at the same time. "Mr. Nunn (we are informed) had under his care a mother and her daughter suffering together from cancer of the breast, and Mr. Lawson attended a young man with cancer of the stomach whose father was at the same time a patient of Dr. Budd's for similar gastric disease. In a family referred to by Mr. Sibley a daughter died at thirty-two and her mother at fifty-two of cancer of the uterus. Mr. Paget's observation extended to an additional generation, and the grand-daughter died of cancer of the uterus at an earlier age than the mother, as she in her turn had died of the same disease earlier in life than the grandmother. From Mr. Sibley's report, I find that the remarkable Middlesex Hospital case illustrates the same fact, the five daughters being all attacked at an earlier age than their mother with cancer of the left breast. And it can be further made out that the malady came on in each at an earlier age than in the one next older than herself. It thus appeared to increase in intensity in the children as they were born nearer the time when the disease broke out in the mother; and it happened that the eldest of the five was attacked with cancer subsequently to its appearance in the second, third, and fourth of her sisters."

hereditary and endemic influences are associated together in the same case, it is difficult, if not impossible, to distinguish the one from the other. Thus, in districts where bronchocele is localized within very narrow limits, it has been noticed that not only the hereditary influence of age but also of sex is apt to be superseded by endemic influence; and hence the occurrence of anticipation of age in the subsequent development of the disease is due to the endemic rather than the hereditary influence. This is well shown in a case (referred to by Dr. Alexander Coventry[1], late President of the Medical Society of the State of New York) of bronchocele developed by drinking the water of Regel's Creek, in which not only were the male and female settlers in that locality equally liable to the disease, but the sheep also suffered in the same way and from the same cause; for Dr. Coventry records that "about the autumn of 1802 I put a small flock of sheep into a pasture through which the stream ran; next spring one lamb proved goitrous; the succeeding season every lamb had a swelled neck, and seven out of eight died. . . . My next neighbour had sheep in an adjoining pasture, which was watered by a spring; his sheep had no distemper. My flock has been kept where they do not get to the Creek water, and are now free from the disease."

This tendency to anticipation of age may apparently be traced in all forms of hereditary disease, and among the examples of its occurrence in hereditary affections of the vascular and the nervous systems (which have been specially referred to in connection with the influence of age) may be mentioned a case, under my own observation, of organic disease of the heart, which was fatal at the age of eighteen years to two sisters, whose mother died from disease of the heart at the age of sixty-four years; and the following case, for which I am indebted to a distinguished physician, of temporary paralysis, which was hereditary for three generations. The ancestor from whom the inheritance in this latter case had been received was a man who, at the age of forty years, had paralysis of the arm after fever (typhus); this paralytic attack was completely recovered from in six months, and he lived to the age of sixty-nine years. In the second generation the second and third sons, and the third, fourth, and fifth daughters, all suffered, in early life, from temporary paralysis. The second son, at the age of thirteen years, had paralysis of deglutition for three months after an attack of scarlet fever (diphtheria?); and again, at the age of twenty years, he had paralysis of one finger for three months after an attack of typhus; he is now, at the age of thirty-eight years, in good health. The third son, when an infant, was paralysed for many months in both lower extremities; and again, at the age of eight years, was paralysed for nearly twelve months, also in both lower extremities, after an attack of scarlet fever

[1] 'New York Medical and Physical Journal,' June, 1824, No. 10.

(diphtheria). The third daughter, at the age of seven years, was paralysed for nearly twelve months after scarlet fever; she is now, at the age of thirty-six years, in good health. The fourth daughter, at the age of thirteen years, had paralysis of one arm after typhus (probably typhoid) fever; the arm was subsequently drawn up, and she died with convulsions about four months afterwards. The fifth daughter, at the age of twelve years, had paralysis of both legs after fever (typhoid?); the right leg recovered at the end of six months, but the left leg was drawn up, and, in consequence, after three years, the tendons were cut; she is now, at the age of thirty-four years, alive and well, with the exception of the left leg being only about half the size of the other leg. It was moreover noticed in this generation that the paralysis in the third and fifth daughters was for many months associated with chorea; and that the eldest daughter, who is now about forty years of age, and always free from paralysis, has for a long time been subject to epilepsy. In the third generation temporary paralysis was developed, at the age of twelve months, in two female children of the fifth daughter referred to in the preceding generation. In the elder of these children there was paralysis of both legs, which continued for six months, and now, at the age of three and a half years, the right leg is much weaker than the left, and there is a tendency to club-foot; in the younger child, now aged two and a half years, there was for two or three days very slight paralysis of one leg.

But it is chiefly in hereditary affections of the skin and its appendances, including the organs of special sense, that such illustrations of anticipation are most commonly noticed, and they are to be met with even in the transmission of very trivial peculiarities of these structures. In a case, for example, which came under the observation of Dr. Underwood,[1] it was remarked that the hair of all the children in one family, "from a very dark colour became exceedingly light, and in one very white, while under two years of age." The mother of these children was born with very dark hair, which, when she was about five years old, became very white, but it subsequently changed to a dark colour again. Among the cases, however, belonging to this class, it is chiefly in affections of the eye that anticipation of age has been most commonly observed, and there are numerous illustrations of it to be met with amongst writers on the subject. Prosper-Lucas[2] mentions the case of a family in which hereditary amaurosis, associated with limitation by sex, occurred in this way at a retrogressively earlier period for three generations. In this case the grandmother had amaurosis at the age of thirty-five

[1] 'Treatise on the Diseases of Children,' tenth edition, with additions, by Henry Davies, M.D., 1846, p. 594.

[2] 'Traité Philosophique et Physiologique de l'Hérédité Naturelle,' tom. i, pp. 400, 401, 1847.

years. Her daughter was deprived of sight by the same affection at the age of nineteen years; she married and had a family of seven children, five daughters and two sons. The eldest of these children was a girl, who became amaurotic at the age of thirteen years, and died at the age of fifteen years, totally blind; the second, a daughter, lost her sight at the age of eleven years; the third, also a daughter, lost her sight at the same age of eleven years; the other children, two of whom were daughters and two were sons, were free from the disease; one of these daughters died at the age of two years, and the other was living at the age of thirteen years; whilst the two sons were living, one aged three years, and the other one year. M. Dumont[1] relates a corresponding case which occurred in a family named Brunet, many of the members of which had been inmates of the Quinze-Vingt Hospital in Paris, and which possesses additional importance from the fact that, although the anticipation of age was associated with a double inheritance of the defect, yet, in consequence of there having been on the mother's side atavic transmission from male ancestors, it was, as in the preceding case, associated with limitation by sex. M. Dumont informs us that "the father became blind at the age of thirty-five years, and died in the hospital. The mother was not blind, but it is certain that her ancestors had been so, for her father and her grandfather died blind at the old Quinze-Vingt Hospital in the Rue St. Honoré. The two sons, issue of this marriage, became blind at the age of twenty-five years. One of them had always had very feeble sight. Each of these two sons had a son, and these two children became equally blind, the one at the age of twenty-one, and the other at the age of twenty years. The family [adds M. Dumont] is now extinct, but it seems that the blindness followed in each individual an increasing progression, in that it supervened in the children at an age less advanced than in their parents." M. Dumont[2] has also recorded another case of hereditary blindness in some respects similar to the last, but in which there was not limitation to one sex. In this case the grandfather was blind, but it is not stated at what age he became so. The father was completely blind at the age of fifty-eight years, and he had blind aunts, whose ages are not mentioned, whilst his wife is stated to have "bad eyes." Of two children born of this marriage, one, a son, died at the age of forty-one years, "attacked with incomplete blindness;" and the other, a daughter, became completely amaurotic at the age of forty years. Neither of them left any children.

On reviewing these three cases of blindness, and there are many similar cases which might be cited, it will be noticed that the anticipation of age refers to the members of successive generations, and not to successive offspring belonging to the same generation, for in

[1] 'Recherches Statistiques sur les Causes et les Effets de la Cécité,' Paris, 1856 p. 87. [2] Ibid., p. 88.

their cases the limitation by age was strictly observed, the disease having occurred in the concluding generation of the first case at the respective ages of thirteen, eleven, and eleven years; in the second case at twenty-one and twenty years; and in the third case at forty-one and forty years. This limitation by age in the development of a disease which is characterised by anticipation of age in its transmission, shows that just as local arrests of development occur in different members of the same family at the same fœtal age, so corresponding arrests of nutrition or of nutritive reproduction in an organ or tissue may likewise occur in different members of the same family at the same period of air-breathing life; the result being referable to the same phenomenon of arrest, which in the one case is associated with development, and in the other with nutrition. This pathological evidence of the connection between development and nutrition may perhaps be more clearly recognised in such cases as the following, in which anticipation of age occurred in consequence of the ancestral disease being developed in the offspring as a corresponding congenital defect. In this case, which was observed by M. Lusardi,[1] of Lille, a man named Vathiau, of the Commune de Marbaix (Nord), was affected with cataract at the age of thirty years, and all of his children were born with the same infirmity. Such cases as the preceding are not uncommon, and, whether the anticipation of age refers to definite periods of fœtal or of air-breathing life, they serve to illustrate the extent to which the influence of age may affect hereditary disease.

With respect to the postponement of hereditary disease, it has been customary with those who have bestowed some attention on the subject to refer to it as though it were simply the antithesis of the preceding condition, and therefore favorable in an inverse relation to the occurrence of anticipation. But whilst admitting that anticipation in such cases is always to be looked upon as an unfavorable sign, it must also be noticed that there is often something of far more relative significance in the postponement of hereditary disease, as it usually shows that the variation from the normal standard of development, which has hitherto been apt to occur at a certain age, may be superseded by a sounder organisation, in consequence of which it may be expected that the disease will hereafter fail to appear when the time for it to do so has passed away. So that, apart from the consideration of its atavic transmission, the inheritance on these occasions is to be regarded as not simply deferred, but lost.

The changes which take place in the organization during the lapse of a certain period of time, in those cases in which an expected disease has not been developed, have the effect of checking its recurrence; and hence it follows that there are but few cases of this

[1] 'Journal Universel des Sciences Médicales,' t. xxv, p. 127, 1822.

kind on record, compared with those in which a transmitted disease has appeared before its time. The hygienic importance of this fact in the development of hereditary disease has not, as yet, been recognised; but some practical, although unconscious, advantage has been taken of it in many cases of consumption, in which it has been customary to send the unfortunate inheritors of the disease to another climate, in the hope of prolonging their existence; and, notwithstanding that the departure of the patient has generally been deferred till after the development of the disease, yet even in some of these cases it has been followed by so complete a change in the system that, in addition to the arrest of present symptoms, there has also been a complete check given to any after appearance of the disease. It is impossible, however, to estimate at present the practical value of such principles in the treatment of hereditary disease, as there is no recorded series of cases in which the members of a family liable to suffer from the development of phthisis, or other disease, at a certain age, have, in accordance with this encouraging fact in hereditary transmission, been subjected to the influence of a change of climate, in anticipation of the age at which in their family the disease has been previously observed to occur. Unhappily, as regards this country, it must be admitted that very little attention has been bestowed on the hereditary transmission of disease, and much valuable information which might be gathered on the subject at our public medical institutions has been allowed to run to waste; so that whilst there are for the most part copious and important records of the diagnosis and after-treatment of disease, there are no records of its family history to which either present or future inquirers can turn for useful information respecting the course and limits of hereditary transmission. Yet there are perhaps few subjects in medicine which have stronger claims on our interests as well as our sympathies; for, independent of any ties of kindred and affection, the importance attached to an individual life in some families, in connection with succession to title or estate, and especially in those cases, which are far from uncommon, in which, owing to phthisis, paralysis associated with fatty degeneration of the voluntary muscles, certain forms of hæmorrhage, &c., the males have been observed to die off in succession at an early age, ought to secure for this subject the fullest investigation. In some of these cases it would, perhaps, hardly be allowable to hope that disease could be prevented, or life to any considerable extent prolonged; but in others a more favorable prognosis might be claimed, if the course needed to ensure success were rightly understood and pursued. The object to be obtained in such a case being, not to cure disease, but to prevent its occurrence by delay; which even a difference of climate could scarcely be expected to accomplish unless a wide margin of time be allowed for in this attempt to tide over the

critical period of life.[1] For even where the removal of the patient has been in anticipation of the disease, a certain number of years would usually be required to elapse before his condition could be pronounced safe, the effect of climate in superseding the morbid tendency in such cases being consequent on the influence of time in changing the organisation. The extent of the change which may be effected in these cases has been well illustrated by M. Estève,[2] who, in some useful observations on age in connection with hygiene, remarks that, if the work be undertaken early and pursued with perseverance, the original constitution of the individual may be modified, the tendency to any particular chronic affection overcome, and a new physiological state produced; and he cites a case related by Ettmuller of a man who, having seen his father, mother, and three sisters die of phthisis, escaped their fate by continually travelling, the result of which was that his constitution acquired a power "capable of surmounting a terrible predisposition." M. Estève adds that "one often sees in the world persons doomed to an anticipated death, or at least to a barren repose, elude in some degree the fatal arrest of nature, and pursue with distinction useful careers," by attention to hygienic precautions. For under improved conditions of life there is often observed to be an increased tendency in each failing organ or tissue to return to a better state of health; and when the development of an hereditary disease has been thus checked, the risk of its appearance in after years will depend partly on the extent to which the disease itself may be subject to the restrictive influence of age, and partly on the success which may attend those ever recurring efforts at reconstruction which in each organ or tissue are associated with the attempted renewal of its youth.

There are some conditions of the system in which, however, the postponement of an hereditary affection cannot be regarded as desirable or even safe. This is very noticeable in those cases in which, owing to delay in the occurrence of some form of hereditary hæmorrhage, disease of a more or less severe and even fatal character is the result. For the abnormal condition of the vascular system in such cases of delayed or suppressed hæmorrhage not unfrequently causes, by reaction, disease of the nervous system. The importance of this

[1] Besides the favorable influence in these cases of a change of climate, attention may be directed to the alleged good effects of bleedings in anticipation of the hereditary development of consumption, as this disease is often associated with or preceded by vascular flushing of internal organs. Boerhaave and Van Swieten have been cited in favour of this preventive treatment, and it is probable that in those cases more especially in which hæmoptysis attends the ushering in of the disease, such anticipative bleedings might sometimes delay if they did not altogether arrest the hereditary development of the disease (Richard de Nancy, 'Traité Pratique des Maladies des Enfans,' pp. 13, 14, 1839).

[2] 'Considérations Générales sur les Ages, étudiés dans leurs rapports avec l'Anatomie, la Physiologie, la Pathologie, et l'Hygiène,' 1859, pp. 61, 62.

fact in connection with the hereditary occurrence of hæmorrhoidal fluxes at different periods of middle or of advanced life, and the danger which is often associated with any exceptional delay or omission in their appearance at the customary age, have to some extent been recognised. But there are other cases less common, and liable therefore to be sometimes overlooked, in which mischief results from the non-appearance of an hereditary hæmorrhage in early life. As, for example, in some cases of hereditary epistaxis in which, when the blood hereditarily determined to the peripheral structures does not thus escape, temporary congestion of the brain, or of some other internal organ, is apt to ensue. Duchamp[1] has recorded a case of this description in which epilepsy was so developed in a girl, aged twenty-one years, whose mother and sisters had been subject to hereditary epistaxis from which she herself had remained exempt, menstruation having moreover been delayed in her case till the age of eighteen years. Sometimes, independent of suppression or delay, the approaching occurrence of even a customary flux has been sufficient to develop disorder in the nervous system hereditarily associated with limitation by age, as in a case recorded by Baumes,[2] of a lady whose arms were agitated by convulsive movements at the approach of the menses, and who transmitted this affection to her daughter. Whilst, on the other side, exceptional cases may be quoted in which the postponement of a customary flux, such as menstruation, has been, through some hereditary peculiarity in development, unattended with mischief of any kind, as in a well-known case observed by Morgagni (epistle 47), who informs us that he "knew a noble virgin, who, being married before her menses, which had been expected for many years, appeared, was nevertheless very fruitful; and that we may be the less surprised thereat, the very same thing had likewise happened to her mother."

Before passing from the consideration of the effect of postponement on hereditary disease limited by age, it will be useful to refer to its occurrence in cases of non-hereditary disease of a periodic character, since the latter class of diseases appears to bear the same or at least a corresponding relation to the daily life of each individual in whom such disease occurs as the former does to the collective life of all the members in the family affected. For in the recurrence of an hereditary disease limited by age the phenomenon of periodicity seems to be simply transferred from the narrow limits which mark the daily history of a single life to a wider range; and although a lifetime instead of a day is needed to complete the cycle of organic change, yet in both cases the same restraining principle influences the recurrence of the disease, whether it be developed at the same hour in each day or at the same age in each life. More-

[1] 'Maladies de la Croissance,' 1823, pp. 31, 32.

[2] 'Traité des Convulsions dans l'Enfance,' 2nd edit., 1805, p. 7.

over, also, as will be noticed at greater length in the concluding paper of this series, departure from the usual order of recurrence may not unfrequently be observed in which an hereditary disease passes over one or more generations, just as a periodic disease passes over one or more days; and when, as sometimes happens, these interruptions for a lifetime or for a day occur so regularly that what had been previously regarded as an exception becomes, as it were, a rule, it is but reasonable to infer that they are subject in like manner to a restraining influence which is the same in principle for both, and that the occurrence of atavism in connection with the restrictive influence of age in a family life is analogous to what is of such common though exceptional occurrence in the periodic phenomena of a daily life.

In like manner also the hereditary freedom from disease throughout a long life which is closed by a death correctly referred to old age admits of being similarly explained and illustrated. For whilst the limitation by age in the appearance of an hereditary disease expresses the result of this restrictive influence on development, in the same way that the periodic recurrence of decay in the vegetable world shows the influence of the seasons on plant life, so the effect of the same restrictive influence can often be traced in cases of hereditary exemption from disease and decay, and the consequently hereditary prolongation of life.

Dr. W. A. F. Browne[1] has remarked, with reference to insanity, that whilst in many families there exists a tendency to premature decay of the mental powers, there are also whole families remarkable for extreme longevity, and in which "there occurs no dotage previous to death." Prosper-Lucas[2] has in like manner made some instructive observations on the effect of hereditary influence on the duration of life; and the extent to which this influence sometimes prevails is, moreover, well shown in some cases recorded by Dr. Andrew Wynter, in an article on "Longevity," lately published in 'Good Words' (July 1st, 1865), among which occurs the well-authenticated case of four sisters, who lived to the respective ages of 107, 105, 100, and 103 years, and the last of whom died some years since in the Edgeware Road.

In conclusion, it may be remarked that it is to some extent due to this restrictive influence of age that hereditary disease is not more commonly recognised; for, generally speaking, the lives of medical men, after securing a full practice, are too short to allow of their knowing what the offspring of many of their patients will suffer from on arriving at the same period of life as that at which the parents had received an indelible impression of disease. When, however,

[1] "Some Notes upon the Hereditary Tendency to Mental Disease," 'Phrenological Journal,' vol. xiv, p. 228, 1841.

[2] Op. cit., t. i, "De l'Hérédité de la Vie," pp. 254—291.

medical observers have been so far favorably situated as to be able to compare notes on this subject, which sometimes happens in the case of a few superintendents of public lunatic asylums, whose term of office has been unusually prolonged, it has generally been observed that age possesses an important influence in restricting hereditary disease; and notwithstanding the well-known disposition to conceal the family history of insanity more than of any other form of disease, yet there is at the present time no hereditary disease in which the restrictive influence of age has been more often traced and more clearly shown to prevail. This result in cases of insanity is not, however, dependent altogether on the life of the observer whilst in office being unusually prolonged; for in addition to the personal experience of those who have had the opportunity of attending in their old age the children of their former patients, there must also be taken into account the many valuable and accessible records of cases in which the occurrence of insanity has been carefully registered, and which help, therefore, to extend what would otherwise be a very limited acquaintance with the past history of the disease. The same restrictive influence has also been observed and recorded in many cases of hereditary phthisis and of hereditary cancer since the establishment of special hospitals for these diseases; and although hospitals for the two last-named diseases will, in consequence of the wider area from which they receive, be in some respects less available for tracing the family history of disease than lunatic asylums which are supplied from a single county, yet each, as the record-office of a special form of disease, may, if well directed, contribute to increase our knowledge of the nature and extent of hereditary transmission. For the information obtainable from such sources will be valuable in connection with the hereditary limitation of disease by sex and atavism as well as age, both of which seem to admit of being also referred to the restrictive influence of time on reproductive development. To some extent this has been already recognised as the determining cause of the difference of sex, apart from sexual limitation in hereditary disease, and there need be no hesitation in allowing that it is also intimately associated with the occurrence of atavism; for with respect to the latter, which will form the subject of the concluding paper of this series, it can be satisfactorily proved that the interruptions which occur in the development of hereditary disease do not result, as is very commonly supposed, from any ill regulated tendency in disease to "leap at random" in the course of transmission; but from a more or less well-defined and restrictive influence, which, instead of being simply limited to one sex or to one age in the same family or in successive generations, is associated with a wider cycle of change. Whilst the restrictive influence of age in connection with the limitation of hereditary disease to one period of life, as well also as its anticipation and postponement, can

in like manner be often very fully illustrated from the same source; for although exactness in the limitation may not be generally maintained, since even the disease itself is liable to undergo a change in the course of its transmission, yet there are many families in which, as Prosper-Lucas has forcibly remarked, "disease has but one form, and death has but one age."[1]

Art. II.

The Practical Medicine of Galen and his Time. By Joseph R. Gasquet, M.B. Lond.

In spite of the valuable additions which English physicians of this generation have made to the history of ancient medicine, there was certainly never a time when the Greek and Roman physicians were less known to their successors. One reason for this is that, while the general standard of education has been very much raised by preliminary examinations, fewer scholars enter our profession, and this is, perhaps, matter for regret; but the principal cause lies deeper, and is one of which we may be proud. The rapid progress of all the natural sciences about the end of the last century has resulted in a revolution in medicine as complete and sudden as that political change which preceded it in France. There is a connection of method (which is the soul of doctrine) between Hippocrates or Galen and Sydenham or Bordeu. There can be none between us and those great physicians of England and France. We have drifted so far from the ideas of our predecessors that we can scarcely realise that Avicenna was seriously commented on in the schools at Montpellier not fifty years since just as it requires a mental effort on the part of the most conservative Frenchman to comprehend the political and social atmosphere in which his grandfather lived.

Not only is this revolution an inevitable result—a "fait accompli"—but it is one which all of us will at once admit to be salutary. Without, therefore, wishing in the least to be reactionary, I cannot help thinking that a little more general study of the father of medicine and of his greatest follower might prove very beneficial. We are in no danger of falling into most of their faults, but we can never have held up before us too often the virtues which distinguished them as true physicians; and reading their works has the

[1] The author of the preceding papers on hereditary disease would be thankful to receive any cases illustrating the influence either of sex or of age on hereditary transmission, and also cases in which there is evidence to show the occurrence of atavism or interrupted descent. Address, William Sedgwick, Esq., 12, Park Place, Upper Baker Street, London.

same effect in medical education as travel has in the training of a man of the world, teaching lessons of tolerance and broadness of mind, which are virtues in which the greatest medical authorities are occasionally a little deficient.

Every one has an opportunity of studying Hippocrates, but the works of Galen are almost necessarily closed to the general reader. Their extreme voluminousness would render the study of even part of them a great labour, and the theoretical physiology which Galen founded on Hippocrates is so thoroughly interwoven with even his most practical works as to make their perusal tedious. It is, perhaps, owing to both these causes that, while we have an excellent translation of Hippocrates from the hand of an admirable scholar, we have no English translation of any part of the works of Galen.[1] There is, indeed, a very meager abstract or table of contents of his works, published by Dr. Coxe, an American physician;[2] and in the strongest possible contrast to this work there is the lively and interesting paper by Dr. Chambers, in the twenty-second volume of this Review, on "Bloodletting in the Olden Time."

Although I do not flatter myself that I have been very successful in the attempt, I have ventured in the following pages to follow Dr. Chambers's plan, extending it so far as to give a short account of the practical treatment of the sick by the great Pergamene physician. I shall endeavour to avoid all theoretical questions which could have no interest for any but the learned, and shall deal almost exclusively with those points of treatment which have a bearing on modern medicine.

Among the general principles of therapeutics perhaps the most important and the most frequently stated is, that health and disease are relative and individual terms, depending on the habitual condition of the person spoken of,[3] and that for treatment to be correct we should know, not merely physiology in general, but also the healthy state of the patient before us,[4] so that our treatment should depend, not upon what disease is before us, but upon the previous state of the patient and the affected part, and upon the special character of the disease; in short, that each case should be treated in and by itself.[5]

The physician, he also frequently tells us, is the spectator at the bedside of every patient of a life and death struggle between nature and disease, in which he is himself to take an active part.[6] He

[1] Gale's translation of a small portion of Galen's works, published in 1536, can hardly be considered available at the present day.

[2] 'The Works of Hippocrates and Galen Epitomized, from the Latin Translations,' by J. R. Coxe, Philadelphia, 1846.

[3] 'San. Tu.,' i, 1.

[4] 'Art. Cur.,' i, 1.

[5] 'Opt. Sect.,' 30; 'Vict. Rat.,' ii, 36; 'Comp. Pharm. Loc.,' i, 1, &c.

[6] 'Const. Art. Med.,' 18.

will be a good physician only in so far as he discovers the ends towards which nature is working, and becomes (as Hippocrates had said before him) the servant, the imitator, and the interpreter of the wise, self-sufficient, and just Author of our being;[1] whence is derived the general principle that "everything in a disease is a ground for an indication of treatment; if against nature, by the use of contraries —if according to nature, by like."[2]

Disease itself he defines to be that which produces changed action in the body;[3] and he consequently states that there are two distinct classes of remedies—those which act upon the disease itself, and those which relieve its effects, the symptoms; and that there are two classes of physicians who err by employing the one of these plans of treatment to the exclusion of the other. The true physician's motto should be Μηδὲν ἄγαν; he should endeavour to combine the two, or, if they are incompatible, treat the disease only, and not its symptoms.[4]

These general propositions may seem to many persons mere truisms, yet they are truisms which even in our own day cannot be repeated too often. We should not have so many interminable (because illogical) discussions on the treatment of acute diseases, if we all bore in mind that "it is not enough to know that a disease is a pleurisy or pneumonia; one must also know of what kind it is."[5] Nor could Hahnemann, Broussais, or Brown, have established their various systems if they had ever realised the double character—physiological and pathological—of disease.

The two great modes of treatment—regimen and bloodletting—which do not depend on the use of medicines, are determined almost entirely by the general rules just stated.

As to diet, Galen tells us, "In every sick man, look from the very first day to the two things which Hippocrates bids us consider in ordering a regimen, viz. the greatest probable intensity of the disease, and the strength of the patient."[6] In long cases especially, "unless you feed the patient well, you will kill your man and the disease together."[7] He therefore in the intervals of malarial fever always fed well,[8] and recognised the importance of keeping up

[1] In 'Hipp. et Plat.,' ix, 8; 'San. Tu. ad Thras.,' 26; vi Epid., v, 1; i Dieb. Dec. 11.

[2] 'Meth. Med.,' xi, 13. To give an instance of this from Galen's own practice, immoderate excretion in fever, is against nature, and is to be checked by opium; while moderate excretion is favorable, and is to be encouraged by emetics, purges, and diuretics.

[3] 'Meth. Med.,' ii, 6.

[4] Ibid., xii, 1.

[5] In 'Vict. Rat.,' ii, 36.

[6] 'Meth. Med.,' x, 5. In another place ('In Vict. Rat.,' i, 44) he tell us that some accused Hippocrates of stuffing (ἐμπιπλᾶν) his patients, others of starving them to death, because he treated each case on its own merits.

[7] 'Art. Cur.,' i, 8.

[8] 'Meth. Med.,' xi, 21; 'Art. Cur.,' i, 11.

the patient's strength during acute local disease, in language which even Dr. Todd would have scarcely disavowed. "The one chance of recovery in pleurisy, pneumonia, angina, synanche, and even inflammation of the liver and stomach, is the strength of the patient."[1] He had also noticed that want of food in "dry, hot, temperaments" might alone produce fever, or assist other causes already at work; and he therefore found that by allowing bread and wine from the beginning of many cases of ephemeral fever he cured many who, under the usual practice of starving for three days, became seriously ill.

In putrid and pestilential fevers, and in those complicated with congestion or inflammation of any important organ, he did not allow food for some days, but gave light wine very soon, believing it to promote all the secretions, unless the fever was very high;[2] and whenever great weakness was perceived, he gave strong wine freely.

I need say little on his rules for bloodletting, as they have been already excellently summarised by Dr. Chambers. The main general rules which he appears to have relied on were—never to bleed a patient under fourteen years of age; to bleed less readily in summer and winter than in the more moderate seasons; and to bleed fair, soft-skinned people, like the Gauls and other northerns, and people from the south or from Egypt, less readily than Greeks or Italians.[3]

Supposing no contra-indication to exist, he used habitually to bleed in fevers and acute inflammations. In ordinary inflammations he bled from the vein nearest to the part affected,[4] or, in rheumatic inflammation, from a distant vein,[5] hoping thus to produce revulsion as well as evacuation.

In continued fevers Galen only drew blood in young, strong, plethoric subjects.[6] He had had abundant opportunities of observing the ill effects of two opposite plans of treatment. He saw the Methodists, on the one hand, refuse to bleed when in putrid fevers the thoracic or abdominal organs became congested or inflamed, imagining that "relaxing" poultices would relieve the constriction which they held to be the essence of inflammation; and he noticed that their well-fed plethoric patients fared much worse than the previously sickly or poor ones.[7] On the other hand, he saw "two patients perish in the very hands of the doctors, who never recovered after falling into syncope;" and even when immediate death was not the result, he saw that excessive bleeding would prolong the disease and make it more fatal, would produce various cachexiæ,

[1] 'Meth. Med.,' x, 5. [2] Ibid., xi, 9.
[3] 'Cur. Rat. per Sang Miss.,' 13, de Sectis 8; 'In Vict. Rat.,' iv, 19.
[4] 'Meth. Med.,' xiii, 11. [5] 'Art. Cur.,' ii, 2.
[6] 'Meth. Med.,' xi, 14; 'Art. Cur.,' i, 13.
[7] 'Meth. Med.,' xi, 15.

anæmia, dropsy, orthopnœa, weakness of the stomach and liver, paralysis, and insanity [1]—a catalogue almost exhaustive of the various ills which an opponent of venesection might attribute to it.

It is therefore clear enough that he never bled in the indiscriminate way in which, alas! our African or Indian practitioners used to do, or as is still unhappily the rule with Italian and Spanish physicians; and we must remember, in any estimate of his bloodletting tendencies, that he had to do with the most gluttonous and idle race that perhaps ever existed. It is still more certain that he would never have followed the late French practice of invariably starving after bleeding; indeed, he seems to have often found it advisable to feed well afterwards,[2] and thought previous abstinence from food was a contra-indication to bleeding.[3]

Cupping was practised with the usual object of attracting blood to the part cupped; therefore the pubes and thighs were cupped some days before the supposed catamenial period in amenorrhœa,[4] the back of the neck was cupped for various ophthalmiæ,[5] the breasts were dry cupped in menorrhagia, and the præcordia in epistaxis.[6]

Galen used also to employ the plan of rubbing the limbs and then binding them; not, as did Erasistratus, to supersede bloodletting, but to produce, as I suppose, something of the effect of "Junod's boot."

This seems to be the place to refer to a very interesting use which Galen made of friction. When a completely adynamic case of fever came before him, with feeble pulse, and cold, livid, swollen skin, he never bled or purged, but ordered the whole surface of the body to be rubbed roughly, beginning from the feet and going upwards, and continuing this treatment until the whole body became very hot.[7] Every one will recognise the similarity between this process and the hot mustard baths recommended by Trousseau and others in cases of adynamic typhoid and scarlatina; and it may be worth remembering as a possible resource in cases of scarlatina where the cold affusion seems desirable, but cannot be practised on account of the prejudices of the patient's relatives.

Before passing to speak of some of the medicines employed in Galen's time, I may remark that by the first reading the prescriptions ordered by the old physicians two effects are produced on the mind. The first is a feeling of great thankfulness that one was not born in an age when every conceivable nastiness of the animal king-

[1] 'Meth. Med.,' ix, 10.
[2] 'Opt. Sect.,' 28; 'Venæsect. adv. Erasistratæos,' 3.
[3] 'Meth. Med., v, 13.
[4] 'Cur. Rat. per Sang. Miss.,' 18.
[5] 'Meth. Med.,' xiii, 19.
[6] Ibid., v, 3.
[7] Ibid., xii, 3.

dom was freely administered;[1] the second is a belief that it is hopeless to expect to discover what medicines they really relied upon among the endless and complicated formulæ which are enumerated for every disease. However, those which they chiefly trusted to may be generally found out by comparing a number of prescriptions for the same disease, or by the special remarks made on them.[2]

Among the medicines used by Galen and his contemporaries there are only a few which were as well known and as commonly employed as at the present day; the greater number are now abandoned; and, on the other hand, a very large number of our most valued medicines were considered by the ancients as violent poisons, or were totally unknown to them. I may say a little about each of these classes.

I. *Medicines used now and in Galen's time.*

Opium is at the head of this list, for there is, I suppose, scarcely a known use of opium which was not understood then. Galen, perhaps, used it more freely than we should in cases of continued fever where there was much pain, insomnia, or great excretion;[3] and it was the favorite medicine in his time in cases of bronchitis, with abundant expectoration and troublesome cough.[4]

But opium was also the chief ingredient (combined with almost all known aromatics and stimulants) in the various "antidotes" which were taken habitually by the Romans to preserve them from the ill effects of poison,[5] of the bites of venomous animals,[6] and of bad diet.[7] These antidotes, or "theriacæ," came afterwards to be used in the treatment of almost all diseases, in an indiscriminate manner, the rationale of which it is difficult for us to follow.

Trousseau and Pidoux, in their ingenious endeavour to explain the action of cinchona in malarial fevers, point out that these compounds must have had something of the same effect as bark, which they call "a kind of natural theriaca;[8] and this is no doubt the real explanation of their former value and present neglect; they

[1] It is but fair to Galen to admit that he opposes, with some severity, one Xenocrates, who used *every one* of the most disgusting secretions of the body in medicine! ('Simp. Med. Fac.,' x, i.)

[2] Galen's work, 'De Simplicium Medicamentorum Facultate,' is a treatise on materia medica; the 'De Compositione Pharmacorum secundum Locos' (one of his most valuable works), is a "Selecta e Præscriptis;" and the 'De Compositione Pharmacorum per Genera' deals with practical pharmacy.

[3] 'Meth. Med.,' xii, 1.

[4] 'Comp. Pharm. Loc.,' vii, 2, 3.

[5] For instance, "Antonine, in order to be safe from poisoners, used to take every day a piece of theriaca, of the size of an Egyptian bean."

[6] Hence the name θηριακά.

[7] 'De Antid.,' i.

[8] 'Traité de Mat. Méd.,' 6e éd., ii, 443. It may, however, be remarked that these authors are wrong in supposing theriaca to have been generally used in the treatment of ague.

served a purpose which is now more effectually answered by cinchona and its derivative. It is much more difficult for us to understand how the theriacæ could preserve from the venom of serpents, yet the author of the treatise 'De Theriaca ad Pisonem"[1] had his facts (if facts they were) to go upon. "No one has ever been known to die who took theriaca immediately after having been bitten by one of those animals which destroy life; and if any one takes it, and is bitten soon afterwards, its venom is harmless, as has been frequently proved by judges, who (having power to put to death and to pardon, and being desirous of trying this medicine) have given it to criminals condemned to death." The author then goes on to say that he had tried the same experiment on wild fowl, and with the same results.

Squills, again, were employed for the same diuretic and expectorant effects as by us.[2] They were sometimes pushed to a dangerous extent, for, after one prescription containing squills, Galen remarks, "It is a good remedy, and after its use patients pass blood by urine."[3]

Aloes, galls, all the terebinthinate drugs, the resins and gum-resins, were much more largely employed than at the present day; while scammony, castor oil, colocynth, henbane, conium, and many other medicines, were equally well known. The root of the male and female ferns were used to destroy the tænia,[4] and also as a bitter in chronic bronchitis and liver disease. Rhubarb (probably from its scarcity) was but little employed.

II. *Medicines used now, unknown or not employed by the ancients.*

Under this head are, of course, to be ranged all the valuable accessions to the materia medica derived from the New World, and almost all the medicines belonging to the mineral kingdom, which were employed only externally by Galen.[5] The only exceptions are, the Lemnian earth, which was given for the bites of snakes; the Armenian earth, which was given during a plague which devastated Rome in Galen's time;[6] alum, which was used as an astringent for dysentery and hæmaturia;[7] and burnt oyster shells and hartshorn, which seem to have been only employed in dysentery.[8]

But if Galen and his contemporaries were timid in administering mineral substances by the mouth, they seem to have been more than bold in using them in other ways. Litharge, antimony, realgar, orpiment, blue vitriol, verdigris, calamine, and oxide of zinc, were used far more freely than we should venture to do; they were very

[1] Apparently not Galen, according to Ackermann (Fabricius, 'Bib. Græc.,' iv, 19).
[2] 'Simp. Med. Fac.,' viii.
[3] 'Comp. Pharm. Loc.,' ix, 2.
[4] 'Simp. Med. Fac.,' viii.
[5] Ibid., ix.
[6] Ibid., ix.
[7] 'Comp. Pharm. Loc.,' ix, 5, x, 1.
[8] Ibid., x, 5; "Remed. Fac. Par."

constantly applied to ulcers, to carious teeth, and injected into the bowel in dysentery.[1]

III. *Medicines employed by the ancients and not used now.*

All of these are derived from the vegetable kingdom; they are extremely numerous, but I may mention a few of the most interesting.

One of the most prominent is *apium* or *petroselinum*, by which names were meant apparently the parsley and celery, wild or cultivated. Apium was constantly used as a diuretic in fevers,[2] and to relieve painful affection of the kidneys,[3] to promote the catamenia, and, combined with the seeds of umbelliferæ, to relieve colic.[4] I have made a few trials of parsley seed, and am sure that further experience would prove it to be a very valuable medicine. Its emmenagogue action is (as MM. Joret and Homolle showed[5]) very evident in cases of atonic amenorrhœa, and it also evidently acts as a diuretic and a sedative to the urinary organs.

The root of the common *caper* was also employed (by Galen more than his predecessors) in the treatment of "indurated" spleen.[6] Its utility in such cases was probably owing to its being a powerful purgative and diuretic, and containing also a bitter principle; but Galen speaks so well in its favour from his own experience that it might be worth a trial in what Sir R. Martin calls "endemic congestion of the spleen," which is treated in India with purges and tonics.

Pepper was not only employed as a stimulant aromatic, but also in tertian and quartan ague, as previously recommended by Dioscorides and Celsus. This is interesting, as it corroborates the remarks made in modern times on the antiperiodic power of this substance.

Of the three purgative medicines mentioned in the following passage,[7] two are unknown to modern practice, and might perhaps be tried with advantage. "Scammony produces a great excretion of yellow bile; the Attic epithymum (dodder), of black bile; and the seed of the (Daphne) gnidium, of pituitous and watery matter."

This list by no means exhausts the "Galenicals" which might be found useful in practice, but it is sufficient to show that, perhaps, "aurum latet in stercore illo scholastico," and that, now we have abandoned the errors of polypharmacy, many a valuable medicine might be found in the simples which we neglect.

I will now proceed to give a short account of the treatment of

[1] 'Comp. Pharm. Loc.,' v, 9, x, 5, 7; and 'Art. Cur.,' ii, 2, 8, 10.
[2] 'Art. Cur.,' i, 9; 'Meth. Med.,' xi, 9.
[3] 'Comp. Pharm. Loc.,' x, 1.
[4] 'Simp. Med. Fac.,' viii.
[5] Trousseau et Pidoux, 'Mat. Méd.,' ii, 394.
[6] 'Art. Cur.,' ii, 5; 'Meth. Med.,' xiii, 19; 'Simp. Med. Fac.,' vii.
[7] 'Ther. ad Pis.,' 3.

some of the most important diseases, and will first remark that two errors were very common in Galen's time. The first was the error in diagnosis (probable enough in a climate where malarial and pestilential fevers were rife) of taking the pyrexia caused by some local inflammation to be an essential fever.[1] The second was the almost uniformly stimulating and astringent plan on which all local diseases were treated. Our author strove to oppose this,[2] but he was evidently often carried away by the current.

Simple ephemeral fevers, which were commonly produced by checked secretion, by exposure to heat or cold, by emotion of mind, or over-fatigue of body, were treated generally by baths alone; light food and wine were generally allowed.[3]

Bubo-fevers (probably not plague, but the same as the epidemics described by Captain Galton and Dr. Sutherland[4] as common in Mediterranean cities) were treated in much the same way. He gave no wine until the bubo had gone down; he applied wool dipped in hot oil to these swellings, and dressed the sores (which he considered caused the bubos) with a stimulating ointment. The treatment of intermittent fevers was to a certain extent influenced by his theory that in tertian the liver was chiefly affected, in quartan the spleen, and in quotidian the stomach. He looked upon tertian as the mildest of the intermittents, having observed that regular tertian generally ended with the seventh attack, while quartan was far more tedious, and quotidian was most dangerous.[6]

In perfectly regular tertian he gave purges or emetics, used apium or anethum as a diuretic, and wormwood and warm baths at a later period of the disease. When tertian was irregular he applied heat to the epigastrium, gave an emetic, bled unless there were contra-indications, and then administered pepper and other stimulants for seven days, after which he fell back upon wormwood. For quartan he bled if the patient was plethoric, ordered a full diet of easily digestible food (looking to the probably long duration of the disease), and gave pepper. For quotidian he employed oxymel and powerful diuretics.[7]

"Putrid" continued fevers were treated with the main purpose of getting rid of the putrescent matter supposed to be contained in the blood by urine, stools, and sweat. He began therefore by giving plenty of barley-water and oxymel, or infusion of parsley root, and clysters. Afterwards he gave camomile and light wine, which he

[1] 'De Præcog. ad Posth.' [2] *e.g.* 'Comp. Pharm. Loc.,' viii, 2, x, 2.
[3] 'Art. Cur.,' i, 2; 'Meth. Med.,' viii.
[4] Quoted by Aitken, vol. i, p. 369. Galen is unfortunately not very explicit on the subject of the plague spoken of in 'Simp. Med. Fac.,' ix, and 'Meth. Med.,' v, 12; but it was most probably a fever of typhoïd character.
[5] 'Art. Cur.,' i, 3; 'Meth. Med.,' xiii, 5.
[6] 'Art. Cur.,' i, 8.
[7] 'Art. Cur.,' i, 9—12.

found of especial value in promoting all the secretions, and ordered tepid baths.[1] He only bled young, strong, plethoric patients.[2]

Galen attached great importance to the *general* treatment of many apparently local diseases, such as ulcers, skin diseases of the scalp, and diseases of the eye.[3]

Among diseases of the nervous system I find few points worthy of note, beyond the distinction (which Hippocrates had established, and which has unfortunately only lately been brought again to light) between the opposite causes of somewhat similar diseases. Thus, Galen points out that loss of blood will produce delirium, convulsions, and lax paralysis,[4] while, on the other hand, plethora will cause apoplexy and other like diseases.[5]

In all cerebral diseases, when the age, strength, and previous condition of the patient allowed it, he used to bleed, to apply stimulant aromatics to the nose and head when the patient was insensible, or cold poppy fomentations in cases of mania or delirium.[6] In paralysis the usual practice was to use stimulating frictions and anodynes internally.[7]

Epilepsy had been studied with, perhaps, more care than any other nervous disease, all the main phenomena being described by Galen.[8] He treated the disease on general principles, by endeavouring to remove the cause. Thus, he bled plethoric patients, generally from the saphena vein in the leg, and purged others regularly every spring.[9] When the stomach appeared to be at fault he administered aloes and bitters—a plan which he also followed in stomach-vertigo. Theriaca was given for epilepsy, as for most other diseases,[10] and stimulants applied to the head;[11] while a number of strange miscellaneous remedies (ass's hoof, goat's liver, hare's blood, peony root hung round the neck! &c.) were employed. Yet the result of all this treatment is summed up in a sentence of remarkable candour:—"Those who have passed the age of puberty are seldom or never cured."[12]

Palpitation seems to have been not an uncommon complaint, and to have been always relieved by bleeding and low diet.[13] "Pleuritis" and "peripneumonia" attract our attention first among diseases of the respiratory organs by the frequent occurrence of these words, and their identity with those we still employ to designate two very

[1] 'Meth. Med.,' xi, 9. [2] Ibid., 14.
[3] Ibid., iii, 2, 8; 'Comp. Pharm. Loc., i; 'Aph.,' vi, 31.
[4] 'Aph.,' vii, 9; 'Meth. Med.,' viii, 10.
[5] 'Aph.,' vi, 31; 'Cur. Rat. per Sang. Miss.'
[6] 'Meth. Med.,' xiii, 21. [7] 'Comp. Pharm. Loc.,' vii, 3.
[8] 'Loc. Aff.,' iii, 7.
[9] 'Cur. Rat. per Sang. Miss.,' vi; 'Aph.,' 47, "Purg. Med."
[10] 'Ther. ad Pis.,' xv.
[11] 'Loc. Aff.,' v, 6; 'Simp. Med. Fac.,' iv, 18. [12] Introd.
[13] 'Loc. Aff.,' v, 2.

important diseases. Yet their meaning was not quite the same as at the present day; for by "pleuritis" was signified, besides what we call pleurisy, all those cases of inflammation of the lung where the pulse was hard and the breathing was not very difficult, all remaining cases of pneumonia being called "peripneumonia."[1] The ancients, consequently, had remarked that the sputa were not invariably rusty or blood-coloured in pleuritis, but were always so in peripneumonia.[2] If they had founded their distinction on the colour of the expectoration (the *prognostic* value of which they had carefully noted[3]), their classification would have been anatomically correct. The opinion of Hippocrates and Galen, that "pleuritis" generally lasts seven or nine days,[4] is confirmed by that of the modern Viennese school on the "cyclic" character of pneumonia.

The treatment adopted for both diseases was to begin by applying hot fomentations;[5] if they did not relieve the pain and other symptoms, blood was let from the vein of the affected side,[6] and purges were administered—generally colocynth and hellebore[7]—unless there was diarrhœa, which was looked upon as a bad sign.[8] Confections were given, containing opium, hyoscyamus, and diuretics.[9] After evacuation, wine was generally allowed, especially when the sputa were scanty, and light food.[10]

Acute bronchitis occupied a comparatively insignificant place in Galen's pathology, no doubt because all cases of severe capillary bronchitis were included under the term "peripneumonia." It was treated usually in his time with opium, mandrake, and henbane—a plan which was originated by Andromachus for all the cases in which the secretion in the chest was abundant. When sleep was required Galen used to add cooked must of wine to the narcotics, and when the secretion was scanty he gave honey, which he found injurious when the expectoration was greater in quantity.[11]

In chronic bronchitis and "orthopnœa" (vesicular emphysema) he ordered light wine for drink; the narcotic preparations were combined with pepper, horehound, galbanum, &c.; and when the sputa were very abundant, yet too tenacious to be easily expectorated, he preferred squills and oxymel in rather large doses.[12]

Those cases of phthisis which begin insidiously were included under the head of chronic bronchitis[13] and loss of voice,[14] and were treated with opium or hyoscyamus and conium, combined with the

[1] 'Subfig., Empir.,' 6; 'Vict. Rat.,' iii, 49.
[2] 'Loc. Aff.,' ii, 9.
[3] 'Tot. Morb. Temp.,' 6; 'Aph.,' i, 12.
[4] Ibid.
[5] Probably to allow for mistaken pleurodynia or intercostal neuralgia ('Loc. Aff.,' ii, 9).
[6] Sometimes even to syncope (iv 'Vict. Rat.,' 73).
[7] 'Vict. Rat.,' i, 2, 33.
[8] Introd., 13.
[9] 'Comp. Pharm. Loc.,' vii, 5.
[10] 'Vict. Rat.,' iii, 2.
[11] 'Comp. Pharm. Loc.,' vii, 2, 3.
[12] Ibid., vii, 4, 6.
[13] Ibid., vii, 5.
[14] Ibid., vii, 1, 2.

different aromatics; turpentine resin and myrrh were very generally added to these confections, by the use of one of which one of the emperors is said to have been cured.[1]

Galen generally bled in the beginning if the patient was strong enough, and gave an opiate confection at night, to check cough and procure sleep, every means (of diet, exercise, &c.) being employed to strengthen the patient.[2] A warm and dry climate was considered the most suitable, Upper Egypt or Libya being generally chosen.[3] Galen also practised the "cure de lait" with very great success; he sent very many of his phthisical patients by sea to Tabiæ, a village on the southern side of Vesuvius, and close to the Bay of Sorrento, in which sheltered climate they principally lived upon milk warm from the cow, sometimes with the addition of honey, or of salt if the bowels were confined.[4]

He attributed those cases of phthisis which begin with more acute symptoms to "ulcer in the lung," which he perceived frequently came on after an attack of hæmoptysis (Morton's "phthisis ab hæmoptoë)."

In order to check the inflammation which he believed caused the ulcer, he used to resort to very severe treatment, as will be seen by the following abstract of two cases, which he reports more fully than is his custom. The first is that of a great lady, who sent for him in consequence of a slight attack of hæmoptysis:

"I did not like to bleed her, as she had had a catarrh for four days, and therefore had gone almost without food; but I ordered a sharp clyster, had her legs and arms rubbed with a stimulating preparation, and bound, and gave her weak barley-water for diet and some fruit. 'At bed-time I administered theriaca,' the opium in which produced sleep and checked the secretion. Next day, "the catarrh being ended, it was quite plain, from the character of the breathing and the cough, that the lung required cleansing. . . . . I kept her perfectly quiet and silent, had her limbs rubbed and bound, and had the whole body rubbed," the head shaved, and a stimulating application used to it. "This evening, again, I gave theriaca" (with less opium than the night before). The same plan of treatment was from that time continued, with the addition of honey as an expectorant, and good food was given to keep up the strength. In the second case, "A young man who had a cough, not from catarrh, but from cold affecting the organs of respiration, had spit up half a pint of blood. I bled him twice on the first day, and twice again the next day; had the limbs rubbed and bound on the first day, and gave at night" a confection containing opium. "The second day, after bleeding, I applied thapsia (a rubefacient) to the chest . . . . .

[1] 'De Antid.,' ii.
[2] 'Meth. Med.,' v, 14.
[3] Introd.
[4] 'Meth. Med.,' v, 12.

and the same on the third day. For the first two days I allowed only weak barley-water; on the third day stronger barley-water, and some fish or other light food. I continued to give the confection on the second and third evenings, for it produces sleep, relieves pain, and checks secretion. . . . . In this way I have cured all those who have put themselves under my care from the first day of the disease."[1]

The distinction drawn by Galen between these two varieties of pulmonary consumption, the slow, non-inflammatory, bronchitic, and the inflammatory ulcerative form, correspond, to a very great extent, with that which the German physicians of the present day propose, and which is, perhaps, most clearly expounded by Niemeyer in the articles of his treatise on medicine devoted to this subject.

As regards prognosis, Galen had observed that those phthisical patients whose sputa tasted salt were in the worst case;[2] and he followed Hippocrates in recognising the fatal influence of diarrhœa, and in looking upon loss of hair as a very bad sign.[3]

Indigestion (a more prominent disease among the lazy gluttonous Romans than even among our own countrymen at the present day) was treated by most of Galen's predecessors and contemporaries in a purely empirical and very unsatisfactory manner. Thus, Andromachus, whose position as physician to the Emperor Nero gave him great influence, used to treat dyspepsia, as a matter of routine, with aloes and aromatics, which he administered three or four times a day on an empty stomach. The same plan was also followed by Themison and other men of note, and of course, as Galen remarked, benefited many dyspeptics, and seriously injured many others.[4]

Galen himself began practice with the principle, which had been laid down for him by his teachers, that in all cases of dyspepsia the stomach required strengthening by the internal administration of astringents and the application of rubefacients to the epigastrium, a principle which he very soon discovered to be untrue.[5] He would then appear to have adopted, in the main, the mode of treatment proposed by one Archigenes, who had pointed out all the chief rational indications of treatment. In acute accidental dyspepsia he produced vomiting, applied hot cloths to the head and præcordia, and starved for twenty-four hours; if it became atonic he gave three or four cups of cold water, or, in more chronic cases, ice three or four times a day on an empty stomach. When there was great heat he applied cold to the epigastrium, and for acid eructations administered a dose of coriander before meals. Obstinate cases of vomiting were treated

[1] 'Meth. Med.,' v, 13.
[2] Ibid., v, 14.
[3] iv 'Aph.,' 12, 14.
[4] 'Comp. Pharm. Loc.,' viii, 2.
[5] 'Meth. Med.,' vii, 4.

with vegetable astringents, and, as a last resource, with opium and aromatics.[1] Galen based his own division of dyspepsia on the "temperaments," which he considered to dominate the whole of medicine; so that the varieties distinguished by him were supposed to be owing to an undue preponderance of heat, cold, moisture, or dryness. Not to enter upon a discussion of theoretical points, I may mention that our author tells us that in "hot" cases food taken is rapidly decomposed, and there is much thirst, fever, and fetid flatulence; while in cases of "cold" indigestion there is no thirst or fever, but simple apepsia, without any rapid decomposition of the food taken, and with acid inodorous eructations.[2] The former of these, therefore, corresponds to our "irritative," the latter to our "atonic," variety of dyspepsia.

The "moist" kind Galen connected with dropsy, and the "dry" with old age or marasmus; but these last two were generally complicated with either "hot" or "cold" dyspepsia, and were looked upon as much more difficult of cure than the two former. These two he treated on the same general principles employed by Archigenes; and I suppose that, before the chemical discoveries made in a later age, no more rational plan could be adopted—giving iced drinks in "hot" dyspepsia, and strong wine, highly spiced food, and little drink, in "cold" cases.[3] For the "dry" variety (corresponding to the senile dyspepsia so well described by Durand-Fardel[4]) he used to order warm baths, abundance of milk, and nourishing, easily digestible food.[5] The rules which he lays down for the use of purgatives are too long to give here, being minute, although thoroughly practical.[6] Stomach-headaches were treated by producing vomiting, and purging with aloes and aromatics, Andromachus's "hiera picra" finding here its best use. Scammony was added when the biliary secretion was considered to be in fault. Stimulant applications were used to the forehead, and sometimes poppy poultices or lotions of solanum.[7]

Perhaps diseases of the liver give us occasion to make the fewest practical notes of treatment in Galen's time, owing to the very confused state of diagnosis, and to the exaggerated ideas entertained

[1] Asclepiades had been bold enough to administer sulphate of copper in obstinate vomiting, for which he is duly censured by Galen ('Comp. Pharm. Loc.,' viii, 3, 4).

[2] 'Sympt. Caus.,' iii, 1; 'Loc. Aff.,' i, 5; 'San. Tu.,' vi, 10.

[3] 'Meth. Med.,' vii, 4.

[4] 'Mal. des Vieill.' p. 723.

[5] Galen considered human milk the most effectual, but (as he amusingly expresses it) used to order asses' milk for those asses who did not like returning to their infantile condition!

[6] 'San. Tu.,' vi, 10; 'Meth. Med.,' vii, 13.

[7] 'Comp. Pharm. Loc.,' ii, 1. Probably the solanum (*στρύχνον*) used was stramonium. The *στρύχνον μανικόν* is evidently belladonna, not (as Pereira considers) Stramonium.

by Galen and his school of the importance of the liver.[1] I need only here remark that icterus was very justly recognised as being, not a disease in itself, but a symptom of other diseases. Thus, it was looked upon sometimes as a crisis in fever, and was then treated by warm baths and frictions,[2] means certainly very often useful in icterus at the present day. When dependent on local affection of the liver, it was considered to be produced either by obstruction, inflammation, or tumour,[3] in which cases the ancients generally purged with colocynth or gave diuretics and wormwood.

Diseases of the spleen occupied a far more important position in Galen's pathology than in ours. This is owing, partly to the "indurations" of the spleen which followed malarial fevers, and for which quinine was not then available, partly to error in diagnosis, the affection of the spleen being looked upon as a primary disease, when it was in reality a secondary one. The favorite remedy before Galen's own time was apparently squills,[4] the root of the caper (of which I have spoken above) having being probably brought into notice by Galen himself. He mentions, among many other remedies, that the water in which red-hot iron had been frequently plunged was recommended as a drink by Archigenes. It is curious that Galen twice[5] insists on dark colour of the skin as a symptom of great value in the diagnosis of splenic diseases. In acute splenic diseases he generally purged freely, a thing he was afraid of in liver disease ;[6] in either case he used to cup over the affected organ.

Dysentery is the most important by far of the intestinal diseases treated of by Galen. It consisted essentially, according to him, of ulceration of the intestine, produced by some acrid humour, generally bile.[7] He considered it specially common in spring and summer,[8] and in persons who, having been long accustomed to hard work, had given it up.[9] The best medicine he knew of was to take very little food.[10] When the dysentery occupied the upper part of the bowel he trusted mainly to remedies given by the mouth (such as burnt oyster shells and hart's horn, and confections of opium, with galls, quince, sumach, and tamarisk) ; when the rectum was ulcerated (as was believed to be specially indicated by tenesmus) enemata were

[1] Yet (as Frerichs remarks, in the historical introduction to his great work on diseases of the liver) the present generation of physicians has, to a great extent, returned to Galen's belief in the sanguifying action of the liver.

[2] 'Comp. Pharm. Loc.,' ix, i.

[3] In iv 'Aph.,' 62; 'Loc. Aff.,' v, 7.

[4] 'Comp. Pharm. Loc.,' x, 2.

[5] 'Loc. Aff.,' v, 7, vi, 1.

[6] 'Meth. Med.,' xiii, 15.

[7] 'Loc. Aff.,' i, 2. In ii 'Epid.,' ii, 17.

[8] In 'Nat. Hum.,' i, 33. It is noteworthy that he says ('Comp. Pharm. Loc.,' vii, 2) drinking the sediment of water which had passed through leaden pipes was a cause of dysentery. Can he have intended to speak of bronze instead of leaden conduits, in which case copper colic (of which dysentery is a symptom) would be produced?

[9] In 'Artic.,' iv, 40.

[10] 'Remed. Fac. Par.'

preferred, alum, orpiment, realgar, and other violent irritants, being employed.[1]

Dropsy was supposed by our author to be most frequently caused by induration of the liver, but he also recognised as causes diseases of the spleen, intestine, kidney, or lung, or retention of the catamenial or hæmorrhoidal flux.[2] When the disease was owing to this latter cause Galen followed the practice of Hippocrates, and bled;[3] in other cases he used to employ squills and the caper-root, as for splenic diseases. It was a common practice in his time to apply poultices containing purgatives and diuretics to the abdomen, of the same kind as Trousseau employs in such cases at the present day.[4] Diabetes was a very rare disease in Galen's practice, as he tells us he had only seen two cases.[5] It was believed by many of his contemporaries to be a disease of the stomach, like bulimia; on the other hand, he himself attributed it to a weakness of the kidneys, which allowed fluids taken to escape immediately.

Hæmaturia our author treated with alum, and arrested urinary secretion with apium and other diuretics.[6] His account of the nature and treatment of urinary calculus proves that he met with almost invariably the lithic acid variety. Thus, he tells us that he believed calculi were generally (not invariably, as some had supposed) formed in the substance of the kidney, in the same manner as tophi in gouty joints;[7] and calculous and gouty patients were subjected by him to the same régime, being fed upon fish and fowl, allowed none but asses' milk, and ordered wine sweetened with honey as a diuretic.[8] The various remedies employed for calculus as lithontriptics, containing myrrh, parsley and cummin, ammoniacum, the stone found in sponges, &c., although very confidently recommended by Galen,[9] need not detain us any longer.

It might be supposed at first sight that Galen admitted the identity of gout and rheumatism advocated by Chomel and Pidoux in our day, for he says, "'Ischias' (sciatica) and podagra are both of the genus arthritis, for, what arthritis is in all the joints, that, in the hip alone, is called ischias, and in the foot podagra."[10] But on further examination it is plain that Galen clearly distinguished from arthritis those cases of acute rheumatism which run a distinct course[11], and that "arthritis" was made up of chronic rheumatism, rheumatic arthritis, gout in its extended form, and other similar affections. He had ample opportunities of observing gout, an ex-

[1] 'Comp. Pharm. Loc.,' ix, 5.
[2] v 'Loc. Aff.,' 7.
[3] 'Venæsect. adv. Eras.,' 6.
[4] 'Comp. Pharm. Loc.,' ix, 3.
[5] 'Loc. Aff.,' vi, 3.
[6] 'Comp. Pharm. Loc.,' x, 1; 'Rem. Par. Fac.,' cap. 22.
[7] In iii 'Epid.,' i, 6.
[8] 'San. Tu.,' vi, 11.
[9] 'Simp. Med. Fac.,' ix; 'Comp. Pharm. Loc.,' x, 1.
[10] 'Comp. Pharm. Loc.,' x, 2.
[11] In 'Aph.,' vi, 30.

ceedingly common disease in his time,[1] and he followed a generally rational plan of treatment. Thus, as he had remarked that the most common cause of the disease was intemperate living, he laid down the régime I have just mentioned, and began his treatment of a case of gout, if in a plethoric person, with bleeding and purging, repeating this early every spring, a course which he found sometimes prevented an attack.[2] But if the patient was weak he abstained from depletory means of any kind, as his experience had shown them to be injurious to the state of the joints.[3] During the fit he applied stimulating poultices, containing poppy juice, solanum, mandrake, &c., and sometimes contrived to produce vesication,[4] and in the chronic state attempted to remove the deformities of the joints by resolvent plasters. Great success was attributed to these; the basis of all of them was the Emplastrum Plumbi (*τὸ φαρμάκον διὰ τῶν χυλῶν*), invented by one Menecrates, with the addition of galbanum, turpentine, wax, and even frogs.[5]

There are innumerable other interesting points of practice on which I would have gladly dwelt, especially on the treatment of elephantiasis, cancer, and poisoning, besides the more theoretical question of the nature and time of crises and critical days; but I have already taken up too much space which might have been devoted to more practical matter, and may perhaps most fitly conclude by applying to the Pergamene his own account of his predecessor Archigenes:

"Most of those who undertake the practice of the liberal arts strive, not so much to be learned in them, as to appear learned to the crowd. But Archigenes, if any man ever did, strove to master the science of medicine, and so has left behind him many works which are justly valued. Everything he wrote does not, indeed, appear to me correct, and, just as he blames many statements made by those whose teaching had helped him to excellence, so it is fair that he also, when wrong, should be blamed by us, his successors; for it is difficult for any man not to err frequently, either from mere ignorance, error of judgment, or carelessness of expression."[6]

[1] In vi 'Aph.,' 28.
[2] 'Sang. Miss.,' 7; in vi 'Aph.,' 47.
[3] 'San. Tu.,' vi, 11.
[4] 'Comp. Pharm. Loc.,' x, 3.
[5] 'Comp. Pharm. per Gen.,' vii, 10.
[6] 'Comp. Pharm. Loc.,' ii, 1.

ART. III.

On the Diagnosis of Obstructive Disease of the Left Auriculo-ventricular Aperture. By THOMAS B. PEACOCK, M.D., Physician to St. Thomas's and to the Victoria Park Hospitals.

NOTWITHSTANDING the exactitude which has been attained in the diagnosis of cardiac affections, there is still much difficulty in recognising the presence of one of the most common forms of defect—obstructive disease of the mitral valve, and authorities are not agreed as to the physical signs which occur in such cases. In confirmation of this assertion it is only necessary to refer to the published opinions of several distinguished writers as to the frequency with which they have detected murmurs caused by the flow of blood from the left auricle into the ventricle. Dr. Hope, writing in 1839,[1] says, "Will it be said that the auricular contraction previous to the ventricular should create a murmur? I have looked for it carefully, and have only once been able to suspect it, without being able to assure myself of its existence." He then proceeds to explain the circumstances to which he supposed the absence of the murmur was to be ascribed, saying, "theoretical reasoning seems to countenance this result of observation, for, as the auricular systole is slight, the quantity injected by it is not considerable; and as the ventricle is already full, it cannot admit that extra quantity necessary to bring it to the state of distension without offering a resistance to its ingress, which must greatly retard the force and velocity of the current—a force, indeed, which can never be great, because the auricles are not only weak muscles, but are unsupported by valves behind." Dr. Markham's views, expressed much more recently, or in 1860,[2] are similar to those of Dr. Hope. He says, "Diastolic mitral murmur is comparatively of rare occurrence. I have not met during the last eight years with more than a dozen cases in which I could with certainty diagnose its existence. Some English authors ignore the possibility of such a murmur, and Dr. Latham speaks of it as a sort of clinical curiosity. On the Continent, on the other hand, authorities treat of the murmur as an established fact. The discrepancy of opinion is doubtless to be ascribed to the rarity of its occurrence, and to the difficulty which usually attends its diagnosis." Dr. Walshe's views, expressed in 1862,[3] are as follows:—"A diastolic murmur of maximum force, immediately above and about the left apex, and conducted in the same directions, though less extensively, as a systolic murmur of the same seat, indicates obstructive narrowing of the mitral orifice, or simple roughness of the auricular surface of the

[1] 'Diseases of the Heart,' &c., 3rd ed., p. 79.
[2] 'Diseases of the Heart,' &c., 2nd ed., p. 122.
[3] 'Diseases of the Heart,' &c., 3rd ed., p. 122.

mitral valve, or both states combined;" and he adds that the "murmur is commonly spoken of as diastolic in rhythm, but in point of fact it is post-diastolic or præ-systolic rather than precisely coincident with the diastole." "This murmur is not infrequently wanting when constriction is found after death. Sometimes the deficiency may be fairly referred to the weakness of the auricular systole and smoothness of the constricted orifice; when the constriction is slight, the friction also will be slight." "I have known the murmur come and go from day to day in a case where the mitral orifice was very greatly contracted and rigid, probably from the varying force of the heart's action." On the other hand, Dr. Gairdner and Dr. Hayden speak of this sign as of almost constant occurrence. The former remarks[1], "I hold that without the auricular systolic murmur we have no security at all that the disease is primary mitral disease; and I confess I am surprised at the extraordinary confusion that prevails in your accustomed text-books as to this refinement of diagnosis, which is to me one of the most plain, almost mathematically demonstrable facts, about which there is hardly the possibility of a mistake. I cannot explain the constant assertion of authors that this murmur is of uncommon occurrence, except on the presumption that almost all of them have confounded it habitually with the murmur of mitral regurgitation;" and he concludes by saying, "auricular systolic murmurs are certainly not rare; to me they are among the commonest and most easily detected of all the cardiac murmurs." Dr. Hayden, in a recently published clinical lecture, says,[2] "A murmur of this character and rhythm" (usually harsh and whispering, occupying the terminal portion of the long or diastolic pause, and immediately preceding the first sound, running up to without extending into it), "loudest at the apex, not audible or faintly so at the base, and not transmitted save occasionally to the left side of the lower dorsal spine, may be regarded as diagnostic of mitral obstruction." He further says that the præ-systolic "murmur is, no doubt, frequently absent in cases of veritable mitral obstruction, but never till the advanced stages of the disease when the vigour of the heart has become much impaired, and even then only at intervals, except as regards the period immediately preceding dissolution." For my own part I have long been aware that in some cases of obstructive and regurgitant disease of the mitral orifice the murmur heard at the apex has a distinct double or to-and-fro character; or, in other words, that the murmur which attends the systole of the ventricle, and is caused by the flow of blood from the ventricle into the auricle, is preceded by another murmur due to the forcible flow of the blood into the ventricle with the active contraction of the auricle. Such cases, however, I supposed to be very exceptional. I chiefly noticed

[1] 'Clinical Medicine,' 1862, p. 598.
[2] 'Medical Press and Circular,' 1866.

these double murmurs in cases of mitral disease of long standing, and especially in young persons, in whom the disease often dates from very early life, and probably in some cases is of intra-uterine origin. In such cases the auricular walls are found considerably hypertrophied, and the contraction of the auricle is doubtless more powerful than usual. When such double murmurs existed I did not hesitate to diagnose the presence of mitral obstruction as well as regurgitation; but, under ordinary circumstances, I believed, as I expressed myself in 1865, that it "would often be difficult, if not impossible, to distinguish between simple obstruction and regurgitation," and I regarded the diagnosis of the former condition as not being generally capable of being effected with certainty. More recently, however, I have been led, and chiefly from the more careful consideration of the remarks on this subject by Dr. Gairdner, in his 'Clinical Medicine,' and from the perusal of Dr. Hayden's lectures, considerably to modify the views I had previously entertained, and to believe that the direct mitral murmurs are less uncommon than I formerly supposed, and that they frequently furnish very valuable aid in the diagnosis of obstructive disease.

The following case, which has recently occurred, affords an example of the existence of a præsystolic as well as of a systolic murmur, in a case of combined obstructive and regurgitant disease of the left auriculo-ventricular aperture, in which, guided by the double character of the murmur, a correct diagnosis of the nature of the disease was arrived at during life.

CASE 1.—*Double murmur at the apex of the heart; obstructive and regurgitant disease of the left auriculo-ventricular aperture from disease of the valves, the sequence of an attack of acute rheumatism three years before.*—H. J—, æt. 20, single, a servant. Admitted into Elizabeth Ward, St. Thomas's Hospital, on the 23rd of October, 1866, labouring under symptoms of cardiac disease and commencing dropsy.

She stated that her father died of consumption at the age of fifty-five, but that her mother is living and healthy at the same age. The family originally consisted of eleven, of whom six died in infancy, and the others are alive. She referred her indisposition to an attack of rheumatic fever, which occurred three years before, and stated that since that time she had never been thoroughly well. About nine months after she was first attacked she became much worse, and after a further period of three months she was admitted into St. Thomas's Hospital. While there she suffered under the usual symptoms and signs of mitral valvular disease. She remained two months, and left the hospital somewhat relieved. On returning home she was able to do some little house-work, but did not go out to service till about a month before her re-admission. The place she then went to proved a somewhat hard one, and her health soon

became more seriously affected. The catamenia had not appeared for two months before her admission, and were previously not regular.

The following notes were taken on October the 29th.—She is excessively anæmic, her face being very pale and puffy looking, but with some colour in the cheeks. Conjunctivæ bluish and glassy, pupils large, lips and tongue pale. She has great difficulty of breathing, so that she is not able to lie down in bed, and she complains of uneasiness in the region of the heart and of palpitation. The pulse is irritable and rapid, 120. The præcordial region is prominent, and the dulness on percussion is considerably extended beyond its normal limits. A loud systolic murmur is heard over the whole space, but it is much the most intense at the apex, where also it is immediately preceded by another murmur, so as to constitute in that situation a distinctly double murmur. The systolic sound is audible also to the right of the upper part of the sternum, in the course of the aorta, and in the left dorsal region behind. There is a harsh murmur in the carotid arteries, but no continuous murmur in the jugulars. The abdomen is somewhat tumid, and the lower extremities are œdematous and pit on pressure. The urine is not diminished in quantity, amounting to three and a quarter to four pints in the twenty-four hours. It has an average specific gravity of 1020, is high coloured, and contains much deposit, soluble on being heated; but there is also a slight opacity on boiling, which is not removed by the addition of nitric acid. She was directed to take ℥xx of Tinct. Ferri Sesquichloridi, with ʒss. of Tinct. Hyosc. in f ʒj of Inf. Quassiæ, three times daily, and to have f ʒiv of wine, with beef tea and milk diet.

On the 5th November she began to suffer from febrile symptoms, with increased difficulty of breathing, pain in the chest especially on the right side, and cough and expectoration of viscid, glairy mucus, containing small air-bells and tinged with blood. On examining the chest the right dorsal region was found to sound somewhat dull on percussion, and respiration was attended by fine crepitation in that situation. The dropsical symptoms were also increasing, and the urine was more decidedly albuminous, and of a dark smoky colour. A blister was directed to be applied on the right dorsal region, and to be followed by linseed-meal poultices. The wine to be increased to f ʒvj. On the 12th she was expectorating mucus deeply tinged with blood and containing minute air-bells. The dulness on percussion was more marked, and coarse crepitation was heard over a large portion of the right back, with bronchial respiration and exaggerated cough and vocal resonance; the urine was, however, no longer albuminous. On the 19th the sputum was less bloody, and the chest symptoms were considerably relieved, and the lung had to a great extent recovered its natural condition, but the anasarca was

increasing. On the 22nd she again began to expectorate blood, and on the 26th the dulness on percussion and crepitation were more marked in the right dorsal region, and the same signs were also detected in the corresponding parts of the left side. The anasarca had become extreme, and the lower extremities were very tense, and the skin somewhat inflamed, and threatening to burst. Two punctures were made with a small sewing needle beneath and behind each knee. From them much fluid was discharged, and the tension of the limbs was considerably lessened. On the 29th, as the apertures had ceased to discharge, the punctures were repeated somewhat lower down, and again considerable escape of fluid took place, and the limbs were proportionately reduced. On the 3rd of December the left leg was cold, and there were large livid patches on the surface, extending as high as the knee; and the same condition existed to a less degree in the right leg. She was also extremely prostrated, the pulse was rapid and feeble, she suffered from extreme difficulty of breathing, and had a very troublesome cough, with copious bloody expectoration. There were signs of considerable consolidation in the right dorsal region, and to a less degree in the left. The gangrene rapidly advanced, and she died exhausted on the 4th at 3 p.m.

The post-mortem examination took place at 10 a.m. on December 5th. The brain and its membranes were healthy. The larynx and trachea did not display any appearances of disease, but the bronchi contained some bloody mucus. There was a small quantity of fluid in each pleural sac. The lungs were altogether sparingly crepitant, and somewhat œdematous. At the posterior part of the lower lobe of the right lung there were remains of pneumonic condensation, and in all parts of both lungs there were masses of pulmonary apoplexy, varying in size from half an inch to an inch in diameter. Some of them were recent, the blood retaining its colour, and the mass being firm and distinctly separated from the adjacent tissue; others were of older date, the clot being more or less decolorised, and softer and less distinctly marginated.

The peritoneum contained three or four pints of serum. The liver weighed 3 lb. 6½ oz., and the two kidneys together weighed 10½ oz., all being congested. The spleen was large, and the pancreas, supra-renal bodies, and intestinal canal, were healthy.

The pericardium contained 7 oz. of serum tinged with blood, apparently from transudation. The heart was of large size, and weighed 17¼ oz. All the cavities were distended with dark coagulated blood. There were thick deposits of old lymph in nodules and flakes upon the right auricular appendage, and some loose adhesions between the pericardiac coverings of the pulmonary arteries and aorta. There was also some fat on the surface of the right ventricle.

The left ventricle was greatly dilated, and was especially widened

at the apex. Its walls were increased in width, and the hypertrophy was very equably diffused over the parietes. The folds of the mitral valve were much thickened and indurated, the aperture was diminished in capacity, and the edges of the valves on the auricular aspect were studded with small but rough vegetations. The aortic valves were also thickened, and displayed some vegetations. The cavity of the left auricle was dilated, and the lining membrane was opaque and thick, and the walls hypertrophied. The right ventricle and auricle were both dilated, and the parietes of the former somewhat increased in width. There were some vegetations on the tricuspid valves. The muscular structure of the heart was dark coloured and firm.

The dimensions of the heart were as follows:

Girth of the right ventricle, 5 French inches and 8 lines (153 mm., 6·03 Eng. in.).

Girth of the left ventricle. 5 French inches and 3 lines (141·75 mm., 5·59 Eng. in.).

Length of the right ventricle, 4 French inches (108 mm., 4·26 Eng. in.).

Length of the left ventricle, 3 French inches and 2 lines (85·5 mm., 3·37 Eng. in.).

The walls of the right ventricle were, at the base, 2·5 lines (5·62 mm., ·22 Eng. in.) in width; at the mid point, 2 lines (4·5 mm., ·17 Eng. in.); and near the apex, 1·5 lines (3·37 mm., ·13 Eng. in.).

Those of the left ventricle were, at the base, 4·25 lines (9·5 mm., ·37 Eng. in.); at the mid point, 6 lines (13·5 mm., ·53 Eng. in.); and near the apex, 4 lines (9 mm., ·35 Eng. in.).

The aortic aperture admitted ball No. 8, indicating a circumference of 33 French lines (74·25 mm., 2·93 Eng. in.).

The pulmonic aperture admitted ball No. 10 = 39 French lines (87·75 mm., 3·46 Eng. in.).

The left auriculo-ventricular aperture admitted ball No. 10 = 39 French lines (87·75 mm., 3·46 Eng. in.).

The right auriculo-ventricular aperture admitted ball No. 13 = 48 French lines (108 mm., 4·26 Eng. in.).

To have retained the natural relation, the mitral aperture should have been sufficiently large to admit balls No. 11 or 12, or have been 3 to 6 lines more in circumference.

In this case the signs and symptoms which were observed during life led to a correct diagnosis of the conditions which were detected in the body after death.

1st. The presence of a small amount of albumen in the urine, indicated by slight opacity on boiling and the addition of nitric acid, the quantity of the secretion not being diminished and the specific gravity being natural, was ascribed to temporary congestion

of the kidneys, due to the great engorgement of the venous system; and this inference was confirmed by the disappearance of the albumen during the progress of the case and by the absence of any disease of the kidneys after death.

2ndly. The attack of pneumonia, which commenced shortly after the patient's admission into the hospital, was characterised by the ordinary symptoms, though the expectoration from the first contained more fluid blood than is usual, and this was at the time ascribed to the congestion of the lung from the mitral disease. Subsequently the sputum became and continued throughout the remainder of the patient's life very bloody, the signs of consolidation in the posterior parts of both lungs were more marked, and there were evidences of secretion in the larger tubes. These symptoms were supposed to be caused by apoplectic extravasations, and the conclusion proved in post-mortem examination to be correct.

3rdly. By far the most interesting feature of the case was, however, the existence of the præ-systolic as well as of the systolic murmur. These sounds were heard most distinctly at the apex throughout the whole of the time the patient was in the hospital, and were readily recognised by those who at different times examined the case. The former was fully explained by the contraction of the left auriculo-ventricular aperture, which, though not very marked, was sufficiently decided, and by the presence of rough vegetations on the auricular edges of the valves. The latter, which was heard towards the left axilla and at the left dorsal region, was obviously due to the aperture not being closed during the systole, from the rigidity and thickening of the curtains. It was indeed obvious that not only had the flow of blood from the auricle into the ventricle during the auricular systole been impeded, but that a portion of the blood had been thrown back into the auricle during the ventricular systole. The aortic valves were also not altogether healthy, and doubtless the systolic murmur heard at the base and to the right of the upper part of the sternum, must be ascribed to the thickening of their curtains and the vegetations on their edges.

CASE 2.—*Præsystolic murmur; very great contraction of the left auriculo-ventricular aperture from adhesion and thickening of the valves, the sequence of an attack of acute rheumatism three years previously.*—S. L—, æt. 21, a compositor, who had been working in a shop where gas is burnt all day, admitted into Edward's Ward, St. Thomas's Hospital, January the 25th, 1867, labouring under symptoms of cardiac disease of fifteen months' duration. He states that his family are healthy, his father and mother being both alive at about the age of forty, and his seven brothers and sisters all living and healthy. His illness commenced with an attack of

rheumatic fever about three years before, with which he was laid by for three months, but he is not aware that his heart was affected at the time. He was very weak after the attack, but ultimately recovered his health, and remained tolerably well till his present symptoms commenced, fifteen months afterwards.

January 31.—He complains of cough and expectoration, and says that three months ago he spat a large quantity of blood. His breathing is very short, and he has palpitation of the heart, and is becoming weaker and thinner. The chest is somewhat sparingly resonant on percussion, and does not move freely. The respirations are twenty-eight in the minute, and are attended with sonorous rhonchus, more particularly on the right side. The præcordial dulness commences at the level of the third cartilage, and becomes entire at the fourth; laterally, it extends from the middle of the sternum to the line of the nipple. There is much tenderness over the whole space. The apex of the heart beats in the fifth interspace, somewhat within the line of the nipple. At the apex of the heart there is a harsh rough murmur, which clearly commences before the systole and terminates abruptly with it; over the rest of the præcordial space the murmur is much less intense, and at the base here is no distinct murmur, but the sounds of the heart are flat. The second sound heard to the right of the upper part of the sternum is feebler than in the corresponding situation on the left side, where, indeed, it is loud and ringing. The murmur at the apex is heard somewhat to the left of the line of the nipple towards the axilla, but it is not audible in the dorsal region. The radial pulse is regular, small, and somewhat feeble—seventy-six in the minute. It very distinctly follows the murmur at the heart. The patient has a very anæmic appearance; his face is pale, and the cheeks slightly coloured and a little puffy; the lips also are somewhat pale. There is no decided œdema of any part of the body. The urine is passed in full quantity—four pints in twenty-four hours. It has a specific gravity, on the average, of 1020, and is acid, but displays no trace of albumen. He was directed to take ℥xx of Tinct. Ferri Sesquichloridi in f ℥j of Infus. Quassiæ, three times daily. To have a compound rhubarb and blue pill occasionally, and a belladonna plaster applied over the region of the heart. From this time he was repeatedly examined, and the general condition and physical signs continued much the same till the 9th of March, when, from having apparently taken cold, his breathing became more difficult, he was troubled with cough, and began to expectorate blood. On the 10th he brought up a considerable quantity of blood, and subsequently the expectoration assumed the characteristic pneumonic character, being minutely intermixed with air and very viscid. The resonance on percussion in the dorsal regions became dull and sibilant, and sonorous rhonchi were heard

there, and to a less degree in all parts of the chest. The urine also became scanty and contained much albumen. The iron was discontinued, and he was directed to take sulphuric acid with magnesiæ sulphas in infusion of roses every four hours, and to have a blister applied between the shoulders.

On the 21st the symptoms were very much relieved, and all traces of albumen had disappeared from the urine, but he still continued to suffer from difficulty of breathing, cough, and expectoration. In a short time he was able to leave his bed and walk about the ward. When, however, the severe weather set in at the beginning of May he was again taken worse, and the patients being at the time removed to the tent for the annual cleansing of the wards, he suffered severely from the cold, and he was in consequence presented at his own desire on the 16th of May. Shortly after, his father applied to me, stating that he was much worse, and he was kindly visited at my request by Mr. Power, the resident medical officer of the Victoria Park Hospital. He found him so ill that he was at once ordered for admisslon into that institution. I saw him there on the 29th, at which time the following notes were taken :

His condition is much altered for the worse since he left St. Thomas's Hospital. His face is very puffy, and he is somewhat jaundiced, and there is considerable œdema of the lower extremities and some ascites. His breathing is very difficult, and he has much cough, and expectorates a small quantity of viscid bloody sputum. He passes only about a pint of water, but it is not albuminous. The præcordial dulness is considerably increased in extent. It begins at the level of the second cartilage, and becomes entire shortly below that point, and it extends laterally from the right side of the sternum to beyond the line of the nipple. The action of the heart is so irregular that it is difficult to distinguish the sounds, but between the sternum and nipple there is a murmur which may be either præsystolic or systolic. At the base of the heart the sounds are very indistinct, but there is no murmur to be heard either there or in the left dorsal region. The radial pulse is small, feeble, and unequal both in force and frequency. Posteriorly some irregular crepitation is heard, and especially low down on the right side. The hepatic dulness extends high into the chest, and the liver can be felt considerably below the edges of the ribs, and it is very tender to the touch. He died somewhat suddenly on the following morning.

The post-mortem examination took place the same day.

Fluid was contained in each pleural sac to the amount of upwards of a pint. The right lung weighed 2 lb. 1½ oz., the left 1 lb. 12 oz., and both were greatly congested and œdematous, but were otherwise healthy. The heart immediately on removal weighed 20 oz., but when washed and deprived of coagulum it weighed only 18 oz. There was a loose mass of false membrane at the apex of the left

ventricle, and several white patches in the course of the vessels on the surface of the right ventricle and on the appendix of the auricle, and the serous coverings of the aorta and pulmonary artery were adherent. The cavity of the right ventricle was enlarged, but the walls were not materially increased in thickness. The left ventricle also was dilated, and the walls were somewhat increased in width. The left auriculo-ventricular aperture was so greatly contracted that it would only admit the point of the little finger, and the valves were greatly thickened and indurated, and the tendinous cords shortened. The left auricle, and especially the sinus, was dilated, and the whole was distended with coagulum so as to form a firm solid mass. The walls of the auricle were thickened and indurated, and had in places undergone the fibro-cartilaginous transformation. The coagulum was partly laminated, and on the outer side, where in contact with the auricular walls, was decolorised, while on the inner side the coagulum was dark and loose. The dimensions of the organ were as follows:

The girth of the right ventricle, measured externally, was 6 French inches (162 mm., 6·39 Eng. in.), that of the left 5 inches and 6 lines (148·5 mm., 5·86 Eng. in.).

The length of the right ventricle was 4 inches and 6 lines (121·5 mm., 4·79 Eng. in.). The length of the left ventricle 3 inches and 3 lines (87·75 mm., 3·46 Eng. in.).

The thickness of the walls of the right ventricle was 1½ line (3·37 mm., ·13 Eng. in.). The thickness of the walls of the left ventricle was at the base 4½ lines (10·12 mm., ·39 Eng. in.), at the mid-point 5½ lines (13·37 mm., ·48 Eng. in.), and at apex 2½ lines (5·62 mm., ·22 Eng. in.).

The aortic aperture admitted ball No. 10, indicating a circumference of 39 French lines (87·75 mm., 3·46 Eng. in.).

The mitral aperture admitted ball No. 3 = 18 French lines (40·5 mm., 1·59 Eng. in.).

The pulmonic aperture admitted ball No. 11 = 42 French lines (94 mm., 3·72 Eng. in.).

The tricuspid aperture admitted ball No. 14 = 51 French lines (114·75 mm., 4·52 Eng. in.).

The peritoneum contained a considerable quantity of fluid tinged with bile.

The liver weighed 3 lb. 2 oz., and was greatly engorged. The spleen was congested and weighed 7 oz. The kidneys together weighed 10 oz., and were somewhat engorged.

In this case, also, the diagnosis was correctly effected during life. At the time the patient was first seen it was quite obvious that the murmur heard immediately preceded the systole of the ventricle; and this observation was repeatedly confirmed, both by myself and others, during the period that he was at St. Thomas's Hospital.

Indeed, it was only at the latter period of his life, when the circulation had become greatly embarrassed and the action of the heart was irregular both in force and frequency, that the time at which the murmur was heard became doubtful. The murmur was also heard only over a somewhat limited space around the apex; and it could not be detected at the base or in the left dorsal region—circumstances which equally indicated obstruction to the flow of the blood from the left auricle into the ventricle. It will further be observed that the general condition of the patient when he was first under treatment, the absence of marked symptoms or signs of pulmonary or systemic engorgement, the anæmic and bloodless appearance, and the freedom of respiration and absence of dropsy, equally tended to the conclusion that the mitral disease was obstructive rather than regurgitant. The condition of the organ after death was entirely in accordance with the inference which had been drawn during life as to the nature of the disease. The mitral orifice was very small. It was evident that the blood could only have been at all adequately transmitted through it while the heart retained its full power and the action was regular, and then only with great effort; and it was also obvious that no material regurgitation could have taken place. Subsequently symptoms of pulmonary engorgement and dropsy supervened, but only when the power of the heart was become greatly impaired, and altogether unequal to transmit the blood from the auricle into the ventricle. It was doubtless at this period that the thrombosis, or, as it has been termed, the aneurism of the left auricle, took place; and this must have very greatly added to the obstruction which previously existed.

Though the stress of the disease had chiefly fallen on the left auricle and right side of the heart, yet, from its long duration, the left ventricle had also become involved, and the heart altogether had attained a great size and very unusual weight for the age of the subject. The lungs and abdominal organs, though not materially diseased, were very much engorged; and this congestion had, doubtless, occurred during the latter period of the patient's life.

The alterations in the condition of the heart in the two cases will be much more obvious by the following statement of the weights and the dimensions of the constricted orifices compared with those of the healthy organ. The latter are taken from my own observations, published in the 'Edinburgh Medical Journal' for 1854, of which an abstract is given in the Croonian Lectures for 1865.

Case 1.—A female, æt. 20. Weight of heart, 17¼ oz. av. Circumference of the left auriculo-ventricular aperture, 39 French lines = 87·75 mm., or 3·46 Eng. in. Average weight of the healthy heart in adult females, 8 oz. 13 drachms. Average capacity of left auriculo-ventricular aperture in healthy females, 45 lines = 101·25 mm., or 3·99 Eng. in.

Case 2.—Male, æt. 21. Weight of heart, 18 oz. Circumference of left auriculo-ventricular aperture, 18 lines = 40·5 mm., or 1·59 Eng. in. Average weight of healthy heart in adult males, 9 oz. 8 drachms. Circumference of left auriculo-ventricular aperture in healthy males, 45·2 lines = 101·7 mm., or 4 Eng. in.

In both cases the weight of the heart was greater than it usually is in cases of mitral valvular disease. This in the first case was due to the aortic valves also being in some degree diseased, and to the disease having been of long duration in the second. In the first case, also, there was regurgitation as well as obstruction, and the left ventricle being in such cases hypertrophied and dilated, the organ is larger and heavier than in cases of simple obstruction. The mitral aperture was extremely small in the second case. I have only in two cases known the circumference less than eighteen lines, and in one of those the capacity was only twelve lines.

These cases are by no means the only ones in which I have, within the last few months, distinctly detected the existence of a præ-systolic or direct murmur, either existing alone or in connection with a systolic or regurgitant murmur. I have recently met with some cases, both at St. Thomas's and the Victoria Park Hospitals and in private practice, in which I have diagnosed, from the time at which the murmur was heard, the existence of mitral obsruction; but I refrain from quoting such cases, as the correctness of the conclusion must, of course, be problematical. My more recent experience, however, leads me to agree with Dr. Gairdner in regarding such murmurs as not of uncommon occurrence, and as affording the only conclusive proof that the disease is primarily mitral; but I do not equally agree with him in "thinking that they are among the most easily detected of all the cardiac murmurs." I am, indeed, much more disposed to unite with Dr. Andrew[1] in regarding "the diagnosis of a præ-systolic murmur as one of the most difficult tasks in the physical examination of the heart"—at least in a considerable proportion of the cases which fall under our notice in hospital practice, and at the latter periods of the disease, when alone the correctness of the diagnosis can be confirmed or disproved. In the conclusion here expressed Dr. Hayden seems also to concur. That, indeed, this is the more correct statement must, I think, be admitted, when it is considered, that notwithstanding the amount of careful observation which has been devoted to the diagnosis of heart affections by some of the ablest physicians of our day, the direct or præ-systolic mitral murmurs have either been almost entirely ignored or regarded as of very rare occurrence. When there is only one murmur heard it is often very difficult to decide whether it precedes

[1] 'St. Bartholomew's Hospital Reports,' 1865.

or accompanies the systole, though in many cases the direction in which the murmur is propagated and its character will afford considerable assistance in the diagnosis. Thus, an obstructive murmur is heard at the apex, and extends thence towards the lower end of the sternum; while a regurgitant murmur extends from the apex towards the left axilla, and is usually heard in the left dorsal region. The former also, as pointed out by Dr. Gairdner and Dr. Hayden, is harsh and short, and terminates abruptly; the latter, on the contrary, is generally soft and blowing, and gradually dies away.

While, however, the præ-systolic murmur affords the most conclusive sign of the existence of mitral obstruction, the coincident changes in the state of the heart, and the collateral effects upon the lungs and general system may materially aid the diagnosis.

1. In cases of mitral obstruction the disease is usually slowly induced, and the main stress of the defect falls upon the left auricle and the right side of the heart. The auricle becomes dilated, its lining membrane thickened, and the muscular walls hypertrophied; the pulmonary artery also expands, and the right ventricle and auricle enlarge, and the walls of the ventricle become peculiarly firm and resistent, and considerably increased in thickness. The left ventricle, on the contrary, often does not undergo any material alteration, either in the thickness of its walls or in the capacity of its cavity, unless, indeed, there be coincident disease of the aortic valves, or the mitral disease be of long duration. Indeed, it has been supposed, though most probably erroneously, that in some cases the left ventricle undergoes an absolute decrease of size and strength. There is, however, usually a very marked difference between the firm leathery feel of the walls of the right ventricle and the soft and flaccid state of those of the left ventricle. Not only also is the left ventricle not materially dilated, but its cavity is often much encroached upon by the protrusion into it of the united and expanded mitral valves, so that its capacity becomes very small. The aortic orifice is also usually in cases of mitral valvular obstruction of small capacity, and strikingly so as compared with the dimensions of the orifice of the pulmonary artery.

Frequently the left auricle is not only dilated and its walls hypertrophied, but their muscular structure undergoes, to a greater or less extent, the fibro-cartilaginous transformation, and the cavity is more or less occupied, as in the case last related, by decolourised and laminated coagula, similar to those found in aneurismal sacs. To this condition the term of aneurism of the auricle has been given. Cases of the kind were, I believe, first related by Mr. Burns[1] and Mr. Abernethy[2] at the commencement of the present century, and others have since been

[1] 'Diseases of the Heart,' &c., 1809, p. 35.
[2] 'Med.-Chir. Trans.,' vol. i, 1809, p. 32.

placed on record by Hodgson,[1] Elliotson,[2] Davies,[3] Hope,[4] &c. Dr. Thurman[5] has described a specimen then contained in Mr. Langstaff's museum, and others are mentioned as existing in the museum of St. Bartholomew's Hospital by Mr. Stanley. Cases are also described in the 'Pathological Transactions' by Mr. Prescott Hewett[6] and Dr. Bristowe,[7] and I have myself related one in a paper on "Aneurisms of the Heart" in the 'Edinburgh Medical and Surgical Journal.' In all these cases the mitral aperture was contracted. In some the thrombosis occupied the sinus, in others the appendix of the auricle, and in yet others, as in that here related, both the appendix and sinus were involved. The preparations of the cases described by Dr. Elliotson and Dr. Bristowe are contained in the museum of St. Thomas's Hospital. My own specimen is in the Victoria Park Hospital museum.

From the changes which have been described several important indications of the nature of the disease may be deduced. The præcordial dulness on percussion in cases of mitral obstruction is not generally much extended to the left of its normal limits, but is wider on the right side than it should be. The sounds of the right side of the heart are also louder, and the action more powerful; and especially the sound of the pulmonic valves, heard immediately to the left of sternum at the level of the third cartilage, is very loud and ringing; and often presents a striking contrast to the sound of the aortic valves, heard at the corresponding point on the right side of the sternum. The apex of the heart generally beats in the fifth interspace and within the line of the nipple, and occasionally it is attended by a purring tremor. From only a small portion of blood being received into the left ventricle and impelled thence into the aorta, the radial pulse is small and not infrequently somewhat quick, as the heart appears to act more rapidly, in order to compensate for the smallness of the current which it is able to transmit with each contraction. Usually also the pulse, though small, is somewhat firm; and it does not generally become irregular till at the more advanced periods of the disease.

In cases of mitral regurgitation, on the other hand, the most marked changes occur in the left ventricle and auricle. These cavities become greatly increased in capacity, and the ventricle especially is widened and rounded at the apex; the walls at the same time generally retaining their normal width or being thinner than

[1] 'Diseases of Arteries and Veins,' 1815, p. 83.
[2] 'Lumleyan Lectures,' 1830, p. 29.
[3] 'Diseases of Lungs and Heart,' 1835. Probably the case described by Dr. Thurnam.
[4] 'Diseases of Heart,' etc., 2nd ed., 1839, p. 574.
[5] 'Med.-Chir. Trans.,' vol. xxi, 1838.
[6] Vol. ii, 1848-49; 1849-50, p. 193.
[7] Vol. xi, 1860, p. 65.

natural. The right cavities also become dilated, but the parietes of the right ventricle are not generally materially increased in width. Hence the dulness on percussion, though it may be extended to the right, is especially widened on the left side; and the impulse of the heart is usually felt in the line of the nipple or to the left of the line; generally also it is diffused so that it can be seen and felt over a large space, and often the precise point at which apex beats cannot be felt. Though the auricle is of large size, yet, as only a small portion of the blood which it receives is thrown into the aorta, while the other, and perhaps the larger, portion regurgitates into the auricle, the pulse at the wrist is small, and from the thinness and feebleness of the ventricular walls, weak. The action of the heart also, from the same cause, rapidly becomes irregular.

2. In cases of mitral obstruction the lungs are in a state of permanent engorgement, yet as this condition is only slowly brought about and does not generally obtain to any great degree except in cases which are of long duration, the pulmonary vessels undergo gradual dilatation and are able to accommodate themselves to the altered condition; apoplectic extravasations do not therefore readily occur, nor does the pulmonary tissue become œdematous, or the bronchial mucous membrane inflamed. The respiratory movements, therefore, though short and hurried, are not greatly embarrassed, nor do the patients usually suffer much from cough or expectoration.

In cases of mitral regurgitation, on the contrary, the valvular defect is often rapidly and sometimes suddenly developed, and the engorgement is not continuous but intermittent; the vessels in the lungs are therefore very apt to give way under the sudden and extreme distension to which they are subjected and apoplectic extravasations occur, the lung tissue readily becomes œdematous, and bronchitis and pneumonia frequently occur. Hence the breathing is usually extremely difficult and laborious; and the patient is troubled with cough and copious expectoration, sometimes thin and watery and much mixed with air, at others viscid and solid, and generally more or less bloody.

3. In cases of mitral obstruction the increased power of the walls of the right ventricle enables it to resist for a long time, so that the effects of the obstruction are to a considerable degree limited to the lungs; and it is only when the resistance of the right ventricle has been overcome that the systemic vessels and parenchymatous viscera become engorged. Hence the functions of the liver, kidneys, and brain are not at the earlier period of the disease materially disordered, and dropsical symptoms do not quickly supervene. In mitral regurgitation this protective influence does not come into play and the system generally becomes greatly and rapidly engorged, the veins of the neck are dilated and pulsate, the liver becomes enlarged, and the kidneys and brain engorged. Hence the

rapid occurrence of dropsy, often with jaundice, and generally with albuminous urine; and the œdema becomes marked in the face and upper extremities as well as in the abdomen and lower extremities.

4. The aspect of the patients with the two forms of mitral disease is generally very different. In cases of mitral obstruction the patient is usually very pallid and anæmic looking, with, perhaps, a slight flush in the cheeks. His countenance bears, indeed, a general resemblance to the appearance of a patient with regurgitant disease of the aortic valves, with, however, this marked distinction, that while in the latter affection the face is generally thin and the expression of countenance anxious, in the former the face is usually somewhat full or puffy looking, and the expression heavy and oppressed. The aspect in obstructive disease is, however, very different from the heavy and bloated and livid countenance which is so strikingly characteristic of mitral regurgitation. It must, however, be understood that these distinctions apply to characteristic cases of the two forms of disease; not, of course, to those in which there is both obstruction and regurgitation, nor yet to the advanced stages of obstructive disease in which the power of the heart is giving way and the circulation is becoming greatly embarrassed.

It may, however, be asked, why thus endeavour to distinguish two affections which are so closely allied, and are so often found to coexist? Independent, however, of the advantages of attaining the greatest accuracy of diagnosis, there is no doubt that practical usefulness will result from our being able to distinguish in the earlier stages of disease between a case of obstructive and one of regurgitant disease. No careful observer can doubt that the prognosis in the similarly related affections of the aortic valves is different, and that the treatment requires equally to be modified; and there seems every reason to believe that the distinction of the two forms of mitral disease must possess equal and similar practical importance.

In reference to the prognosis, the immediate danger in obstructive disease of the mitral valves is less that in regurgitant disease; for the condition being generally slowly induced and only attaining an aggravated degree after the lapse of a long period, the heart is able to accommodate itself to the altered circumstances and the lungs and system at large are less seriously involved. In mitral regurgitation, on the contrary, the condition is often rapidly and sometimes suddenly brought about; the heart, therefore, is not able to resist the altered condition, the lungs and general system become quickly involved, and the patient is exposed to great and immediate danger. In the former affection the patient suffers chiefly from faintness and debility and the tendency is to death from asthenia. In the latter he complains of oppression and sense of suffocation, and death usually occurs from apnœa. This is due especially to the secondary affections of the lungs and the dropsical symptoms,

but the patient may also die comatose, owing to the brain being congested and the blood loaded with effete materials from the disordered functions of the liver and kidneys. It must, however, be borne in mind that obstructive disease is necessarily a permanent condition; whereas regurgitation may only be a temporary defect, due to maladjustment of the valves from distension or irregular action of the heart. Another source of danger which threatens in both these forms of disease is embolism from the deposition of fibrinous clots on the valves or lining membrane of the auricle, and the conveyance of portions of separated clot into some of the smaller vessels which may so become obstructed. I do not know that in this respect there is much, if any, difference in the two forms of disease.

The difference in the modes in which obstructive and regurgitant disease tend to terminate will also necessitate in general somewhat different methods of treatment. In both forms of disease it is of essential importance to avoid as far as possible all causes which can add other sources of embarrassment of the circulation to those which are already in operation; and we must endeavour to prevent the occurrence of pulmonary complications, to maintain the healthy performance of the functions of the liver and kidneys, and to uphold the overtaxed power of the heart. In cases of obstruction, however, the latter requirement becomes of paramount importance, and our chief aim must be to aid the heart to propel the blood through the constricted orifice. In cases of regurgitant disease, on the other hand, the predominant sources of danger are the engorgement of the lungs and of the parenchymatous viscera, and our chief attention must be directed to the relief of such conditions. The former object may be best effected by a tonic and analeptic course of treatment, and by remedies calculated to strengthen the muscular structure of the heart and quiet its action, and of these iron is by far the most effective. The latter intention must be carried out by the use of mild, alterative, and eliminant medicines, and especially by aperients and diuretics, and for this purpose digitalis is probably one of the most effective remedies which we can employ. In both forms the strength of the patient must be upheld by light and easily digestible and nutritious food, and by the exhibition of stimulants, preference being shown rather to the use of wine and spirits than of malt liquors.

Art. IV.

Some Remarks on Maniacal Chorea and its probable Connection with Embolism. Illustrated by a Case. By H. M. Tuckwell, M.D. Oxon.; Physician to the Radcliffe Infirmary; Consulting Physician to the Warneford Lunatic Asylum, Oxford; late Radcliffe Travelling Fellow.

W. E—, æt. 17, was admitted into the Radcliffe Infirmary on April 24th, 1867, under the care of Dr. Acland (to whose kindness I am indebted for permission to note the most prominent symptoms of the case, assist at the examination of the body after death, and publish the case in illustration of the remarks I have to make). The boy who had been addicted to masturbation, and believed himself that his present illness had been brought on by that vicious habit, was reported by his mother to have been attacked with St. Vitus's dance nine days before admission, and, within the last few days, to have become unmanageable, and, at times, violent. He had never before had a similar attack, nor had he suffered from rheumatism in any form whatever. Neither she herself nor any other of his relations had, as far as she knew, ever had rheumatism; but her own sister had had St. Vitus's dance of so furious a kind that she was taken to an asylum, where she died. The boy on the day of admission was observed, as he sat in the hall, to be suffering from severe chorea, but he could walk and was quite conscious. *A musical systolic murmur was plainly heard over the cardiac region.* He was sent to bed, and ordered Liq. Potass. Arsen., ♏ v, ex aquâ, sextâ quâq. horâ; milk, beef-tea. In the course of the same night he became delirious, and, getting out of bed, wandered about the ward. A solution containing Morph. gr. $\frac{1}{3}$, was twice injected beneath the skin of the arm in the course of the night, with the effect of making him quieter but not giving him sleep.

April 25th.—He is now delirious and affected with severe chorea. His muscles are remarkably well-developed for his age, and the appearance of his body generally is that of vigorous health. Pulse 108, moderate volume. Tongue moist but coated. Skin rather hotter than natural. *A systolic murmur, blowing but no longer musical, is plainly heard, seemingly louder at the apex than at the base.* Urine passed involuntarily in bed.

26th.—He has passed another very restless and sleepless night, two more subcutaneous injections of morphine being made without effect. He is now in a state of wild maniacal

delirium, singing, shouting, talking incoherently, struggling violently, and constantly jumping out of bed; the whole being accompanied with furious choreic movements of the face, arms, neck, and trunk, while the legs are comparatively quiet. The arms and trunk are covered with several bruises and excoriations. Lips covered with sordes; tongue dry. Pulse 130, weak. Skin hot. Urine still passed in bed. No motion. No auscultation can be made.

Prætermit. Mist. Arsenic.—Sumatur Extr. Cann. Ind. gr. ½, sextâ quâq. horâ.

27th.—He is less disturbed, partially conscious, so that when spoken to he tries, though ineffectually, to put out his tongue, which is very dry. There is still twitching of the arms and neck and occasional grinding of the teeth, but the wild delirium of the previous day has ceased. Urine still passed involuntarily; no motion. Careful auscultation of the heart is again made, *when it is found that the systolic murmur has entirely ceased, and that both sounds are quite clear.*

Pt. in usu Extr. Cann. Ind. tertiâ quâq. horâ; Sumatur Spir. Vin. Gall. ʒss, tertiâ quâq. horâ; Injic. Enema Terebinth. statim.

28th.—He dozed a little in the night and again this morning. He now lies quiet with the eyes wide open and sunk in their orbits; the pupils being very dilated and acting but sluggishly when a candle is held before the eyes. He tries still ineffectually to put out his dry tongue when told to do so, but seems otherwise unconscious of what is going on around him. There is a little twitching in the muscles of the face, arms, and hands, but the legs are quite quiet. The breathing is sighing and irregular. Pulse 128, regular, but very feeble. Skin hot. Urine still passed in bed. Bowels acted freely after the injection. *No murmur can be detected.*

29th.—Has dozed occasionally, but has had no sound sleep. Lies quite tranquil and unconscious. Eyes wide open and deeply sunk in their orbits; the pupils still dilated but less so than yesterday. Lips very dry and covered with sordes. Tongue dry and hard. Pulse 120, regular, but very small. Skin natural temperature. No paralysis in the face, but a strangely pinched, almost senile appearance in the features. Breathing regular but frequent. Abdomen flattened and hard. Both food and medicine return from the corners of the mouth. Emaciation remarkable. Urine passed in bed. *No murmur can be detected.*

May 1st.—He continued in much the same state till last night, when he again became maniacal. This morning he is comatose, and apparently moribund. Eyes half-closed and

eyeballs rolled upwards. Breathing very rapid and stertorous. Emaciation extreme. He died the same evening.

2nd.—*Autopsy*, sixteen hours after death, in warm weather. Post-mortem discoloration well marked in all the depending parts of the body—the face, front of thorax, abdomen, front of thighs (the body having been turned over on its face a few minutes after death, and kept in that position till the examination was made, in order to avoid any errors of observation from hypostatic congestion of the cord). Blood generally fluid. Decomposition already commencing in the organs of the thorax and abdomen. Excoriations on the arms and trunk. In removing the skull-cap some tough adhesions connected the dura mater with the bone at one point, just to the left of the longitudinal sinus, where some unusually firm and prominent Pacchionian bodies sprouted out. A little farther back, a short distance above the torcular, there projected from the outer layer of the dura mater a small bony plate about half an inch in length and a quarter in breadth. The arachnoid was everywhere turbid from the presence of an abnormal quantity of subarachnoid fluid. The *Brain* was remarkably well developed and weighed fifty-five ounces. The surface of the cerebral hemispheres being next carefully examined, there was noticed in the middle of the upper surface of the right hemisphere a round patch, about as large as a crownpiece, which was distinguished from the surrounding grey substance by its bright rose-red colour. On cutting into it, the grey substance within the borders of this patch right down to the bottom of the deepest sulci was much softened, and had the same bright, rose red-tint. The whole was dotted with numerous punctate spots of deeper red, and presented a perfect specimen of acute or red softening, limited to the cortex and not extending into the subjacent white substance. At the under and outer aspect of the posterior lobe of the same hemisphere was an exactly similar but larger patch of red softening, involving in like manner the cortex, but extending also a short space into the medullary substance of the hemisphere. I afterwards carefully dissected out the posterior cerebral artery which led in the direction of this latter patch, and came at length upon one of the principal branches which ran straight into the midst of the softened mass. This branch was completely occluded by a firm and dark coagulum, which, being followed up, became continuous with a small, white, tough, fibrinous concretion, tightly wedged into the vessel at an angle where it subdivided into three smaller branches. Beyond this concretion, and extending down each of the branches in question were other firm dark coagula identical in character and appearance with that on the proximal side of the concretion,

and imbedded in the softened cerebral substance. The contrast between the white concretion and the black coagula before and behind it was most striking. On returning now to the former of the two softened patches and searching as before, I came upon an arterial branch in the midst of the patch which was filled out with a long, firm, worm-like coagulum, just as in the other patch; but the part immediately beyond this had been so cut and sliced in the previous examination that I was unable to discover the embolus which had, in all probability, been connected with this clot, as with the other, and caused the softening. The anterior pillars of the fornix were unusually soft; but, with this exception, although every part of the cerebral hemispheres, of the corpora striata and optic thalami, with the brain-substance round and about them, of the crura cerebri, of the cerebellum, of the pons varolii and medulla oblongata, was minutely sliced and closely examined, nothing whatever abnormal could anywhere be found. The medulla oblongata in particular had a remarkably healthy appearance. Microscopical examination of the softened cerebral grey matter showed a fine granular degeneration of the nerve substance, and very fine granular particles dotted along the small bloodvessels, part in clusters, part diffused.

The Spinal Membranes and Nerves were perfectly healthy. No thickening round the nerves; no ecchymosis; no congestion of the membranes could, though sought for, be found. Portions of the *spinal cord*, from the cervical, dorsal, and lumbar regions, were prepared for minute examination by hardening in chromic acid. The rest of the cord was placed in strong spirit. On making sections of these different portions after several weeks' immersion, it was found that, whereas the upper part of the cervical and the whole lumbar region were throughout firm and hard, so that perfect sections could be made with a Valentin's knife, the lower cervical and the whole dorsal region were softened centrally, so that complete sections could hardly be made. In the middle dorsal region, indeed, this was impossible; for the centre was quite diffluent and could not be cut, but broke away before the knife, leaving a ragged hole in the middle of the section. Some of this diffluent matter removed and examined microscopically, was seen to be composed mainly of fatty granules and globules of different sizes. The several sections were next, with Mr. Charles Robertson's kind assistance, stained with carmine, soaked in turpentine, and mounted in Canada balsam. They now showed, when examined under the microscope, that this softening affected the posterior columns of the lower cervical and all the dorsal region at their most central part,—that part, namely, which on transverse section appears as the thin edge of

the wedge, and is situated immediately behind the grey commissure. There remained here little else than the connective tissue of the cord, the nerve-substance having almost entirely disappeared. The sections from the upper cervical and lumbar region were, on the contrary, quite normal from their circumference right through to their centre. The grey substance was everywhere natural. In the most diseased specimen of all the dorsal sections an appearance presented itself, in one of the bloodvessels that traversed the softened portion, so like in miniature to that which was present on a larger scale in the brain, that it was impossible to resist the conviction that a minute embolus was lodged in this little vessel—one of the smallest arteries. This vessel may be seen in the preparation (now in my possession) springing from a larger branch, which has been divided longitudinally, and running into the midst of the diseased tissues. At a short distance from its origin it bifurcates; and, just at the angle of bifurcation, is evidently obstructed by a solid body which completely blocks up its calibre. The two branches beyond the obstruction can with a higher power (Powell and Lealand's $\frac{1}{5}$th), be traced distinctly for a short distance into the softened substance by which they are surrounded and in which they lose themselves. The appearance of this little bloodvessel is all the more striking in that there are many bloodvessels of different sizes to be seen ramifying among the healthy tissues of the cord in the various sections made, but they are all empty; nowhere is there any trace of obstruction like that here described.

The Heart.—Pericardium healthy. On cutting through the large vessels in order to remove the heart, a quantity of black fluid blood gushed from their mouths. The walls of the left ventricle, which was firmly contracted, were healthy; the muscular substance ruddy and firm. The ventricle being laid open in the usual manner by a V-shaped flap, and the incision along the septum prolonged upwards into the aorta, the aortic valves appeared perfectly healthy. The mitral valve, looked at from its ventricular aspect, seemed perfectly competent and presented no unusual appearance; but, examined from its auricular aspect, by opening out the left auricle, it was altered as follows:—A vascular, velvety line of redness, extended in a sinuous course all along the free margin of each cusp and served as a base for numerous, bright, warty vegetations, which stood out in relief, some as large as a pin's head, others so minute as to be only just visible to the naked eye and that in a certain light, but shining like little white beads when slightly magnified. In two or three places these little growths appeared to have been shed off, for they hung only by a fine pedicle to the line of attach-

ment and clung close to the chordæ tendineæ. The majority, however, were firmly fixed to the valve and seemingly grew from it. On one of the cusps of the tricuspid valve, the same vascular line, though less distinct, was seen to be present; and sprouting from it in a similar way were three or four vegetations rather larger than those on the mitral valve.

The Lungs.—On tracing up the branches of the pulmonary artery to their smaller divisions, I came upon numerous concretions which were in all cases wedged into the vessel just at an angle of branching; every concretion having at its proximal and distal end a firm, long, black clot, just as in the brain. The parts of the lung in the direction of which these clots were traced were congested in patches, but no more than this, neither consolidated nor degenerating.

The Kidneys were congested; their capsule stripped off readily. Sections through the cortical and medullary substance brought to light numerous minute, tough, granular concretions, which protruded from the cut mouths of the smaller arteries. These concretions could be picked out of the vessels with fine forceps, and were evidently little emboli exactly similar in naked-eye characters to the growths on the mitral valve, and to the emboli in the pulmonary and cerebral arteries. In some of the places where these little bodies were lodged, there were small patches of congestion round and about them; but there was nowhere anything like the yellow, wedge-shaped spot characteristic of renal embolism. It seemed, indeed, that here, as in the lungs, the transmission from the heart had been too recent to allow of the changes which naturally follow on embolism. The several concretions from the different arteries of the body were now prepared for the microscope and examined side by side with the vegetations from the cardiac valve. The appearances in all were identically the same, an amorphous granular substance, with here and there faint traces of fibrillation.

The *Spleen* was small, and, though minutely examined, bore no signs of embolism. The same may be said of the *Liver*, which was healthy.

Remarks.—My attention was first called to the peculiar appearance of the cardiac valves in fatal chorea in the year 1861, when I watched in the Hôtel Dieu, under Professor Trousseau's care, a terrible case of the kind in a woman aged twenty-four, in whom no history of rheumatism could be traced nor cardiac murmur detected, and who died rapidly. At the post-mortem examination nothing particular was noticed till the heart was examined, when there was disclosed a condition of the mitral valve, the description of which might fairly be given

word for word as in the boy's case above narrated.[1] The whole thing was a mystery. A year later, in the summer of 1862, I was fortunate enough to observe another similar case in a young woman, aged twenty, four months pregnant, who lay in one of the wards of the General Hospital at Vienna. The chorea was, I well remember, attributed to her pregnant state; and she died in spite of all remedies. The pericardium, as above, was healthy: but there, sprouting from the mitral and aortic valves, were the shining vegetations just as in the previous cases. The uterus contained a fœtus in the fourth month of development. Seeing how exactly the appearance of the heart in this woman who was pregnant tallied with that in the French woman who was not pregnant, it was hard to believe that the pregnancy was the cause of the chorea, and it was impossible to resist the conviction that there was somehow or other a relation of cause and effect between the heart disease and the chorea. But what was that relation? Dr. Bright endeavoured, as is well known, years ago, to explain the connection between the two, in a "Report of Three Cases of Spasmodic Disease accompanying Affections of the Pericardium;" in which paper he regarded *pericarditis* as the cause of chorea, by irritation propagated from the inflamed pericardium to the phrenic nerve, conducted by the afferent fibres of that nerve to the spinal cord, and thence violently reflected along the motor nerves to the muscles of the body. It is a remarkable fact that in the very case which suggested to him this ingenious interpretation of a difficult question, the selfsame appearances are reported to have been present on the *endocardium* as in the three cases already mentioned. His words run thus:—"The semilunar and mitral valves had each a fringe of vegetations, doubtless rather recent, forming a raised irregular line along the auricular side of the mitral and the aortal side of the semilunar." No stress whatever was laid on this, but the whole importance of the case was centred in the pericarditis which was also present. Now, in the three cases that I have brought forward, *endocardial* disease was similarly present in all, but in none was there a sign of *pericarditis*. Seeing this, and seeing that, as I shall presently show, pericarditis is, compared with endocarditis, a rare phenomenon in fatal chorea, Dr. Bright's theory failed to solve the riddle. Dr. Burrows, in the last chapter of his work on "The Connection between Affections of the Heart and Diseases of the Brain," accepted, and still further developed, this reflex theory of Bright's. He supposed that the pneumogastric nerve might share with the phrenic the office of conducting the irritation from the heart to the cord; so that any part of the heart,

[1] This case has since been published in Trousseau's 'Clin. Méd.,' ed. 2, vol. ii, p. 181.

and not the pericardium alone, might, when diseased, act on the cord as a source of irritation. This theory had certainly much to recommend it, and might have been accepted as complete and satisfactory, if only it could have been shown that in all examinations of bodies which had died of chorea no other lesion was present except the cardiac disease. It seemed, however, to be insufficient on this account,—that in many cases of fatal chorea, positive, sometimes extensive, disease of the nervous centres themselves, in the shape of softening, had been found present. In such instances as these it was evident that there must have been something more than a mere peripheral excitement of some nerve or nerves connected with the spinal cord. The nerve-centres themselves were manifestly diseased, and the reflex theory did not accord with the post-mortem appearances. The chorea was more likely to have been centric than eccentric. The theory which met with the sanction of Drs. Todd, Begbie, and Watson, and which more recently has been accepted by Dr. Handfield Jones,[1]—that the cardiac disease, the chorea, and the delirium, when present, must all be regarded as the effects of one common cause, rheumatism—had also much that was reasonable on its side. But, although the intimate relation of chorea with rheumatism might well be considered as an established law, yet in many cases of fatal chorea where the clinical history was recorded, the entire absence of ordinary rheumatic symptoms in the patient, and of rheumatic history both in the patient and his relatives, was incontestable. Again, the softening of the brain and spinal cord remained equally unaccounted for by this as by the preceding theory. While it was known that the rheumatic poison, whatever that might be, attacked by preference, and almost exclusively, the fibrous tissues of the body, yet inflammation or thickening of the fibrous membranes investing the brain or spinal cord was of so rare occurrence in deaths from chorea (see Analysis), that the chorea could not be explained in this way. Were the rheumatic poison a direct agent in the production of chorea, one would have naturally expected to trace signs of its presence in the cerebral and spinal meninges, rather than in the soft parenchyma of the brain and spinal cord themselves. But post-mortem evidence showed that, while softening of the nerve-tissue was by no means uncommon, thickening of the meninges was quite exceptional. Besides, the regular appearance, case after case, of the warty vegetations on the cardiac valves seemed to imply that there must be some more immediate and special connection between these peculiar little growths and the choreic symptoms than had as yet been discovered. In 1863, two very remarkable papers published by

[1] 'Brit. Med. Journ.,' 1866, p. 496.

the late Dr. Kirkes[1] offered at last a solution of the difficulty by far the most convincing that had yet been proposed. He had been long struck with the constant coincidence of chorea and valvular disease; and, happening at length on a case where groups of ecchymoses "consisting of masses of tortuous vessels gorged with dark stagnant blood variegated by pale buff-coloured streaks apparently of separated fibrine," were found present in various parts of the body; while, at the same time, "pale, soft, loosely-adhering granules studded the borders of the aortic and mitral valves," he came to the conclusion that the blood is rendered impure in chorea partly by the introduction of inflammatory products from the diseased valves, partly by direct introduction of very small fibrinous concretions into the circulation, and "temporary obstruction in the minute capillaries occasioned by fibrinous particles arrested therein." In short, chorea is, according to his views, a result of irritation produced in the nerve-centres by fine molecular particles of fibrine, which are set free from an inflamed endocardium, and washed by the blood-current into the capillaries of those centres. Dr. Monckton published last year[2] an account of a young man who died of general and violent chorea, and in whom the absence of any cardiac murmur during the whole course of the attack is commented on. Shortly before his death *the pulse in the right radial and brachial artery ceased to beat*, while that in the left was "full and thumping." After death a cluster of warty growths was found fixed to one of the semilunar valves of the aorta, and a patch of the size of a filbert, *of a wine-red colour* and soft, was detected in the white substance of the middle lobe of the left cerebral hemisphere. Most unfortunately the brachial artery, which had in all probability been plugged by an embolus, could not be examined. Though Monckton was thus unable to bring to light any embolus, he very reasonably thinks that the patch of red softening was a consequence of embolism. The frequent occurrence of softening of the brain or spinal cord, or both, had struck many previous observers, and had not passed unnoticed by Kirkes. But he seems to be in error when he states that "the softening appears to be always the pale kind of softening." In the case first related in this paper, in Monckton's case, in two cases related by Babington,[3] and in one of Hughes's[4] cases, the softening is described as red or vascular. Dr. Kirkes is, further, not inclined to attribute the softening of the nervous centres to embolism of their larger

[1] 'Med. Times and Gaz.,' 1863, vol. i, pp. 636, 662.
[2] 'Brit. Med. Journ.,' 1866, No. 305.
[3] 'Guy's Hosp. Rep.,' 1841, p. 246.
[4] Ibid., 1846, p. 371.

vessels, but rather to "the imperfect nutrition of the nervous centres, or the unhealthy state of blood which attends the development of the chorea." The case which I have brought forward is nevertheless in a remarkable manner generally confirmatory of his views and observations. It adds to the force of his anticipation—"that future experience will still more positively demonstrate that an affection of the left valves of the heart, with the presence of granular vegetations upon them, is an almost invariable attendant upon chorea, under whatever circumstances the chorea may be developed." It shows how emboli may be scattered broadcast over the body when the valves of the heart are covered with these vegetations, and how one such embolus lodging in an artery of the brain may cause softening of that part of the brain which is supplied by the occluded artery. But here we must stop. Whether the group of symptoms included in the term chorea are to be regarded as a consequence of this cerebral softening, or whether a finer capillary embolism of the nervous centres must be assumed, as Kirkes supposes, to explain the difficulty, cannot yet be determined. We have not as yet sufficient evidence to justify the conclusion that embolism is a direct cause of severe chorea, but we have enough to warrant the strong suspicion that such may be the case, and to fix the attention of medical men on the heart and bloodvessels in the future examination of all fatal cases.

The question which next naturally suggests itself, is whether all cases of chorea, the milder ones which recover and whose internal organs we cannot see, as well as the severe ones which die and whose internal organs we can see, are to be similarly explained. Such a generalisation as this would be at present too hasty. It is quite possible that there may be a centric and eccentric chorea, as there is a centric and an eccentric epilepsy, but it behoves us nevertheless to seize every opportunity of examining the bodies of those who, while affected with mild chorea, die from some accidental cause (as continued fever) unconnected with the chorea. I can find only one such case recorded anywhere, and that in Dr. Kirkes's paper,—a mild case of chorea terminating fatally from enteric fever, *in which the mitral valve was freely studded with granular vegetations.* In consulting the writings of all authors on the subject, it will be found stated that fright or violent mental emotion is a most frequent cause of chorea. Now in Trousseau's case the onset of the disease was clearly and positively traced to a quarrel. Had this woman recovered, or had the mitral valve after death been carelessly examined, this case would have gone to swell the list of cases of chorea from mental emotion. In like manner,

had no post-mortem examination of the case recorded by myself been made, the chorea might fairly have been attributed to masturbation. Pregnancy is likewise spoken of as a cause of chorea; the pregnant uterus being regarded as the centre from which, as from an irritant, a stimulus is conveyed to the cord, and the chorea being supposed to follow as the excessive reflex action of the irritated nerve-centre. But in all the fatal cases of chorea in pregnancy that I find recorded, whenever the condition of the heart has been examined, the usual vegetations have been found there as in the Viennese woman of whom I have spoken.

Although a cardiac murmur is to be heard in a large number of chorea cases, mild as well as severe, it is absent in many. The absence of a murmur is generally taken to indicate absence of any valvular disease, and may, likely enough, turn the observer in a wrong direction searching for some other cause, as fright, worms, pregnancy, &c. But post-mortem observation proves incontestably that there may be well-marked valvular disease even when no cardiac murmur has been detected on the most careful auscultation. Witness the case I have recorded, where, for five days before death, no murmur could, though eagerly sought for, be heard! Witness a case recorded by Dr. Hillier,[1] where warty vegetations were found on the auricular surface of the mitral and tricuspid valves, and where a systolic murmur at the apex was audible only for one day during the whole time that the disease lasted—a period of nearly two months! Witness the cases of Monckton and Trousseau, in which no murmur was present from first to last! The following explanation of this peculiarity suggests itself on a review of these cases. The mitral valve may be perfectly competent even though its auricular surface is inflamed and covered with concretions; and hence there may be no systolic murmur. At the same time, the vegetations on the valves may be so fine as to be incapable of producing an onward or diastolic murmur. It is probably only when a vegetation gets detached and hangs for a time before it is finally separated, that it would be likely to interfere with the closure of the valve-flaps, or vibrate in the current of blood, so as to cause a temporary murmur which, unless the heart were examined hour by hour, might well escape observation. This interpretation accords very well, too, with peculiarities which I have often noticed in choreic murmurs,—viz. first, that they are very apt to change from time to time their tone and characters; secondly, that they not seldom disappear and then reappear at short intervals; so much so, that I have often, while making an auscultation in chorea, felt sure that I have heard a

[1] 'Med. Times and Gaz.,' 1863, vol. ii, p. 144.

murmur come and go with different beats of the heart. The case of W. E— is singularly interesting in this respect, the more so that the facts were witnessed by other ears than my own. A murmur, with a distinct musical note on the first day, completely changed its tone and characters on the second, and disappeared entirely on the fourth. In a case of Dr. Gray's which I watched with him last year, and which he kindly allows me here to refer to, this singular changeableness of choreic murmurs was strangely illustrated. A little girl came into the infirmary affected with chorea, in whom a short shrill diastolic murmur was plainly heard at the base. While taking arsenic for her chorea she was attacked with well-marked rheumatic fever. The diastolic murmur now disappeared and was succeeded by a systolic at the base. Shortly afterwards, as the rheumatism made progress, there was heard a systolic murmur at the apex, and a diastolic again at the base. She recovered from this rheumatic attack, but again, after a short interval, had a relapse; the chorea in this, as in the preceding attack, becoming more severe as the rheumatic fever declared itself. She finally recovered, and at the last auscultation made before she left the infirmary, no diastolic murmur could be heard at the base, but there was a decided systolic murmur at the apex. I have at the present time (May 18th), a boy, aged 16, under my care who is recovering from a severe attack of chorea following rheumatic fever. When I first saw him, and the chorea was at its height, there was a loud musical systolic murmur at the apex. Since he has begun to recover, the murmur has quite changed its tone and now remains as a soft blowing murmur without any musical sound.

Again, the not infrequent complication of hemiplegia with chorea, to which Dr. Todd has especially called attention in his clinical lectures[1] under the title of "choreic hemiplegia," has never yet been satisfactorily explained. It is quite possible that this, at least in its severer forms, will yet be found due to a real cerebral lesion caused by embolism,—a hemiplegia from embolism with chorea superadded. Dr. Todd himself laments that there are no post-mortem records to give a clue to the real nature of this peculiar form of paralysis. But yet, in another part of his great chapter on hemiplegia, where he is speaking of the convulsive movements which not infrequently affect the paralysed side, he narrates two cases of hemiplegia, one certainly, and the other most probably, a consequence of cerebral softening, in which convulsive movements of the limbs on the affected side of the body—the side of course opposite the cerebral lesion —so exactly simulated choreic movements, that they might well

[1] Ed. 2, by Beale, p. 802.

be called choreic. He is here, certainly, struck by the intimate relation which these cases have with ordinary choreic hemiplegia; for he writes,[1] "In cases like these, as doubtless also in chorea, the perverted nutrition of the brain occasions irregular developments of the nervous force, which are tantamount to a state of irritation, and give rise to convulsive twitchings. In chorea the alteration is not so profound as to be beyond repair; in the softening of adults, unhappily, the lesion is too often beyond recovery." We have thus evidence to show that choreic movements may accompany, and in all probability be caused by, cerebral softening,—evidence as it seems to me of no slight importance in estimating the possible influence of cerebral softening on chorea generally.

The true pathology, then, of the less severe forms of chorea must, from the very imperfect post-mortem evidence as yet afforded, remain an open question: but there are strong clinical reasons for supposing that, at least in many such cases, a condition of the cardiac valves similar to that now recognised as the special lesion of the maniacal chorea is present. How far embolism may play a part in chorea generally cannot at present be determined, but demands serious consideration.

One more feature of importance which stands prominently forward in studying the symptoms of maniacal chorea, is the startling rapidity with which emaciation sets in. It has been remarked on by previous writers, among others by Romberg.[2] In the case which I have recorded it was perhaps the most startling of all the symptoms. The boy, whose limbs on his admission were fairly developed for his age, became in a week emaciated to a degree; his eyes and cheeks hollow, like those of an old man. Nothing that I have ever seen in any other disease, even in the worst form of fever, could compare with it.

Before concluding, it may be well to give a short analysis of all the fatal cases of chorea, with autopsy annexed, that I have been able to collect.[3] They amount to 31 in number, making in all, with the one that I have narrated and the two others that I have mentioned, 34. In 25 of these the endocardium was found diseased; the presence of warty vegetations on the valves being specially alluded to in 20. Of the remaining 9, no mention is made of the heart in 5, and it is reported as healthy in 4. The pericardium was found diseased in only 8 of the 34. The brain was found altered in 18; its membranes

[1] Op. cit., p. 754.

[2] 'Trans. of the Syden. Soc.,' vol. ii, p. 53 et seq.

[3] The well-known tables of Dr. Hughes are not included in this list, but two fatal cases narrated by him separately and at length in his excellent article on chorea, in the 'Guy's Hosp. Rep.,' 1846, are included.

congested in 6, and opaque in 2; its substance more or less softened in 10. Of the remaining 16, no mention is made of the brain in 7, and it is reported as healthy in 9. The spinal cord was found altered in 16; its membranes congested in 5, and opaque or thickened in 2; its substance more or less softened in 9. Of the remaining 18, no mention is made of the cord in 12, and it is reported as healthy in 6. To judge from this analysis the morbid appearances after death from chorea stand thus in order of frequency: Endocarditis by far the first; softening of the brain second; softening of the spinal cord third; pericarditis fourth and last.

An analysis of the ages of the above cases is also instructive, as showing that those who have passed the age of childhood, *i. e.* who are above fourteen, are more often affected with the severe and maniacal form of chorea than mere children. Thus, the ages of 32 out of the 34 being given, it is found that 21 of the 32 were at or above the age of fourteen, and that 6 of these 21 were at or above the age of twenty.

It is worthy of remark that, in the majority of cases where the vegetations are specified as present, it is on *the auricular surface of the mitral valve* that they are found,—a situation where they are pre-eminently exposed to the action of the blood-current, and liable to be swept onward at each contraction of the auricle. This very situation might further prevent their being recognised in the examination of the heart. I was particularly struck with this in the case of W. E——. When the ventricle was opened in the ordinary manner, and the mitral valve looked at as usual from its ventricular aspect, no morbid appearance presented itself. It was only when the left auricle was slit up, and the valve carefully examined from its auricular aspect, that the disease was exposed. It is quite possible that, when the attention of the observer has been particularly fixed on the brain and spinal cord, the heart disease may have been thus overlooked.

The conclusions, which in the present state of our knowledge may fairly be drawn, are,—

1st. That in death from chorea, the presence of warty vegetations on the mitral or aortic valves is the rule.

2nd. That these vegetations may be set free, and carried as emboli to different organs of the body.

3rd. That the brain is often found softened; and that this softening may, certainly in some instances, be attributed to plugging of the cerebral arteries.

4th. That the spinal cord is not uncommonly found softened; and that this softening may, in all probability, be, in like manner, attributed to plugging of the spinal arteries.

5th. That a cardiac murmur may not be heard, even though the valves be seriously diseased; and that, consequently, certain cases of chorea attributed to pregnancy, fright, worms, &c., can be really due to the presence of vegetations on the cardiac valves.

6th. That recent investigations into the morbid anatomy of chorea warrant the suspicion that this disease may, at least in its severer forms, depend directly on irritation or softening of the great nerve-centres induced by the presence of emboli in the bloodvessels of those centres.

A list of references to published cases in addition to those already quoted is herewith appended.

Goodfellow, 'Med. Times and Gaz.,' 1862, vol. i, p. 629; Thompson, ibid., 1863, vol. ii, p. 94; Russell, ibid., 1865, p. 88; West, 'Diseases of Infancy and Childhood,' ed. 5, p. 220; Pitman, 'Lancet,' 1861, vol. i, p. 215; Fuller, ibid., 1862, p. 515; Nairne, 'Med.-Chir. Trans.,' vol. xxxiv, p. 37; Heslop, 'Dublin Journal of Med. Science,' 1858, p. 307; Keiller, 'Ed. Med. Journ.,' 1858, p. 81; Ogle, 'Trans. of Path. Soc.,' vol. xvii, p. 421; Peacock, 'Brit. and For. Med.-Chir. Rev.,' vol. xxxii, p. 492; Roger, 'Arch. Gén. de Méd.,' 1867, p. 67; Day, 'Clinical Histories,' p. 109.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

CHRONICLE OF MICROLOGY.

By J. F. Streatfeild, F.R.C.S.,

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Part I.—Physiological Micrology.

Brain.—In a horizontal section of the hypophysis of the brain of the calf, Peremeschko distinguished in the direction from before backwards, five layers of which the two last are remarkable in the calf: 1. anterior glandular layer of a grey-red colour—the cork layer of the author: 2. a medullary layer peculiar for its white colour: 3. posterior part of the glands of a grey-white colour: 4. a small layer of spongy substance, which connects the posterior surface of the glands with the capsule: and, 5. a thickening of the gland capsule, in the form of a bright white layer. Between the cork and the medullary layers is found a space, which, in the calf and sheep, appears as a simple cross fissure, in other animals and in man, ramified, and here and there the cork and medullary layers is interrupted by bridge-like communications. In man it appears to be an extension of the cavity of the infundibulum; its walls, in the animals, are lined with flat cells, in man with ciliary epithelium; its contents, in a preparation in spirit, consisted of a finely granular mass and of colloid-like droplets, besides, in man, granules of 0·0125‴ diameter, which, with the finely granular mass or the yellowish transparent droplets, are filled, and, in the first instance, exhibit nuclei. The cork layer is divided into lobes and lobules by connective tissue; the latter divisions, gland-sacs, of globular or oval shape, measure, in man 0·031‴. The contained cells, of 0·0062‴ measurement, are distinguished by their resistance to acetic acid and by a globular colloid-like body in the centre. The cork layer shows a wide tract in the pedicle of the hypophysis, exceptionally in man, ordinarily in animals, even up to the tuber cinereum. The medullary layer is also divided by septa of connective tissue into lobes, which are arranged in a radiated direction in the spaces; their elements are free nuclei of 0·0037‴ diameter, and cells which are distinguished by want of protoplasma, and the distinct appearance of the nuclei before the cells of the cork layer. The medullary layer reaches even to the upper part of the funnel, but its meshes are here, for the most part, only filled with a finely granular mass, which only here and there contains

morphological elements.—*Zeitschrift für rationelle Medicin*, 3rd series, vol. xxx, Part I, p. 147.

Muscle in Cross Section.—Kölliker is inclined to agree with the writer (Henle) that the *Cohnheim's fields*, as Kölliker calls them, correspond to the cross sections of the fibres which run through the muscle. But, according to his view, they are not, however, cuttings through the individual fibrils, but little fibril groups he calls "muscle columns." He does not say how many fibrils cut across are accorded to one muscle column, and, to guess at this, one is led to the comparison of the diameters of the fibrils and of the columns. In the frog, Kölliker estimates the former at 0·0012, the latter at 0·002 to 0·005 mm., but in mammals, according to his account, the diameter of the fibrils amounts to 0·0010 to 0·0015, and of the columns 0·0013 to 0·0025 mm. According to that, the diameter of the columns and fibrils turned out to be nearly the same, and one may question whether it gave columns that contained more than two fibrils. One learns, also, that the Cohnheim's fields are only faintly shown in the muscles of the rabbit and of the tail of the crab in the fresh state; that, in other muscles again, treatment with distilled water, or salt solutions, or other reagents, was necessary for their inspection, so we must not ignore various circumstances whereby always here and there the limits of individual fibres may be obliterated. And so also it may happen that, in a longitudinal section, the fibrils here and there seem to anastomise at an acute angle.—*Zeitschrift für rationelle Medicin*, 3rd series, vol. xxx, Part I, p. 44.

Connection of the Lymphatic System with the Serous Cavities.—This subject, in the Physiological Institution at Leipzig, has again received special attention; and in the Reports of the Royal Society of Science in Leipzig, 1866, Parts 2–5, are found three treatises on this subject. Of these the two latter remain to be noticed—F. Schweigger-Seidel and J. Dogiel, jointly treat *On the peritoneal cavity in the frog, and its connection with the lymphatic system.* The authors are convinced that in the peritoneum of the frog typical apertures exist, whereby the peritoneal cavity is in immediate communication with the lymphatics. But these openings are also demonstrable in lizards and slowworms, and indeed are of larger diameter. The investigations, in the frog, include that part of the peritoneum which, as an evidently single membrane, is separable from the posterior abdominal parietes: in the lower half, about the kidneys, where it passes across between them; in the upper, where it passes over the mesentery, and in this direction, limited by the great abdominal lymphatic reservoir. This peritoneal membrane consists of a connective tissue ground layer and a double epithelial membrane, of which one belongs to the abdominal cavity and the other to the lymphatic sac.

The ground layer is made up of thin bundles of connective tissue lying close together, between which spaces are left, which appear like fissures in the lamellæ, but by tension they resemble oval or round holes. On an average these openings measure 0·03 mm. in diameter. The interval of these holes is no way equal, but the num-

ber of the holes in the ground layer is always very considerable. The peritoneal epithelial cell layer of that membrane is shown, by treatment with nitrate of silver, to consist of cells, which are large but irregularly placed; sometimes remarkable for their extended length, or almost spindle-shaped, so that they may be of 0·01 to 0·02 mm. in breadth, and of 0·04 to 0·08 mm. in length. Such large cells are most commonly seen arranged around a common centre in a radiating manner, and, by a close scrutiny, one perceives that each such centre corresponds to an opening in the basis membrane. Such openings, in the silver preparations, were sometimes surrounded by a brownish-coloured festoon of a globular form, which one might well consider to be nuclei, the true signification of which also, as nuclei, may be well determined by preparations which were so put up that the peritoneal membrane stretched on cork could be instantaneously plunged in hot water, and then dyed in a carmine solution. The cells, arranged in a radiating manner, were nucleated, and these nuclei surrounded as a festoon the individual openings in the peritoneal membrane. The nuclei were 0·015 mm. in length and 0·009 mm. in breadth. The holes in the ground membrane were sharply defined, and their borders were so overtopped by the nucleated points of the epithelial cells that they pressed like little tufts here and there into the lumen (in the relaxed condition of the membrane), appearing to produce a closure of the opening. The epithelial layer on the side of the peritoneal membrane, which corresponded to the lymph sac generally, consists of irregular polygonal cells. How the cells in this layer are related to the openings in question, and how, within these openings, they are connected with the peritoneal epithelial layer—as to that a more certain indication could not be obtained. Yet the openings, on the side of the lymph sac, seemed almost always to be smaller than on the surface of the peritoneal cavity, wherefore also a funnel-shape of the holes was spoken of.

The so-conveyed information of an open communication between the great lymphatic reservoir of the frog with the peritoneal cavity may, however, be easily proved, by way of experiment. If, with the necessary precautions, one places in the peritoneal cavity of a living passive frog some granular colour, such as carmine, one should find it again at once in the course of the blood circulation. The corpuscular ingredients of a certain fineness reached, in all circumstances, from the peritoneal cavity easily into the great reservoir, and would gradually pass from hence into the so-called saccus iliacus, and would be driven further into the lymphatic heart.

The authors confirm the existence of the ciliary cells, already pointed out by former observers, in the peritoneal cavity of the frog on the forementioned partition wall between the peritoneal cavity and the lymph reservoir; and this observation seems to be altogether more significant, because they found the ciliated cells around the openings. The ciliary cells themselves are considerably smaller than epithelial cells of the proper serous membrane; in section they are only 0·01 mm. in breadth. In the silvered membranes one observes between the

large irregular cells larger or smaller fields of an elegant mosaic, in some cells of which ciliary filaments are still recognisable, &c.

The second treatise, by C. Ludwig and F. Schweigger-Seidel, has for its subject the centrum tendineum of the diaphragm. The well-known investigations of Recklinghausen on the diaphragm gave occasion immediately to follow up this part of the question, concerning the connection between the lymphatics and the serous cavities of the body. The description is preliminarily limited to the diaphragm of the rabbit.

At the centrum tendineum one may indeed with the naked eye distinguish a radiated and a circular connective tissue layer. The radiated layer lies on the peritoneal side, and consists of tolerably equally broad bundles, between which cleft-like spaces of 0·06 or 0·12 mm. in breadth, present themselves. The circular layer which is found on the side of the pleura, is more uniformly shown. Moreover, there are two more much finer layers, which have a course from the pleura and from the peritoneum, and in which the fibres have a more transverse direction. These four layers are not intimately blended, they rather separate themselves, especially from the pleural side, from each other in succession, in the fresh state as well as after preliminary hardening.

The trunks of the blood-vessels, for the most part, lie near the pleura. The finer twigs penetrate between the several layers of the centrum tendineum, and are also especially disposed in the spaces between the bundles of the radial fibre layer, partly parallel to those bundles, partly in an oblique direction from one side to the other.

By injection of the lymphatics in the centrum tendineum it is soon shown, according to Recklinghausen's experience, that fine corpuscular elements may pass over from the abdominal side of the diaphragm, without ceremony, into the lymphatics. A rabbit was cut across in the abdominal region, the œsophagus, with the great vessels, was secured with a ligature including the vertebral column; before one proceeded to the removal of gut and stomach, the anterior half of the animal was then hung up with the head downwards, and the abdominal parietes somewhat stretched upwards, and the proper injection mass simply put in the concavity of the diaphragm, whereby one either lets the liver remain as it was, or it is drawn somewhat upwards by means of a thread fastened round the gall-bladder. After some time, one finds that the lymphatic network on the convex side of the diaphragm is injected. The result is even better when one makes the diaphragm perform rhythmical movements, whilst one inflates the lungs intermittingly by a tube secured in the trachea.

After so complete an injection, one finds the proper lymphatic network on the pleural side; the vessels by this procedure expend themselves partly forwards in the region of the vasa mammaria, partly descend backwards in the direction of the thoracic duct. On the peritoneal side of the injected diaphragm that network and the larger vessels are completely wanting, and here one only observes

ray-like streaks passing off, filled with injection mass, which correspond to the spaces between the bundles of the radiated fibre layer. These streaks are always first filled with the mass. On cross section of the injected centrum tendineum, one finds the spaces between more or less considerably filled with the mass, but the lymphatic network lies between the circular fibre layer and the pleura. The injection mass is always kept in an even course, it follows no irregular proceeding between the individual tissue layers, and there also never, by elastic filling, by chance comes out any fine extravasations which might be considered to indicate connective tissue spaces or fluid canaliculi. Hence we must conclude that these fissures, like the proper lymphatics, must have a special investment. The silver method also confirms all this, to make apparent the disposition of the lymphatics in the centrum tendineum.

By the manifestation of the passage of the corpuscular elements from the peritoneal cavity into the lymphatics, which also in the living animal certainly takes place without any injury to the peritoneal covering of the diaphragm, the approximate investigation of the following anatomical relations is brought to our minds. The connective tissue ground layers, or the peritoneal foundation, shows some places denser and some others of looser texture. The denser parts, with a regular arrangement of the fibres, cover the radiated bundles of the centrum tendineum, the looser parts in which fine and very fine connective tissue bundles are arranged as a network bridge over the spaces between the radiated bundles. To this regular arrangement the foundation membrane of the peritoneum follows the same course in the peculiarities of the epithelial layer. Thus, if one treats the centrum tendineum on the abdominal side with silver solution, there at once comes into view, with low magnifying powers, a perceptible streaking, which is arranged so that the epithelial cells are small over the spaces, and are generally separated from each other by irregularly enlarged black outlines, whilst over the radiated bundles there lie certainly larger epithelial cells. The latter are on an average 0·032 mm. in diameter, the former only 0·012 mm. The certain appearance of intercellular apertures which, as a matter of course, appear in differently prepared specimens, does not allow one to decide with absolute certainty, in consideration of the suspicions of an accidental origination; for a similar arrangement of the cells around the visible openings, as they are found in the peritoneum of the frog, does not appear in the rabbit's diaphragm. It is of greater consequence, however, in this relation, that the connective tissue ground layer or the peritoneal foundation membrane, on which the epithelial layer rests, is formed of meshes. For no special value can be attributed to the mere openings in the epithelium, when they do not also pass through the foundation membrane. But if the depicted arrangement of the centrum tendineum on the absorption and forwarding of fluid substances from the abdominal cavity be accounted for, then the movements of the diaphragm in respiration will render uninterrupted absorption possible, with regard to the epithelial cells on the peritoneal side. The uniform layers of small

cells over the radiated spaces were not unfrequently interrupted by groups of smaller cells, which looked partly like lymph corpuscles, and it may be affirmed of them, with tolerable certainty, that they proceed from the cells of the serous membrane itself by separation. In silver preparations all stages of the separation could be followed within a limited space. With the separation of the nuclei proceeds a multiplication of the cells, which were placed side by side in little globular heaps. This process never extended over the spaces. If the proceeding were physiological, there would be with it a place shown where the origin of the lymph cells could certainly be seen. The really existing cellular elements in the lymphatics of the diaphragm are hither conducted from the epithelium of the peritoneum. *Schmidt's Jahrbücher*, No. 2, 1867, pp. 155–7.

Cornea.—The cornea of the frog, according to Engelmann, consists of about fifteen to twenty lamellæ, each of which, as in the higher animals, is made up of parallel fibrillæ, whilst the fibrillæ cross the other neighbouring lamellæ for the most part rectangularly. Here and there fibre layers pass from one lamella over into another. In the contact surface the lamellæ lie uniformly distributed, and at a moderate distance the corneal cells, which the author describes as polygonal non-granular protoplasma masses, with always one nucleus and processes, which, six to twenty in number, lie most of them in the level between two lamellæ; they, however, also pass through the lamellæ—are said some to be in intercommunication and some as free ends. There also appear, in variable numbers, little cells, differing in form and position, which move in the spaces, which are filled with fluid, between the corneal fibrillæ, and so separate the latter.—*Zeitschrift für Rationelle Medicin*, 3rd series, vol. xxx, Part I, p. 123.

Part II.—Pathological Micrology.

Tubercle.—Dr. Richard Dawson, of Brighton, has communicated a paper on this subject to the Royal Medical and Chirurgical Society. The author asserted that tubercle affected the microscopic elements of cell-tissues, partially as regards the cells composing the organ, generally as relating to the human body diseased. He showed sections from miliary tubercle of the lung, and that, in them, the cells lining the air sacs were, in places, no longer transparent, oval, small, and few—nucleated, but had become dark, enlarged, warty, and irregular in form, with many nuclei. This most remarkable change he further illustrated by specimens from the same bodies, as well as by sun-pictures of the same. Some of these presented the meningeal arteries and vessels pulled from the cerebral hemispheres. It was seen most clearly in these that, at intervals along the course of the capillary vessels, bulbous enlargements occurred, and these swellings were in the outer coat of the artery, for the calibre of the vessel was undiminished. The nature of this bulb-formation he showed to be disease, arising in the cells composing the outer coat; and this disease he called "neuclear hypertrophy," from its nature; for it was

seen that the long undulating fibre-cells composing the outer coat lost their length, while their nuclei became enlarged and greatly hypertrophied. Many cells taking on this change, the bulb was formed, gradually enlarging, and capable of eventually obliterating the vessel, though not doing so of necessity. The change occurring in the diseased lung cells was of a nature very similar: their nuclei hypertrophied, and many cells taking on this change at once, a mass was formed of enlarged cells pressing on each other, and eventually shutting up the air sac altogether, giving rise to a tubercle. There was, therefore, no foreign deposit, but an abnormal action set up in the nuclei of normal cells.—*Proceedings of the Royal Medical and Chirurgical Society* ,April 9th, 1867.

Addison's Disease.—Of its nature, microscopically, Dr. Greenhow says that "the translucent portion is generally found to consist of a more or less fibrous basis, mixed with much granular matter, containing shrunken cells and nuclei, some of the nuclei being occasionally elongated into spindle-shaped bodies. The yellow opaque nodules consist, in different proportions, according to circumstances, of amorphous granular matter, mixed with irregular-shaped shrunken cells, nuclei, and oil. When the creamy liquid from cavities, or so-called abscesses, has been examined, it has been found not to be true pus, but to consist mainly of oily debris." In one case, the author writes, of the "seat of discoloration," "I found, in addition to the usual deposit in the rete mucosum, slight traces of colour in some of the superficial scales of the epidermis, others being perfectly normal; there were also small masses of pigment deposited here and there in the cutis. Whether in the rete mucosum, or in other parts of the skin, the pigment is arranged in granular masses; and the margin between the pigmented layers of skin, and the super and subjacent layers is always abrupt and well defined, and follows the undulations of the rete mucosum, even when, as in the case just referred to, there are scattered deposits in other parts of the skin." —*On Addison's Disease*, 1866, pp. 58 and 61.

Liver, Syphilitic.—Among conclusions he has arrived at, Dr. E. Leudet says, that the lesions said to be syphilitic are sometimes accompanied by a supplementary hypertrophy of the normal elements of the gland, or by a starchy or waxy degeneration of this organ.—*Archives Générales de Médecine*, March, 1866, p. 332.

Fibroid Thickening of Pyloric half of the Stomach.—This case was brought before the Pathological Society of Philadelphia. The patient, a man of sixty-five years of age, had had nausea, pain in the left side, and emesis. A tumour in the belly was felt. In two years he died. On p.m. section the peritoneum was found dotted with ulcers, large and small, some healed. The pyloric extremity of the stomach was hard and rounded, becoming thinner and thinner towards the cardiac end. The lungs were dotted with little hard white bodies. The heart was small and edematous-looking. Dr. S. W. Mitchell says—"At the line of union of right auricle and ventricle there was a bulging sac full of gelatinous fluid surrounded by smaller ones much

like it in appearance. The largest was an inch long. Also, from the right auricle down to the apex along the right cardiac border, we found a line of similar bodies. These seemed to me to be pericardial doublings which had once held fat."

Dr. W. W. Keen adds—"The cardiac extremity of the walls of the stomach was somewhat thicker than usual. A section showed the glands considerably enlarged."

"The pyloric extremity was about three or four times the normal thickness. A section here gave a large amount of fibrous stroma, arranged in oval and circular meshes. In these meshes were masses of nuclei and cells. The latter were nearly all fragmentary. Their walls were generally indistinct, their forms mostly rounded, or if angular, it was apparently by reciprocal pressure. Some few were nucleolated. They were more numerous than the cells."

"*Heart.*—The apparently gelatinoid cysts showed (1) some connective tissue, with here and there an elastic fibre; (2) a large number of fat globules, and (3) abundant granular matter and fragments of cells. The connective tissue had no fragmentary arrangement, nor did it contain within it the fatty and granular matters."

"*Peritoneal Ulcers.*—They showed simply connective tissue with pigment in large quantities, and granular debris."

"*Lungs.*—The little, round, hard, shot-like bodies were made up of an outside circular ring of connective tissue, with a large deposit of pigment in it. The contents of this ring of connective tissue were hard, gritty, friable, did not effervesce on the addition of hydrochloric, nitric, or acetic acid, nor was it soluble in these acids. Its character was not determined, but it was probably degenerated tubercle.—*American Journal of the Medical Sciences*, April, 1867, pp. 436-8.

Abnormal Hair in Syphilis.—Mr. Erasmus Wilson says, in a communication to the Royal Medical and Chirurgical Society, that only one disease of the hair, viz. tinea or phytosis, has been hitherto noted as due to an alteration of structure. He proposes to add another. In a case of constitutional syphilis, accompanied with trichorrhœa of the hair of the head and alopecia, the hairs of the beard presented some remarkable phenomena; the shaft was blackened and nodulated from point to point, and at these spots broke through with the most moderate force. Under the microscope the diseased parts were found to be some constricted and some enlarged into a kind of fusiform bulb; they consisted of a dark cylinder, enclosed in a transparent cortical envelope; the dark cylinder being composed of nucleated cells, pigmentary matter, air cavities, and crystalline fragments, and being continuous above and below with the medulla, of which it seemed to be an enlarged expansion, while the transparent envelope was a thin layer which represented the fibrous portion of the cuticle of the hair. The dark cylinder was, in fact, an arrest of development of the hair at its cellular stage, &c.—*Proceedings of the Royal Medical and Chirurgical Society*, June 11, 1867.

REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

On the so-called Urethral Fever.—Professor Roser observes that various explanations have been offered of cases of which almost all surgeons have met with examples, wherein febrile symptoms, exactly resembling those of ague, are observed after the passage of a bougie for stricture, even a fatal termination sometimes ensuing. Some regard them of a pyæmic character, and Sédillot recently endeavoured to explain them by the supposition that excoriations produced by catheterism gave rise to urinary resorption. To this Mercier objects that the accident never attends urethrotomy.

Dr. Roser notices the following points:—1. Urethral fever seems only to have been met with in old cases of stricture. In calculous affections without stricture, and notwithstanding the great irritation produced by fragments of stone and by lithotrity instruments, it is only exceptionally observed; and it has not been met with in recent injuries of the urethra, in prostatic disease, in operations on the penis or on the female organs, or in inflammatory affections of the urinary organs. Urethral fever would seem, therefore, to consist in a peculiar reaction which the male urethra, in a condition of chronic stricture, exerts on the occurrence of a relatively slight mechanical stimulus. In several instances the fever has arisen spontaneously, giving rise to the supposition that the mechanical stimulus has in such cases been produced by the difficult discharge of the urine.

2. It can be attributed to no toxical cause, the symptoms being entirely different from those which accompany uræmia and ammoniæmia. The various forms of pyæmia may be met with after operations for stricture, but, besides the febrile paroxysm, it is attended by its usual train of symptoms—phlebitis, peritonitis, articular inflammation, &c.

3. We may regard a special reflex action from the urethra to the nervous system as the cause of urethral fever. It originates without any previous alteration in the condition of the blood; but the question arises how the reflex action is brought about, and how are febrile symptoms the result of its operation.

To discuss this point, we should have to go into the questions of the production of fever, of shivering, hot fits, &c., which have occupied so much attention; and perhaps there is nothing more likely to throw light on these questions than the study of urethral fever. Dr. Roser is very desirous of calling the attention of those who have the direction of large hospitals to the point, as so small a town as Marburg supplies but an insignificant number of cases of stricture. Moreover, urethral fever is not met with in his practice, as he prevents its occurrence by the administration of morphia, either before or immediately after the passage of an instrument. This procedure is an additional argument in favour of the reflex nature of the affec-

tion, while it prevents a complication that sometimes proves dangerous, or even fatal.—*Archiv der Heilkunde*, 1867, No. 3.

Military Surgery.—Dr. Rodolfi, in charge of numerous cases derived from the Italian campaign of 1866, after remarking on various interesting points on military surgery, terminates with these conclusions:

1. Absolute and permanent immobility of a limb wounded in an articulation suspends pain, and almost always obviates the necessity of amputation.

2. Bonnet's apparatus, constructed of iron or brass wire, not being rigid, but elastic, is not suited for wounded joints. It favours the production of frequent contractions by communicating the oscillations of the bed.

3. Amputation should only be resorted to when there is no probability of saving the limb by conservative surgery.

4. Flap operations give rise to the readiest cures. Teale's method should be preferred in amputation of the thigh, as preventing the protrusion of the stump while securing the free discharge of pus and the formation of a good cicatrix. Good results may follow Gritti's patella-flap operation.

5. None of the subjects of severe wounds who were treated prophylactically by means of 12 grammes of the sulphite of magnesia per diem contracted resorptive fever. The sulphites proved useless in patients suffering from bad wounds, who became affected with this fever immediately on entering the hospital.

6. The application of ice in combination with compression constitutes the only mode of arresting hæmorrhage by oozing in gunshot wounds of the face. The perchloride of iron sometimes induces fatal syncope, and the various caustic and astringent hæmostatics are of difficult application, especially when the wound is in communication with the mouth.

7. Drainage has proved of practical utility in corrupted infiltrations and large collections of pus.

8. Nélaton's probe may lead to grave errors concerning the seat of the projectile. Great advantage results from guarding its porcelain button by a canula.

9. Favre's electrical sound, defended by gutta-percha tubes, may be of great assistance in searching for balls.

10. The conical balls of modern warfare are best removed by the forceps employed by the American surgeons, or by Ferguson's forceps, as modified by the author.—*Gazette Med. Lombardia*, No. 15.

Fracture of the Cartilaginous Septum Narium.—M. Jarjavy has collected fourteen cases of this occurrence, observed at his own (the Beaujon) or other of the Paris hospitals. His general conclusions are as follows:

1. A violent blow on the nose may produce solution of continuity of the cartilage of the septum, and even rupture the fibrous attachments which fix the lateral cartilages to the lower edge of the nasal bones.

2. The fracture may be healed without any consecutive complication.

3. Among the symptoms are bleeding, which stops spontaneously, tumefaction and tenderness, abnormal mobility, with crepitation and a sense of want of resistance to the finger. There is flattening of the cartilage, which is sometimes persistent, and the voice is nasal.

4. The fracture may be of little consequence, only producing some incurvation of the dorsal line of the nose or a lateral deviation.

5. It may, however, be complicated by a wound of the dorsum of the nose, and sanguineous effusion or abscess of the septum.

6. The cutaneous solution of continuity may become fistulous, and the blood or pus of the tumour of the septum obtain exit by an accidental opening on the back of the nose.

7. The pus which issues is in a notably disproportionate quantity to the size of the wound.

8. It can be made to flow out by compression of the sides of the nose.

9. A probe introduced into the fistulous opening penetrates into the substance of the septum, and traverses the cavity of the abscess, which is limited within by the cartilage, and externally by the detached mucous membrane.

10. The fistulous opening at the back of the nose usually heals rapidly as soon as a counter-opening is made for the discharge of the pus at the most dependent part of the tumour of the septum.

11. A probe passed into this opening detects a detachment of the mucous membrane and an aperture in the septum through which the instrument can be passed.

12. Fracture of the septum may become complicated with bloody or purulent tumour of the septum, unaccompanied by any external wound or fistula at the back of the nose.

13. The sanguineous tumour, developed shortly after the accident, causes more or less impediment to respiration. If a puncture be promptly made, blood mixed with dark coagula flows out, and is soon replaced, first by sanguinolent, and then by transparent serosity.

14. When the tumour is not considerable enough to completely obstruct the passage of air into the nostrils, and the patient has not at once sought for assistance, after a few days headaches arise, and the integuments of the nose become red, swollen, and œdematous—the median portion of the forehead also soon becoming affected. Under the influence of this inflammatory action, the tumour of the septum enlarges, and completely obstructs the nares. The patient, if now first seen by the surgeon, presents the tumour of the septum as described by Cloquet, Fleming, and Bérard, who regarded it as a special affection, while it is really but a complication of the fracture.

15. The same phenomena are observed when there is a wound at the dorsum of the nose communicating with the tumour; for although the patient has been able, by blowing the nose, to expel blood, a portion of this always remains in the lower part of the detached

mucous membrane. To prevent the formation of abscess a counter-opening must always be made as soon as possible.—*Bull. de Thérapeutique*, June 30.

Strangulated Umbilical Hernia.—We some time since (July, 1866, p. 271) called attention to M. Demarquay's observations on the subject of the fatal results which follow operation for strangulated umbilical hernia. The modification of it which he then described, and which essentially consisted in making as small an aperture in the sac as possible, had not then been attended with the success he hoped for, although this was nearly being the case, as the patient lived for forty-eight hours, and then died of secondary strangulation. Convinced of the correctness of the principle of avoiding extensive exposure of the sac, he has again operated on a lady sixty-four years of age, and with success. Strangulation had existed for three days, and fæcal vomiting, with great alteration of countenance, had set in. Having practised an incision over the tumour between five and six centimètres in length, he exposed the sac by carefully dissecting the tissues layer by layer, which, as the patient was stout, required some time. Seizing the sac with a forceps, he opened it just sufficiently to admit the index finger of the left hand, by which he hooked (*accrochait*) hold of the constricting ring, and, gliding a probe-pointed falciform bistoury along the palmar surface of the finger, he relieved the strangulation by an incision of one and a half centimètre in length. The wound was united by five points of interrupted suture, and dressed with glycerine, small doses of opium being continued during two or three days. Recovery was complete. The prevention of peritonitis, which has always proved so fatal in these cases, M. Demarquay attributes to the smallness of his incision and slight consequent exposure. He made no attempt at examining the condition of the intestine, or at returning it into the abdomen. The tumour gradually became reduced in size, so that what was a voluminous hernia is now no larger than an egg. In M. Demarquay's former case he believed that he had not extended his incision of the constricting ring sufficiently, a fault he took care to avoid on this occasion.—*Union Méd.*, Nos. 78 and 82.

On some Errors in Military Surgery.—Professor Roser, continuing the criticisms under this title which we noticed in our July number, next comes to—

6. *Excision of Joints.*—Here he complains that the scientific principle upon which traumatic excision should be founded, the prevention or limiting of septicæmia, is insufficiently acknowledged by surgeons, who for the most part engage in not very fruitful discussions on mere methods of procedure. The enlightened views expressed by Park, in 1782, were lost sight of for more than half a century, until revived by Stromeyer on the occasion of the Schleswig-Holstein war. The danger of resorption of the fetid discharges which attend gunshot wounds of the joints is obvious, and if the patient escapes the peril of acute septicæmia he has to confront those of the more chronic form at a later period. The pus obtains

insufficient issue, and the progress of the case is delayed by the presence of necrosed fragments and fistulous apertures. A patient so situated in a military hospital is, moreover, exposed to the great danger of miasmatic pyæmia. Excision simplifies the condition of such a case by substituting clean-cut surfaces and ample means for the egress of fetid discharges. The leading point, then, to be borne in mind in determining on excision is the prevention of the accumulation of fetid discharges and the production of septicæmia consequent on their absorption.

7. *Amputations.*—To the question, Why do we amputate in shattered limbs? the answer has been different at various periods, until, at the present time, the chief reason is admitted to be for the purpose of removing the parts which, by the corrupted discharges they produce, are the sources of septicæmia. When such symptoms have set in, primary amputation having been neglected, the course to be pursued, whether in civil or military practice, is not always easily determined, and the surgical handbooks are so contradictory in their recommendations, founded as these are on no scientific basis, that the practitioner gets little help from them. This arises from the comparative recent acknowledgment of the influence excited by the absorption of the matters of decomposition in the production of surgical fever. In relation to this subject, Dr. Roser believes the following positions may be laid down:

(1) Early amputation is to be performed for the purpose of avoiding septicæmia; the obtaining a smaller and simpler surface, or a better covering for the wound, being only secondary objects. (2) When resorted to later, it should still be so for the purpose of removing the parts which, by supplying the decomposed discharges, have given rise to the resorptive fever. The rapid improvement in patients where such sources of resorption have been cut off has long been matter of observation, even when the cause was not perceived. (3) There are cases of septic infiltration of the cellular tissue, in which the infection of the connective tissue spreads from mesh to mesh, often in a manner analogous to the progress of emphysema; and if amputation be not resorted to without delay the patient dies. This condition is usually observed after crushing of limbs, with little or no loss of blood, the products of decomposition having no issue. (4) Many cases of acute traumatic gangrene, or acute traumatic putrefaction, are complicated with such extensive decomposition and with so foul a surface of the wound that a considerable resorption of the putrid product is to be feared. The more this is the case the more prompt should be the resort to amputation. If we wait until the fever or inflammation abates, the surface of the wound becomes cleaner, or the gangrene is limited, the patient will die. (5) When the foul surface of the wound is not very large, the sequestral inflammation having already set in, and when we therefore may hope the patient will have soon passed through the septic fever, the operation may be delayed until this first stage has passed. (6) When septic fever becomes combined with specific or zymotic pyæmia, no determinate plan of treatment, according to the present

state of our knowledge, can be laid down. Dr. Roser lays great stress on the importance of simplicity of dressing and after-treatment as means of preventing the fouling of the wound and consequent resorptive fever.—*Berlin Klinische Wochenschrift*, Nos. 20 and 21.

Summary.

Amputation.—Fremmert. On Chopart's Operation. (Archiv f. Klin. Chir., B. ix, H. i. Relates a case in which this was performed on both feet with excellent results. A good critical examination of the value of the operation.)—McGill. On the adoption of the Periosteum Flap in all Amputations in Continuity. (Trans. Amer. Med. Assoc., vol. xvii.)—Ried. Case of Successful Amputation at the Hip-joint. (Jena Zeitschrift, B. 3, H. 2.)—Melchiori. Successful Case of Amputation with Gritti's Patella-flap. (Annali Univ. di Med., May.)

Aneurism.—Morton. Case of Axillary Aneurism. (Amer. Journ. Med. Sci., July. Interesting case, in which the subclavian was tied between the scaleni; and on account of repeated hæmorrhages, ligature of the subscapular, amputation of the arm, and amputation at the shoulder, were successively resorted to. Illustrated by woodcuts.)—Duncan. On Treatment of Aneurism by Electrolysis. (Edinb. Med. Journ., August.)

Breast.—Rezzonico. On Diseases of the Breast. (Annali Univ. di Med., April and May.)

Burns.—Von Pitha. On the Treatment of Burns. (Allg. Wien. Med. Zeit., No. 25. Contains some interesting details of the case of the Archduchess Matilda.)

Contractions.—H. Meyer. On Forcible Stretching of Contracted Joints. (Arch. f. Klin. Chir., B. ix, H. i. The mechanism of the obstacles offered in contraction of the knee-joint, and of the mode of action of apparatus, described by the aid of diagrams.)

Dislocation.—Cameron and Bell. Cases of Dislocation of the Femur reduced by Manipulation. (Glasgow Med. Journal, July.)—Sayre. Dislocation into the Ischiatic Notch of nine months' standing reduced by Manipulation. (Trans. of Amer. Med. Ass., vol. xvii. Woodcuts.)—Kelly. On Reduction of Dislocation of the Jaw. (Dublin Journal, August.)—Lotzbeck. On Tardy Dislocation of the Clavicle. (Deutsche Klinik, Nos. 18 and 21. Two cases, one acromial and the other sternal, in which the dislocation did not occur until some months after the accident.)—Dauvé. Case of Incomplete Dislocation of the Sixth on to the Seventh Cervical Vertebra. (Gazette des Hôp., No. 91. Binocular mydriasis was observed during three days.)—Langenbeck. On a New Mode of Reducing Dislocation of the Humerus. (Allg. Med. Zeit., No. 23.)—Brinton. On Dislocation of the Body of the Sternum. (Amer. Journ. Med. Sci., July. Relates a case, and compares it with thirteen others already recorded. Some new investigations on the anatomical structure of the superior sternal articulation.)

Ear.—Grossmann. On the aid derived from the Rhinoscope in

Diseases of the Ear. (Allg. Wien. Med. Zeit., Nos. 20—22.)—Gruber. On Separation of the Attachments and Loss of Substance of the Membrana Tympani. (Ibid., No. 23.)—Bonnafont. On Otorrhœa in Children. (Union Méd., 81. Believes that the frequency and importance of otorrhœa are underrated, and recommends its treatment by the insufflation of astringents, figuring a new otoscope by which this is facilitated.)—Wendt. Report on his Polyclinic for Diseases of the Ear at Leipzig. (Archiv. f. Ohrenheil, B. 3, H 1—3.)—Lucæ. On a New Mode of Examining the Ear. (Ibid.)—Gruber. Anatomy of the Temporal Bone in relation to Diseases of the Ear. (Wien. Med. Wochenschrift, Nos. 53 and 54.)

Elephantiasis.—Carnochan. Case of Elephantiasis Græcorum treated by Ligature of the Common Carotid on both sides. (Amer. Journ. Med. Sciences, July. Two plates.)—Bottini. Case of Elephantiasis Scroti. (Annali Univ. di Med., May. Woodcuts.)

Excision.—König. On Excision of the Knee in Children. (Arch. f. Klin. Chir., B. ix, H. i. König gives some measurements in relation to the lines of the epiphyses, and the statistics of the operation hitherto published, adding two cases of his own. He believes that the mortality would be considerably less if the gypsum bandage were applied after the operation.)—Ried. Three Cases of Excision of the Elbow performed on both Arms. (Jena Zeitschrift, B. 3, H. 2. Ried states that since he has had the Jena Clinic he has performed about thirty partial or total excisions of the elbow, with only one fatal result.)

Eye.—Magawly. On Chondritis Syphilitica. (St. Petersb. Med. Zeit., H. 4. Notes on four cases of an affection of the tarsal cartilage, attended with its considerable tumefaction, but in which there is no ulceration or other participation of the external skin. The affection yields to mercury.)—Agnew. On a New Method of Operating for Divergent Squint. (New York Med. Journ., July.)—Arlt. Case of Removal of *Cysticercus cellulosæ* from the Bulb. (Allg. Wien. Med. Zeit., No. 27.)—Adamiuk. On the Etiology of Glaucoma. (Annales d'Oculistique, July. Referred to obstruction of the venous circulation.)—Sichel. On Enucleo-dissection of the Globe. (Ibid., and Gazette Méd., No. 27. Case related of melanoid cancer of the eye, in which dissection was combined with enucleation.)—Knapp. Report on 100 Cataract Operations, performed by Von Graefe's New Method. (Achiv f. Ophthalm., B. xiii, A. i.)—Green. On Astigmatism. (Amer. Journ. Med. Sciences, July.)—Wilson. On Examination of the Eye by the Ophthalmoscope. (Dublin Journ., August.)—Von Graefe. On Basedow's Disease. (Berlin Klin. Woch., No. 31.)—Galezowski. Lecture on Strabismus. (Gaz. des Hôp., No. 90.)—Bonomi. Treatment of Lachrymal Tumour. (Annali Univ. di Med., May. Relates several cases in proof of the utility of the caustic treatment.)—Foltz. On an Improvement in the Operation for Lachrymal Tumour. (Gazette Méd. de Lyon, No. 25.)

Face.—Verneuil. Management of Hæmorrhage in Operations upon the Face. (Gazette des Hôp., No. 93. By preliminary plugging the nares.)

Fracture.—Bigelow. On an Operation for Ununited Fracture. (Boston Med. Journ., May 16 and 23. Notes of eleven cases given of successful treatment, by connecting the excised ends of the bones by silver wires, great care being taken to preserve the periosteum.)

Hernia.—Uhde. On Hedrocele. (Archiv für Klin. Chirurgie, B. ix, H. i. Two cases of hernia of the rectum, occurring in children, given with post-mortem examinations and lithographs.)

Jaw.—Bottini. Case of Subperiosteal and Subcapsular Disarticulation of the Jaw. (Annali Univ. di Med., May. Woodcuts.)—Enos. On the Intra-oral Method of Operating on the Lower Jaw. (Trans. Amer. Med. Ass., vol. xvii.)

Keloid.—Pick. Observations on Keloid. (Wien. Med. Woch., Nos. 57 and 58.)

Laryngoscopy.—Oliver. Removal of a Fibro-plastic Tumour from the Right Vocal Cord. (Amer. Journ. of Med. Sciences, July. Woodcuts.)—Turck. On a New Caustic-holder. (Allg. Wien. Med. Zeit., No. 25.)—Gerhardt. Case of Polypus of the Larynx. (Jena Zeitschrift, B. 3, H. 2.)

Necrosis.—Bottini. Case of Excision of the Frontal Bone for Necrosis. (Annali Univ. di Med., May. Woodcuts.)—Bottini. Case of Reproduction of the entire Tibia in Necrosis. (Ibid., July.)

Ovariotomy.—Lyon. Case of Ovariotomy, followed by unusual conditions. (Glasgow Med. Journ., September. Fistular openings in a portion of intestine presenting through the retraction of the wound of the parietes.)—Sklifossoffsky. Two Successful Cases. (Archiv. f. Klin. Chir., B. ix, H. i.)—Danzel. Notes on Three Cases. (Ibid.)—Labbé. Fatal Case. (Gazette Hebdom., No. 28.)—Bottini. Two Unsuccessful Cases. (Annali Univ. di Med., July.)—Stillings. Twelfth Case. (Deutsche Klinik, Nos. 22, 23, and 25. Fatal in ten days from tetanus and pneumonia.)—Kœberlé. Successful Case. (Gazette des Hôp., No. 90.)—Weinlechner. Successful Case. (Wien. Allg. Med. Zeit., No. 26. The first successful one of the nine operations which have been performed in Austria.)

Parotid.—Otto Weber. Three Cases of Total Extirpation of the Parotid Gland. (Deutsche Klinik, Nos. 24—27.)

Phimosis.—Kersch. Observations on Phimosis. (Prag. Vierteljahr., B. 3. Contains an account and figures of a new instrument for circumcision.)

Plastic Surgery.—Simon. Plastic Operations on the Rectum and Vagina. (Prag. Vierteljahr., B. 3. With plates.)

Pyæmia.—Woodward. Report on the Causes and Pathology of Pyæmia. (Trans. Amer. Med. Ass., vol. xvii.)

Spine.—Lee. On the Treatment of Angular Curvature of the Spine. (Trans. Amer. Med. Ass., vol. xvii. With woodcuts.)

Trephine.—Gosselin. Clinical Lecture on the Indications of the Trephine. (Gazette des Hôp., No. 81.)

Urinary Organs.—Hughes. Report on Vesical Calculi. (Trans. Amer. Med. Ass., vol. xvii. A case in which lithotomy was performed

three times on the same patient, æt. 62, between 1855 and 1860. Woodcuts of the twenty-six calculi removed. Hughes is a warm admirer of the bilateral method, by which he has operated in twenty-one cases, only losing one.)—Post. A New Instrument for Bilateral Lithotomy. (Ibid.)—Simon. Employment of Lithotrity with Lateral Lithotomy in a Case with two very large Calculi. (Deutsche Klinik, No. 32.)—Berutti. On Lithotomy in the Male and Female. (Gazette Med. di Torino, Nos. 20—25. An historical and critical essay, containing nothing new.)—Greenhow. On Lithotomy and Lithotrity in the Punjab and North-west Provinces. (India Annals, July. The experience of twenty surgeons collected, furnishing 1851 cases of lithotomy, 259 proving fatal.)—Kraus. On Chronic Prostatitis. (Wien. Med. Woch., No. 46.)

Uterus.—Storer. On the Clamp-Shield in Extirpation of the Uterus. (Trans. Amer. Med. Ass., vol. xvii. Woodcuts.)

REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond.,

Obstetric Physician and Lecturer at St. Thomas's Hospital; Examiner in Midwifery to the Royal College of Surgeons.

I.—Pregnancy.

1. *Diseases of the Pregnant Woman.*—Dr. De Cristoforis. ('Annali Universali di Med., 1867.)
2. *Clandestine Labour, followed immediately by Infanticide; Expulsion Two Months later of an Hydatid Mole.* Dr. Fischer. ('Annales d' Hygiène,' April, 1867.)
3. *Diffuse Myxoma of the Ovum-membranes.* Dr. Breslau. ('Wiener Med. Presse,' 1867.)
4. *Case of Extra-uterine Gestation.* Dr. Pletzer. ('Monatssch. f. Geburtsk., April, 1867.)

1. *Diseases of the Pregnant Woman.* By Dr. De Cristoforis.—The physician to the Ospitale Maggiore, at Milan, in a very admirable report, describes pregnancy as an absolute cause, direct and indirect, of disease in various organs. *First.* The mechanical action of pregnancy as a cause of hydraulic derangements in the circulation; of material alterations in the heart, respiratory centre, intestines, kidneys, and skin, and of morbid phenomena in the peripheral nervous system. The compression on the iliac veins by the uterus is a frequent cause of œdema of the lower limbs and pudendum, sometimes even preventing the examining finger from reaching the os uteri. Sometimes a true mortification of the cellular tissues of the labia majora has resulted, leaving a fistula tedious in healing. Sometimes the vulvo-vaginal gland is involved in the phlegmon. The treatment found useful was repeated puncture of the skin, so as to drain off the effused serum from the distended tissues. A fine needle was employed. Ice relieved the irritation from the punctures. If per-

sistent, this œdema of the pelvic tissues becomes a cause of protracted and difficult labour. He relates examples of extreme varicose dilatation of the pelvic veins, in which some relief was obtained by plugging the vagina with tow, thus supporting the distended vessels.

In another order of cases trouble arises from *mechanical superior arterial hyperæmia*, occasioned by the gravid uterus pressing upon the abdominal aorta and the bifurcation of the iliac arteries. This gives rise to an imperfect distribution of blood, in minus to the lower extremities, in plus to the upper parts. The passage of the blood downwards is impeded. A primary effect of this is hypertrophy of the heart, stimulated to stronger efforts to overcome the mechanical obstacle. He thinks this has more influence than the simple physiological nisus aroused to supply the growth of the embryo and uterus, which Larcher thought was the main cause of the cardiac hypertrophy of pregnancy. He generally discovered a soft prolongation and a blowing character in the first sound of the heart in pregnant women, independently of all organic fault. This disappears after labour. Following upon this distress of the centre of circulation come bronchial catarrh, pulmonary œdema, serous diarrhœa, albuminuria, general subcutaneous œdema of the upper parts. He describes the pulmonary œdema thus:—From the fifth to the sixth month there begin cough and slight difficulty of breathing, increasing with the advance of pregnancy, and almost without febrile symptoms; catarrh disturbs rest, and the patient has to sit up in bed at night; *bleeding, even repeated, gives only short and illusory benefit.* At the end of the eighth week the œdema of the lower parts has extended to the upper, fits of dyspnœa cause increased distress, digestion is impaired, the strength is lowered, and a serous diarrhœa aggravates all. Two or three weeks before the term the patient complains of wandering uterine pains, as of preparatory contraction. A kind of dull, unconscious, slow labour goes on for fifteen or twenty days before true labour can be certainly recognised. The uterus participates in the general adynamia; the sphincter is unable to resist the fœtal part which falls upon it; reflex irritation is set up, and feeble contractions are excited. The orifices once sufficiently dilated, expulsion ensues rapidly, and sometimes without warning. Another feature deserves attention—the separation of the placenta is almost always interrupted, because the contraction of the uterine tissue is slow and imperfect; hence a great disposition to consecutive hæmorrhages. When the uterus is emptied, if no puerperal complication arises, the subsidence of the œdema and then of the circulatory phenomena quickly proceeds. The intestines and kidneys give evident signs of the same hydraulic action. Obstinate serous diarrhœa is the result of the stasis of the arterial and venous circulation of the intestines. Thus also the kidneys—a congestive state is established behind the point of compression of the abdominal circle; the return of the venous blood is retarded, and this remora necessarily extends to the capillaries and arterial circulation. The quantity of blood supplied to the kidneys is thus lessened, and hence these give a

diminished secretion of urine, and a sanguineous plasma is effused from the renal capillaries into the tubuli uriniferi, resulting in albumen in the urine. The author habitually examines the urine of all gravid œdematous, anasarcous women, and says that in the greater part, if albumen was formed in the later months, it quickly disappeared after labour. He affirms that too much weight is attached to albuminuria if it be taken as a certain prognostic of eclamptic fits. He has repeatedly seen albuminuric women go through pregnancy, labour, and childbed, without fits. He has also seen cases in which albuminuria was consecutive upon convulsions.

In cases of death following this order of phenomena De Cristoforis found abundant fluid, frothy, slightly red, on cut surfaces of lungs; the mucous membrane of the small intestines swollen, soft, covered with colourless viscid mucus.

The distress described came on at an earlier period of pregnancy when the uterus was unusually distended, as by twins or hydramnios.

He relates an instructive case, in which *amaurosis* followed upon the conditions described. M. M—, æt. 41, became œdematous at the beginning of the seventh month, then rapidly anasarcous, and was admitted with pulmonary œdema and dyspnœa. The urine was densely albuminous. In twelve days the patient complained of weakness of sight, then of nearly complete amaurosis. On reaching the middle of the ninth month, the symptoms all persisting, a dead ascitic fœtus, macerated, was born. On the seventh day of childbed hydræmia destroyed the patient. Autopsy revealed the usual effusions and the characteristic material alterations of albuminuric nephritis; the left kidney had reached the stage of fatty degeneration. He notices another case of amaurosis, which became chronic.

Another consequence of the distension of the uterus is the stretching of the skin, leaving cicatrices and indelible furrows on the abdomen and the inner and upper part of the thighs. But he relates a striking case, in which similar scars were found in a woman who had never been pregnant.

He next considers pregnancy as an indirect cause of *sanguineous dyscrasia*, of *organic alterations of the heart*, and *nervous disorders*. On one side there is greater activity of the reducing acts, tending to impoverish the blood; and on the other there is an absolute deficiency in the reconstructive acts, both together leading to anæmia, that is, to aglobulism, hydræmia, oligæmia, leucocythæmia. The earliest effects of this state are syncope, palpitations, vague sensations in the head, classed as headache, tendency to sleep, distress in the limbs, easily induced distress in breathing, dyspepsia, often, says De Cristoforis—referring to ideas prevalent in Italy—interpreted as signs of plethora, and being treated by a few bleedings. The loss of muscular tone induces more feeble impulse of the heart; the pulse becomes soft and compressible; the respiratory acts are weakened, inducing deficient oxygenation; the nervous centres, ill nourished, give occasion to syncope, apathy. Nutrition being impaired, the patient emaciates and becomes pale. The loss of fibrine rendering the blood thin, disposes to œdemas. If in this state labour super-

vene, and a moderate loss of blood occur—if labour be protracted—if imprudent movement, as rising in bed, vomiting, &c., takes place—the weakened heart is overpowered, and death easily follows. The heart, badly nourished, falls into denutrition, its tissue retrogrades, and is supplanted by fat.

The nervous system is variously affected. Obstinate vomiting is one effect. It is to be combated by regulated and nutritious diet, with diet rather than medicines, which commonly fail. *Asthmatic attacks* and *tonic cramps* deserve special attention. In the first disorder there are no auscultatory signs; it is a pure neurosis. In the second the spasm is at first localised in the upper limbs, face and neck. Then general clonic contractions occur. The muscles become painful, the limb rigid; attempts to straighten it cause pain. A febrile condition supervenes. This disorder disappears after pregnancy, but is liable to be reproduced in subsequent pregnancies.

Pregnancy considered as a determining cause in the production of morbid forms predisposed by other causes.—He describes under this head a series of cases of dropsy, or serous effusions in the serous cavities, developed by pregnancy in poor women admitted from the aguish districts, exhausted by frequent labours, suckling, hard work, and malaria. He observes that the dropsies are such as pregnancy can produce alone, and which it is more powerful to produce under the cited persisting conditions. Premature labour was a frequent result. This indication the author insists upon; but in several cases, where advancing asphyxia precluded this, gastro-hysterotomy was performed the moment the last beatings of the heart announced the cessation of life.

In the third chapter he describes *retroversion* of the gravid womb, which he says he has met with frequently. With regard to the influence of pregnancy upon existing nervous diseases, he narrates two cases of epilepsy, which at every pregnancy was greatly aggravated, both in the frequency and severity of the fits.—(*Annal. Univ. di Med. Milano*, Jan., 1867.

2. *Clandestine Labour, followed immediately by Infanticide; Expulsion Two Months later of a Hydatid Mole.* Dr. Fischer.—A young woman, pluripara, delivered herself in a wood, threw the infant (born alive) and afterbirth into a stream, and returned to a poor-house. Two months later she passed a mass of hydatidinous placenta. At the base of the mass was a fleshy membranous substance. Near the point of the ovum was a vesicle the size of a grain of coffee, quite different from the hydatids, attached to the walls of the ovum by a whitish cord. This vesicle was considered to be the umbilical vesicle of an embryo that had disappeared. In this case there had been a twin-pregnancy; one of the embryos had died, and the ovum had been transformed into an hydatid mole, too adherent to the walls of the uterus to be expelled at the time of labour of the living child. If this explanation be not admitted, it must be concluded that a true superfœtation, or an impregnation subsequent to the labour, had taken place. As the woman was in custody, the latter event was improbable.—*Annales d'Hygiène*, April, 1867.

3. *Diffuse Myxoma of the Ovum-membranes.* Dr. Breslau.—During a labour Breslau felt an elastic body resembling the head of a macerated fœtus infiltrated with serum, lying in the os uteri. Above this were distinct parts, something like moveable cranial bones. Heart-sounds were heard. A tumour-like sac was attached to the membranes. Between the chorion and thickened amnion was a soft mass like Wharton's jelly, consisting of mucin, with traces of albumen. In some spots of this homogeneous ground-substance was a tissue of nearly sarcomatous character.—*Wien. Medicin Presse*, 1867.

4. *Case of Extra-uterine Gestation.* By Dr. Pletzer.—M—, æt. 40, had had two children. On July 31, 1865, she was seized with sickness and abdominal pain during a sanguineous discharge. Symptoms increased to collapse and peritonitis. These disappeared. Some weeks later the same phenomena were repeated and subsided. A swelling in the pelvis began to increase, growing more to the left side; and some weeks later the parts of the fœtus could be made out. At the normal end of gestation pains set in, ending with slight loss of blood and the discharge of a completely formed decidua. Six months later sickness, constipation, abdominal pain, collapse set in, and death in forty-eight hours. *Dissection.*—A large quantity of bloody serum in abdomen and fresh plastic layers. An embryo of six or seven months' development had passed into the abdomen from a rupture in the left tube, and had grown to the peritoneum and intestines. The fœtus had partially fallen into state of litho-pœdion.—*Mon. f. Geburtsk.*, April, 1867.

II.—Labour.

1. *On the Knowledge of Osteomalacia, especially of old age, and on the Presence of Lactic Acid in the Osteomalacic Bones.* By Dr. O. Weber. ('Virchow's Archiv,' 1867.)
2. *A Case of Utero-intestinal Fistula following on difficult labour.* M. Demarquay. ('Gazette Med.,' June, 1867.)
3. *On Expression of the Fœtus.* By Dr. Kristeller. ('Monats. f. Geburtsk.,' 1867.)
4. *On the Diagnosis of Obliquely-distorted Pelvis.* By Dr. J. Schneider. ('Monats. f. Geburts.,' 1867.)
5. *Case of Spontaneous Rupture of the Uterus.* Dr. Scharlau. (Ibid.)
6. *On the Mechanism of Labour.* Dr. Spiegelberg. (Ibid.)
7. *On the Production of Inverted Uterus.* Dr. J. M. Duncan. ('Ed. Med. Journ.,' May, 1867.)
8. *Case of Complete Inversion of the Uterus after Delivery.* Dr. Inglis. ('Edin. Med. Journ.,' July, 1867.)
9. *On the Relation between the External and Internal Conjugate Diameters of the Pelvis.* Prof. Dohrn. ('Mon. f. Geb.,' 1867.)

1. *On the Knowledge of Osteomalacia, especially of old age, and on the Presence of Lactic Acid in Osteomalacic Bones.* By Dr. O. Weber.—The essence of the puerperal form of osteomalacia, as

also of the senile form, is, according to Weber, the resorption of the salts of lime, beginning in the walls of the Haversian canals and of the medullary spaces. He has also observed a distinct growth of the cartilage which remains after the resorption of the lime-salts, which growth takes place at the cost of the vanishing bone, so that the shape is preserved. This happens under inflammatory symptoms and rheumatic pains. The medulla was found often red, penetrated by small ecchymoses, the vessels of the Haversian canals more developed, and the growing cartilage-insulæ surrounded by blood-vessels. Osteomalacia must be regarded as a kind of ostitis.—*Virchow's Arch.*, 1867.

2. *A Case of Utero-intestinal Fistula following a difficult Labour.*—By M. DEMARQUAY.—The fistula was believed to be formed between the uterus and the small intestines. All the fæcal matters passed through it. No treatment was discovered.—*Gaz. Med.*, June, 1867.

3. *On Expression of the Fœtus.* By D. KRISTELLER.—Dr. Kristeller, quoting a paper by V. Ritgen on "Delivery by Pressure instead of by Extraction," advocates the use of this method in certain cases of inaction of the uterus. He says it has the advantage over extraction of not requiring the premature rupture of the membranes, of not disturbing the flexion of the fœtal-chin upon breast, or limbs upon trunk, or of bringing the uterus into correct relation with the axis of the pelvic brim. It is executed by placing the patient on her back, the operator spreading his hands upon the uterus, so that the palms shall be able to compress the fundus downwards, whilst the thumbs and fingers shall compress the uterus on the sides. The pressure is made to resemble the course of a natural contraction in its progress and periodicity. It may require to be repeated fifteen or twenty times. If more than this is necessary the case is unfit for the operation. Expression, or "squeezing out," of course, will often excite uterine action, which will help the operator. Expression is also most useful in aid of extraction, as, when the forceps is used, Kristeller relates cases in proof of the value of his method. (A modified form of this proceeding has long been taught and practised in this country, where it is the custom to promote the expulsion of the placenta by pressure and friction upon the uterus during the second step—"following the child down."—R. B.)—*Monatsschr. f. Geburtsk*, May, 1867.

4. *On the Diagnosis of the Obliquely-distorted Pelvis.* By Dr. J. SCHNEIDER.—The diagnosis of this deformity is unusually difficult. Dr. Schneider has tested the value of the rules for estimating the internal measurements by external measurements proposed by Naegele. He concludes that these are untrustworthy; there are no constant relations between the two. He believes a better method consists in observing the form and alterations of position of the sacrum; in the projection of the posterior spine, which approaches the middle line; in the greater elevation of the anterior spine of one side; and, by internal examination, in the projection of the promontory to one side.—*Monats. f. Geburtsk.*, April, 1867.

5. *Case of Spontaneous Rupture of the Uterus.* By Dr. Scharlau.—Pelvis generally and obliquely contracted; second labour; spontaneous rupture: escape of child, with exception of the head, into abdominal cavity. Cephalotripsy, turning, death. The following measurements of the pelvis were made:

True conjugate	=	8·5 centim.
Transverse	=	12·6 „
Left oblique	=	11·8 „
Right oblique	=	11·0 „

—*Monats. f. Geburtsk.*, Feb., 1867.

6. *On the Mechanism of Labour.* By Dr. Spiegelberg.—The author contends that the *transverse* position of the head at the brim of the pelvis is more frequent than the oblique. In 700 normal cases observed he found 570 heads in the transverse diameter; in an oblique diameter only 130 heads. The child's back lies to one side of the mother; the head with its sagittal suture in the longest or transverse diameter of the pelvis. If the transverse diameter of the uterus and of the brim are parallel, the sagittal suture enters transversely; if the transverse diameter of the uterus lies in an oblique diameter of the pelvis, the head will enter the brim in this direction. The uterus, in addition to its common inclination to one side (Dubois), is sometimes twisted a little on its axis in the direction of its inclination.

In whatever direction the head enters the pelvis, in that will it be driven into the cavity; and will only make its rotation on its vertical axis in the lower half of the pelvis.—*Mon. f. Geb.*, Feb., 1867.

7. *On the production of Inverted Uterus.* J. M. Duncan,—Dr. Duncan contends that the essential condition of inversion is paralysis or a passive state of the uterus. The same view was first advanced and clearly illustrated by Prof. Lazzati and his assistant, G. Casati, of Milan.—*Edin. Med. Journ.*, May, 1867.

8. *Case of complete Inversion of the Uterus after Delivery.* Dr. Inglis.—The patient was a primipara: pains came on suddenly and strongly. Chloroform administered. Face directed to right groin. The occiput came round nearly to left groin in course of labour, but progress then ceased. The forceps was applied for a short time. Head was ultimately expelled by natural efforts. The placenta was found expelled, and on being handled great pain was felt. The inverted uterus was inside the placenta. The placenta was partly adherent; it was peeled off. The whole uterus was gently compressed, and a depression made in the fundus with both thumbs; the yielding was very gradual, and the uterus did not lose its tendency to invert until it was entirely replaced from within and grasped from without. Pulse 80; no sign of shock. She recovered without marked symptoms. An interesting feature is insisted upon by Dr. Inglis. The occipito-posterior position, he says, occurred in seven out of nine cases of inversion with which he was acquainted. He believes that the rotation of the occiput forward led to the inversion.

[1] In the Appendix to the new 'British Pharmacopœia,' are tables giving the relations of metrical measures to the English.

By rubbing round against the inner surface of the cervix, the head produces an extremely complete and extensive dilatation, and, in time, also an increased expulsive action which may become inordinate. If then expulsive action should come on in such a case, before the cervical portion of the uterus has regained its tone, either hour-glass contraction or inversion of the uterus will certainly follow.—*Ed. Med. Journ.*, July, 1867.

9. *On the relations between the External and the True Conjugate Diameters of the Pelvis.* By Prof. Dohrn.—The results confirm the conclusions of Michaelis, Credé, and Schröder. In the flattened pelvis the subtraction to be made from the external conjugate to arrive at the true conjugate is liable to the greatest variations. The measurement of the external conjugate on the living subject cannot be depended upon as a means of estimating the true conjugate.—*Mon. f. Geb.*, April, 1867.

III.—Puerperal.

Contributions to the Knowledge of Acute Fatty Degeneration in Puerperal Women and New-born Children. Dr. C. Hecker.—*Mon. f. Geb.*

Contributions to the Knowledge of Acute Fatty Degeneration in Puerperal Women and New-born Children. By C. Hecker.—The author was induced, by reflecting upon a case of acute yellow atrophy of the liver which he had observed, to study the connection of this disease with pregnancy more closely. He thinks he is now in a position to affirm that puerperal women are liable to a disease running rapidly, even suddenly to death, soon after labour; the symptoms being obscure, without jaundice or intestinal hæmorrhages, and only recognised as acute fatty degeneration on dissection, the basis of which was laid in pregnancy. There are similar phenomena in new-born children. He gives cases:—1. Normal labour; child dead. Profuse hæmorrhage, after expulsion of placenta; sudden death an hour after labour. The child had probably been dead eight days. On dissection, the brain was found anæmic, lungs somewhat œdematous; no trace of thrombosis in the pulmonary arteries; heart small, pale, flaccid, containing hardly any blood; valves sound; tissue showing no change under microscope. Under the peritoneal coat of the liver were considerable blood-effusions, so that the colour of the surface was deep red; the substance was brown-red, volume somewhat increased. On section, the blood effusions could be traced along Glisson's capsule to the finer branches of the vena portæ, so that the parenchyma looked as if sprinkled. Under the microscope the liver-cells were seen infiltrated, the pigment-nuclei increased, numerous white blood-corpuscles, free nuclei and molecules. The kidneys were in the condition of so-called "turbid swelling," and in the fluid a crowd of nuclei were floating. There was submucous blood-effusion in the renal pelvis, and small ecchymosis in the mucous membrane of the bladder. In another case a pluripara was delivered after a face-presentation. Profuse hæmorrhage during expulsion of placenta. Death of mother forty-five hours after labour, under

obscure symptoms. It was found that the liver was in an advanced stage of parenchymatous inflammation, the liver-cells being much destroyed, and infiltrated with fat.

As to the new-born children, Hecker calls attention to the memoirs on acute fatty degeneration of new-born domestic animals, by Fürstenberg and Roloff, in 'Virchow's Archiv.' (1864-5). He relates the case of a child that died ninety hours after birth, in which the liver was found smaller than natural, was bright yellow, like a goose's liver, and showed the character of acute liver atrophy; the parenchyma was completely destroyed, and under the microscope were seen fat droplets and molecular detritus. The kidneys showed uric acid infarctus, and considerable epithelial degeneration.—*Monatssch. f. Geburtsk*, 1867.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

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On the Efficacy of Mercury in the treatment of Syphilis. Discussion at the Société de Chirurgie, in Paris.—A discussion has lately taken place in Paris as to the value of mercury in the treatment of syphilis, and many of the most distinguished surgeons of France have given their opinions upon the subject. The general conclusion has been favorable to the employment of this metal, although some of the speakers limited its use to certain stages of the disease, and one denied its efficacy altogether. The last-named speaker, M. Després, who had derived his experience from the Hospital de l'Ourcine, is decidedly opposed to the employment of mercury in syphilis, and prefers a tonic and supporting treatment. His statistics, founded upon 234 cases, show that 28 in 100 returned uncured after a mercurial course, while only 10 in 100 returned uncured after a more or less completely tonic treatment. He thinks that the tonic treatment is the physiological remedy for syphilis, and that when the mercurial plan succeeds, it is only owing to the fact that it is combined with a good tonic regimen, which is the true therapeutic agent. It should be stated that M. Depaul energetically disputed the statistics of M. Després, and considered them as of no value, because the latter surgeon had not taken sufficient time to follow out the cases, and because he had not sufficiently studied the relative value of the mercurial and the expectant treatment. M. Depaul affirms that mercury acts in a special manner on the pathological products of syphilis, and removes new formations: he has himself observed the rapid cure of persistent head-affections, the disappearance of multiple tumours of the tongue, &c. In answer to the question whether mercury ought to be administered, in syphilitic affections, to pregnant women, M.

Depaul answers in the affirmative, and he declares that in many cases where the syphilitic mother has borne dead children, or has aborted, she has afterwards borne living children when mercury has been administered during the pregnancy; and again, when the syphilitic taint has been contracted during the pregnancy, a mercurial course will ensure the production of a living child, while, without such treatment, the probability is that the child would be born dead. Syphilitic infants ought, according to the same surgeon, to be treated by mercury, which in these cases acts in an almost wonderful manner. The editor of the 'Bulletin Général de Thérapeutique,' in commenting on the discussion in the Société de Chirurgie, states that, with the exception of M. Després, who is at the commencement of his surgical career, all the speakers expressed the opinion that mercury is a very valuable and often indispensable medicine in the treatment of syphilis. The anti-mercurialists allege that syphilis is only a poison introduced into the blood, and that nature tends to eliminate it; that the cutaneous symptoms of all kinds are only different forms of elimination, and that the work of nature, therefore, ought not to be disturbed, but should rather be supported by a tonic regimen. But it is answered that nature often eliminates the poison very imperfectly, and that then a little assistance is of great service, and that the mercurial preparations are the best assistants which medicine can offer. It is well known that syphilis, like other diseases, will be cured spontaneously; but it does not therefore follow, say the mercurialists, that mercury is useless. The fact is that syphilis varies very much in its character, according to the region it attacks, and the constitution of the individual who suffers from it; and it is well known that of two men contracting the disease from the same woman, one will speedily get rid of the poison, while the other will suffer the whole series of secondary and tertiary symptoms. Another cause, according to the writer in the 'Bulletin,' which has caused great difficulty in the therapeutics of syphilis, is the confusion which so long existed as to the nature of chancre before the distinction was made between the soft and the hard chancre. The former never, or very rarely, causes constitutional syphilis, and may get well without any internal treatment; and this fact lends great weight to the views of the anti-mercurialists. Among the practitioners who advocate the use of mercury in syphilis, very different opinions prevail as to the time and manner of administering the drug, and M. Diday, of Lyons, for example, does not give mercury for the primary sore, or at least he gives it but seldom. He prescribes it only after a few weeks, and when the persistent induration proves that the constitution is affected; nor does he give it in secondary symptoms, if they are slight, and if the constitution of the patient indicates sufficient strength to resist the morbid agent. But he gives it if the first eruption, instead of consisting of roseola or distinct papules, consists of deep-coloured spots, scales, or pustules. In reference to the period when mercury ought to be given, there was considerable difference of opinion expressed by the speakers, some, as MM. Dolbeau and Perrin, preferring to wait for the appearance of the eruption;

while others, as MM. Cullérier, Verneuil, Guérin, &c., recommended that it should be given immediately; and this view is advocated by the editor of the 'Bulletin,' who argues that as the diagnosis between the hard and soft chancres may now be easily made, there is no good reason for delay. He thinks that the anti-syphilitic treatment should be commenced as soon as it is evident that the primary sore is an infecting one. This is moreover the opinion of M. Ricord, who considers the induration of the chancre as the first secondary manifestation. The tertiary symptoms of syphilis did not occupy much of the attention of the speakers, because all were agreed as to the efficacy of the iodide of potassium; but this medicine sometimes fails, and it appears to act better when its use has been preceded by a mercurial course. In concluding the analysis of the discussion, the editor of the 'Bulletin' lays down the following general rules as to the treatment of syphilis. When the infecting nature of the chancre is recognised, it is best to begin the treatment as soon as possible. The mercurial preparations should vary according to the kind of patients, and if one preparation does not seem to succeed, another must be tried. Syphilis may be radically cured, provided that the mercurial treatment and the administration of iodide of potassium are accompanied by a restorative regimen, and abstinence from all kinds of excess.—*Bulletin Général de Therapeutique*, July 30, 1867.

On the Employment of Woorara as a Therapeutic Agent. By Dr. Lussana, Jousset, and others.—In connection with the investigation of the poisonous properties of woorara when administered to the lower animals, the question has been raised whether this agent might, under any circumstances, be employed in medical practice. Dr. Lussana, of Parma, thinks that such a proposition would be unjustifiable, because we possess no certain standard by which the effects of this substance can be measured in the human subject, and because we know of no means by which its poisonous effects could be counteracted. But Dr. Jousset, of Bellessme, while admitting that the therapeutical effects of woorara have not hitherto been encouraging, thinks that in such a fearful disease, for instance, as tetanus, the use of even dangerous remedies may be admissible, and that woorara ought not to be altogether banished from the domain of medicine. He refers to the well-known depressing power of woorara on the motor system of nerves, and, therefore, he argues it should not be denied that such an agent may have a certain operation on diseases in which the activity of this system is in excess. The degree of participation of the nervous centres, both motor and sensitive, cannot be accurately determined in many diseases, but in tetanus, especially the traumatic form, the affection of the motor nerves appears to be excessive; while in other convulsive diseases, as epilepsy, hysteria, &c., there is an affection of the nervous centres with reflex action upon the motor and sensitive nerves. Dr. Jousset thinks that woorara might be employed in tetanus, because when suitably administered and in a sufficient dose, it has been known to cause relaxation of the muscles in a sufficient number of cases to justify us in regarding it as an heroic

remedy in this disease. More recently Dr. Auguste Voisin, of the Bicêtre, in Paris, and M. Liouville, interne of the Paris hospitals, have communicated to the French Institute a paper on the experimental and clinical studies of the woorara poison. From their investigations on the effects produced by this poison on some of the lower animals, they were induced to try its powers upon the human subject, the patients treated being twelve epileptics in the Bicêtre. They began, however, with exceedingly small doses, which produced no appreciable effect, and they cautiously increased them. They used the poison by hypodermic injection, endermically (by vesication), and internally by the mouth and anus. The dose administered by the last-named channels was forty centigrammes (a centigramme being the hundredth part of a gramme, which is equal to about fifteen grains). The general effects varied according to the dose and the mode of application, but were well marked, the local phenomena being those of irritation, and the general symptoms being those of fever, consisting of disturbance of the circulation, respiration, calorification, and motion; and of hypersecretions, and of symptoms connected with the cerebral and vesical functions. With respect to the therapeutical value of woorara, MM. Voisin and Liouville do not profess to have made much progress; and in the case of epilepsy all they state is that, if there is not much at present to be adduced in favour of woorara, at least there is nothing to be said against it. Both by experiment upon the lower animals and by clinical observation they have learned that the treatment of poisoning by woorara consists in the application of a ligature round the limb between the part injected and the heart.—*Schmidt's Jahrbücher der Gesammten Medicin*, September, 1866, and *Edinburgh Medical Journal*, January, 1867.

On the Use of Chloride of Iron in certain Acute and Chronic Affections of the Skin. By Dr. Bedford Brown, of Washington. —Although the affections of the skin are of varied origin, character, and progress, and therefore require a great diversity of remedial means for their treatment, yet Dr. Bedford Brown believes that the chloride of iron possesses such a variety of beneficial properties as to render it extensively applicable in such cases. From its tonic and alterative powers, together with its very active astringent or hæmostatic properties, it seems to be peculiarly adapted for the control of those affections which are seated in the vascular network of the skin, whether acute or chronic; and a very considerable experience has convinced Dr. Brown that few remedies, if any, possess such decided virtues as the chloride in skin-diseases. One of the leading points in the treatment of cutaneous affections, according to Dr. Brown, is the control of the active and passive forms of vascular dilatation; and as the chloride of iron is capable of exercising this control, it may even prevent smallpox from becoming confluent, and acute cutaneous affections from becoming chronic. For it is excessive engorgement, says Dr. Brown, which converts discrete variola into the confluent form, and it is passive engorgement or dilatation which converts a transient cutaneous

affection into a chronic form. From the fact that chloride of iron possesses hæmostatic powers, and that it has even effected the cure of aneurism by its power over the action and dilatation of the large vessels, Dr. Brown argues that it may and does limit the extension of skin-diseases and diminishes the purulent secretions by which some of them are characterised. The diverse character of cutaneous inflammations and their tendency to assume the chronic form are due in great part to excessive vascular communication and a low standard of blood-plasticity and nutrition; and as iron promotes a more healthy condition of the blood and its vessels, it is especially adapted to the limitation or circumscription of such inflammations. In the treatment of smallpox in its worst form Dr. Brown believes the chloride of iron to be decidedly efficacious, as it modifies and diminishes inflammatory action and curtails the process of suppuration, confining these processes, as it were, within safe limits. Dr. Brown cites some cases in which the chloride was used beneficially in the treatment of smallpox, the patients passing successfully through the disease under its employment, and he even claims for it the possession of abortive powers in the more serious forms of the disease. In carbuncle, in chronic pustular and vesicular affections, in impetigo and ecthyma, and in phagedænic affections of the skin, Dr. Brown has employed the chloride of iron with the most satisfactory results. From its power of improving the condition of the blood it seems to promote the healing of sores; and from its property of controlling and regulating the process of suppuration, it is most admirably adapted to the treatment of those chronic skin affections in which suppuration is a leading element. Dr. Brown prefers the tincture of the chloride of iron to the watery solution of the salt.—*The American Journal of the Medical Sciences*, 1866.

On the Therapeutical Properties of Podophyllin. By Dr. Ernst Schmidt.—Dr. Schmidt reports on the properties of podophyllin, on which he has made some special investigations during a residence of nearly nine years in America. The drastic property of this substance he states to be ten times as powerful as that of the resin of jalap, half a grain to a grain of podophyllin acting as powerfully, although somewhat more slowly, than five to ten grains of the other drug. Larger doses, as four to six grains, prescribed in bilious fevers, occasionally caused griping, nausea, giddiness, sweating, and persistent watery stools, without, however, causing gastro-enteritis, as is repeatedly observed after large doses of other drastic medicines such as jalap and croton oil. Dr. Schmidt never employed such large doses, but in strong constitutions he has given half a grain to a grain every one or two hours; and only on one occasion he was obliged to give two grain-doses in a case of lead colic. After a few half-grain doses, or one dose of a grain, abundant stools generally ensued in from four to six hours; but if a more rapid operation is required, a couple of grain-doses may be given with an interval of an hour. Dr. Schmidt states, as a remarkable fact, that with the stools a very large quantity of intestinal mucus and bile was evacuated. Occasionally in children the above-mentioned moderate

doses caused at first some vomiting, succeeded by the evacuation of considerable quantities of mucous and biliary matters, so that no injury resulted. Dr. Schmidt thinks that this action only occurs when accumulations have been formed in the upper part of the intestinal canal, and he regards it as directly favorable in many cases, as, for instance, in children in whom there are symptoms of cerebral irritation arising from errors in digestion. He has found podophyllin especially useful in habitual constipation arising from sluggishness of digestion and of biliary secretion, and also in congestions of the portal system, in which cases he has found that doses of one twelfth to one sixth of a grain given once or twice a day have been followed by good results. The doses which are found suitable may be continued for a considerable time, as it is a remarkable property of podophyllin that its action is not suddenly weakened. Dr. Schmidt recommends this remedy also in the treatment of intestinal worms. In one case, in a woman, after two doses of a grain each, four pieces of a round worm were broken off, after other anthelmintics had been employed without success. In children he has repeatedly given moderately purgative doses of podophyllin for the *oxyuris vermicularis*, and after the purging has commenced he has used injections containing an emulsion of castor oil, with which half a grain to a grain of podophyllin was added. Of the operation of podophyllin in tape-worm he has no experience, but he believes that in recent cases of *trichinosis* it would act favorably by the copious evacuation of the mucus of the stomach and bowels.—*Schmidt's Jahrbücher der Gesammten Medicin*, November, 1866.

On the Use of Chlorine in the Treatment of Asiatic Cholera and Choleraic Diarrhœa. By W. M. Dobie, M.D.—Dr. Dobie's observations on the use of chlorine in cholera were made in Chester, where the disease prevailed in the month of September of last year. At the commencement of the visitation the cases were of a very malignant type, and the mortality was very great, the first six cases having all died. The first case on which the chlorine was tried also died; but the partial improvement which was observed to follow this treatment induced Dr. Dobie to administer it in other cases. It was accordingly given to many patients, both in the cholera hospitals and in private practice, and the results are briefly recorded. These were more satisfactory in the hospital, as in that establishment the air was purer, the nursing good, and the other appliances of the best description, but Dr. Dobie attributes great remedial power to the chlorine. He states that of 34 cases of cholera treated on the ordinary plans, 8 recovered and 26 died, while of 18 treated with chlorine 10 recovered and 8 died. The chlorinated mixture employed by Dr. Dobie differs from that prepared by the druggists, and he gives a formula by which it may be made to contain the largest proportion possible of chlorine. His plan consists in *first* mixing together chlorate of potash and hydrochloric acid, and then adding the water, for he believes that when the chlorate is dissolved in a considerable quantity of water, and the hydrochloric acid added afterwards, as is usually done, very little chlorine is developed. The dose of the

chlorinated mixture was half an ounce every hour or two hours, and if the first dose was vomited another was given after a shorter interval. In the numerous cases of choleraic diarrhœa, which likewise occurred in Chester, the chlorine mixture, in Dr. Dobie's hands, proved eminently beneficial, and he believes that many of the cases would have passed into true cholera if the disease had not been checked in the early stage.—*Edinburgh Medical Journal*, March, 1867.

On the Anæsthetic Properties of the Tetrachloride of Carbon. By Dr. A. E. Sansom.—The introduction of the tetrachloride of carbon into medical practice, although attributed to several other authorities, appears to be really due to Dr. Sansom and Dr. John Harley, who made the first experiments upon this substance as an anæsthetic in the year 1864, and the record of their experiments was made in Dr. Sansom's book on chloroform, published in May, 1865. Other practitioners have subsequently published their experience as to the anæsthetic properties of the tetrachloride, which has been employed on several occasions in the human subject with success. It is said by Dr. Sansom to be pleasant to inhale, and he finds the symptoms of commencing anæsthesia, in his own person, to be not unpleasant, and he has not found any patient object to its administration. It has been employed for the relief of irritation, as in hay-asthma, for subduing pain, for controlling palpitation of the heart, for treating the spasms of chorea, for the induction of sleep, for relieving or abolishing the pains of labour, and for inducing anæsthesia in the performance of surgical operations. Dr. Protheroe Smith has found it relieve the pain of headache, toothache, dysmenorrhœa, and labour. In cases of natural labour the tetrachloride has been employed by Dr. Protheroe Smith, Dr. Black, and Dr. Sansom; and in none of these cases (eight in all) has it manifested any unfavorable effect; and it has greatly relieved, if it has not altogether abolished the suffering. In Dr. Sansom's cases that gentleman came to the conclusion that it increased muscular power, and certainly in no case did it suspend the efforts of labour. In the performance of surgical operations a state of narcotism is necessary; and it would appear, according to Dr. Sansom, that the prolonged employment of the tetrachloride in such cases is undesirable and likely to be injurious. So far as the earlier stages are concerned, the action of the tetrachloride is beneficial, as it is stimulant, anodyne and hypnotic, and produces no unpleasant effect; but its ponderous vapour, its insufficient volatility, and the consequent difficulty of its elimination from the system, are sufficient reasons against its employment in anything like large doses. Dr. Sansom remarks that the tetrachloride may be obtained in a state of purity at a small cost, but that at present its manufacture is not perfect, and that some of the other chlorides of carbon are occasionally mixed with it. Some of the specimens which he has examined gave obvious indications of containing bisulphide of carbon, and this is a deleterious contamination. The tetrachloride of carbon may be purified by agitating it with an alcoholic solution of potash, washing with pure water, and subsequently redistilling. On the whole, Dr. Sansom

does not regard the tetrachloride as being a perfectly good anæsthetic; but he thinks that, in several instances, it may be advantageously mixed with chloroform, as it has some properties which are antagonistic to those of the latter substance, and may, therefore, modify its action. The use of tetrachloride of carbon may be made to precede that of chloroform; and, although he invites further experiments upon the point, Dr. Sansom thinks that a mixture of one part of the tetrachloride of carbon and six of chloroform will constitute a safe, as well as agreeable, anæsthetic.—*British Medical Journal*, Sept. 7, 1867.

On the Resin of the Veratrum viride and its Therapeutical action. By Dr. Stöhr, of Würzburg.—The resin of the *Veratrum viride* employed by Dr. Stöhr is an extract from the root of the plant, which in the dry state ought to yield a bright brown, faintly-smelling powder, with a slight taste. The cases in which Dr. Stöhr employed this resin included several of pneumonia, one of acute rheumatism, one of measles, and one of emphysema, in the last of which there were very distressing palpitations of the heart. In this last case the patient took four pills, each containing one-sixth of a grain of the resin with one-eighth of a grain of morphia, at intervals of half-an-hour. At the third dose the temperature of the extremities was reduced, the frequency of the pulse was lowered, the breathing became quieter, and the palpitations ceased in two hours, and after the fourth pill nausea supervened. The general effects produced by the resin of the *Veratrum viride* were similar to those just mentioned—namely, reduction of the temperature, of the pulse, and of the frequency of respiration. Dr. Stöhr considers that the use of the resin is indicated only in those diseases in which the pulse and the temperature are unusually high, because in such cases a very rapid remedial action is desirable, and the unpleasant effects are outweighed by a quickly successful result. When the medicine is employed, its rapid and energetic action must be recollected, and the patient should be carefully watched.—*Schmidt's Jahrbücher der Gesammten Medicin*, December 10, 1866.

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,
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On the Pathogeny of Fatty Transformations, with Experimental Researches on the Action of Phosphorus on the Living Tissues. By Dr. L. Ranvier.—The author combats the idea accepted by Virchow, that in the case of fatty transformation following inflammation, the former may be considered a part of the inflammatory process. Numberless facts show that inflammation and fatty degeneration,

instead of being connected, are in reality opposed. In inflammation of the subcutaneous cellular tissue fatty cells lose the fat which they contain; their nuclei and the little mass of enclosed protoplasm give place by division to an abundant production of cells, which fill the old adipose vesicle. In acute osteitis we find the fatty medulla transformed in like manner into proliferous tissue. In acute or chronic inflammations of joints, the cartilage cells, which, physiologically, contain fat, are deprived of it as long as cellular proliferation continues. This disappearance of fat in elements which physiologically contain it is met not only in inflammatory processes, but also in all active new formations. Thus, when new growths take their point of departure from cellulo-adipose tissue, or from the medulla of bone, they determine the disappearance of fat in the cells which they invade. But a more important fact still is that when, under the influence of a pathological cause, fatty transformation has invaded certain cells, it is observed that these cells may free themselves from the fat which they contain under the influence of inflammation, provided it supervene before the cellular elements are completely destroyed by the degeneration. Nevertheless, at the end of the inflammatory process, and in the last stage of every new formation, a fatty transformation is observed of superabundant elements. This transformation must not be attributed to the original irritative action, but rather to an alteration of nutrition, for it never supervenes when the cells are in full proliferation. It occurs only when the former movement is arrested, and when nutritive changes go on with difficulty in those elements of which the number is excessive in proportion to the vascular supply. The first day of a catarrhal inflammation the exudation is transparent, and the numberless cellules which it contains afford abundant evidence of active multiplication, but do not enclose a single fatty granule. Later, when the exudation becomes yellow and opaque, the contents of the cells are nearly all granules and globules of fat. The author sums up the results of his experiments on animals and observations in the following conclusions: The protoplasm of cells appears to be the seat of changes in, and of the elaboration of the materials brought by the blood; it is also in the protoplasm that fat is at first deposited. The presence of fat in a cell which does not apparently contain it in the normal state proceeds from a diminution of the nutritive movement of that cell. If the movement is rendered active by irritation, fat disappears. Certain cells have considerable activity in the elaboration of fat, but it does not follow that they form it from other materials with which they are brought in contact, for blood contains fat in a metamorphosed state, that is to say, in a state of combination or of saponification. Fat-cells, the cells of the liver, &c., seem then to bring back such metamorphosed fat to the neutral or insoluble state. In dead fœtuses retained some weeks in the uterine cavity, metamorphosed fat becomes apparent to microscopic observation in the liver, the kidneys, the heart, and cartilage cells. Phosphorus determines fatty transformations, because it enfeebles the nutrition of histological elements.—*Gazette Médicale de Paris*, July 6 and 13, 1867.

On a Form of Fever which Prevailed throughout certain Districts of Western Malwa in 1865. By H. C. Brodrick, M.D., 1st Regiment Central India Horse.—The author was employed by the Indian Government to report on a very fatal form of fever known in the districts of Western Malwa by the name of *Gujrati Beemari*, and amongst the Mussulmans as *Yerkan* or *Peelia*, both terms indicating jaundice. The disease is described as an ardent fever, without remissions, attended with congestion of the liver, pain in the side, and jaundice, and with congestion of the lungs or pneumonia, indicated by difficulty of breathing, cough, and bloody expectoration. The skin and conjunctivæ were yellow, and the perspiration stained the clothes of a yellow colour. The disease prevailed *pari passu* with cholera, and the symptoms of both seem to have been confounded by the native population. The author believes it to have been a form of jungle fever, without remissions. Relapses, however, were observed. The disease was generally fatal on the third day. In one village visited, which originally contained fifty houses, twenty-five had been desolated by the fever. There was no evidence that it was contagious. The author has treated several cases in the Cavalry Hospital at Augger of a fever which he believes to be identical with the *Gujrati Beemari*. They were very unpromising cases, but yielded to quinine in doses of from ten to twenty grains, with animal broths and alcohol. The author suggests that this disease may be identical with Palee plague.—*Madras Quarterly Journal*, October, 1866.

On Leucocythœmia and the Pathogeny of Hæmorrhages and Thromboses which supervene in that affection. By MM. Auguste Ollivier and Louis Ranvier.—The authors observe that Virchow, and the principal writers of Germany who have discussed the pathological changes in leucocythœmia, have considered the presence of white globules between the hepatic cells or the renal tubuli as the result of *hyperplasie* of the interstitial connective tissue of the organs. This interpretation seems at first sight to account for the enormous production of white globules, although it does not explain the manner in which they penetrate the circulatory system. Since the researches of Reclinghausen there has been a disposition to consider the plasmatic network of the connective tissue as the origin of the lymphatics, and to explain the presence of globules in their final ramifications, by supposing that they are penetrated by the cells of that tissue. It follows that every multiplication of these cells would throw a great number of globules into the lymph, and thence into the blood. On the other hand, we know that lymphatic tumours may develope in a large number of organs without determining leucocythœmia. The observations of the authors have led them to give a different interpretation to the facts. In a case of leucocythœmia they believe they have observed, both in the liver and in the kidney, a connection of continuity between the interior of the blood-vessels and the islets formed in the stroma of these organs by an accumulation of white globules. Hence the presence of white globules in these islets results from capillary hæmorrhage, and moreover, in the kidney they

succeeded in discovering some red globules mixed, in small proportion, with the white. As to organized lymphatic new formations, they are inclined to consider them as having two origins—the one a hyperplasie of the connective tissue, the other the formation of a lymphatic tissue developed at the expense of extravasated white globules. They look on the white globules as embryonic cells, capable of forming a new tissue. In the majority of cases the absence of red globules in the meshes of the connective tissue is explained by the rapid destruction of these globules when extravasated. The case examined by the authors exhibited multiple cerebral hæmorrhages, and also coagulations of blood in the crural and other arteries. The latter are frequent in leucocythœmia. According to Förster, they would be due to a chemical alteration of the plasma of the blood, although it is difficult to conceive how the same modification could produce at once hæmorrhage and thrombosis. The authors offer the following interpretation: The red globules of the blood circulate easily in the capillaries because they are smooth and reducible. White globules are larger in man, rugous, and possess the property of adhering. If the white globules become very numerous the capillary circulation will be difficult; and if, as in the case they observed, the number of white globules is larger than that of the red, and their size attains $\frac{12}{1000}$ of a millimètre, as frequently happens in leucocythœmia, they think that the capillary circulation would be extremely slackened, and even suppressed in some organs. Hence there will be a detention of globules in the capillary network, and a sufficient tension to determine the rupture of the vessels. Stagnation of blood will equally account for coagulation in the arterial or venous trunks. The most powerful of the causes producing thrombosis is arrest or slackening of the circulation.—*Gaz. Médicale de Paris*, June 15, 1867.

The Influence of Disease upon the Weight of the Body. By Thos. Layton, Interne, Paris.—The author's observations were undertaken at La Charité, under the direction of Prof. Monneret. In regard to acute diseases, two principal periods are noticed—those of loss and reparation. Whilst the period of loss lasts the weight diminishes, as a rule, rapidly, and from the proportion of its diurnal variations an idea may be formed of the relative gravity of the case. The author has seen a decrease in typhoid fever of more than 500 grammes daily. In reparation the increase of weight is at times sudden and unexpected, and is ushered in before other symptoms would warrant the announcement of convalescence. This fact is so striking that but little hesitation need be felt in declaring convalescent the patient whose weight begins to ascend, even though, from the indications usually accepted, this announcement may seem premature. The series of weights taken during convalescence furnish the means of knowing whether this state of reparation will be short or tedious, as a steady and rapid augmentation accompanies a speedy return to health, while the contrary obtains when the intermediary condition between health and disease is destined to drag on slowly. The author has made these observations on weight coincide with thermo-

metric investigation, and has found that an elevation of temperature is attended by a diminution of weight; or in other terms, the higher the fever the lower is the figure given by the scales. The author's paper contains a number of observations of the variations of weight in different acute and chronic diseases.—*Southern Journal of Medical Sciences*, August, 1867.

Case of Cholesteatoma. By Dr. F. Bateman.—T. H—, æt. 26, a tall, well-made farm labourer; admitted into Norwich and Norfolk Hospital on July 7, 1866. *History.*—Been married four years; had one child twenty-one months old; never had syphilis; had always been a steady man, and had enjoyed good health until nine months after marriage. First morbid symptom, a shaking first of right hand, then of left; this was followed in a few months by frontal headache. About a year ago weakness of legs set in and inability to guide himself, but intelligence and memory remained unimpaired; about this time slight embarrassment of speech was observed, this was followed by dimness of vision, which resulted in complete blindness, the right eye being first affected. *Symptoms observed on admission.*—Staggering gait; no actual paralysis of limbs, but perhaps a little less muscular power on right than on left side; sensation unimpaired; difficult micturition; no want of power over rectum. He complains of severe frontal headache, is deaf on the right side; there is double amaurosis, the pupils act, though sluggishly; no apparent disease of the eye itself. Intelligence and memory unimpaired; he converses coherently, with well-selected words, but he has an unnatural jerking, spasmodic manner of speaking, as if the same want of power of co-ordination in the act of volition which is apparent in his gait extends to the expression of his ideas. After a few days the frontal headache increased in intensity, he became drowsy, and died suddenly on the eleventh day after admission. *Necropsy*, thirteen hours after death.—Body well nourished; lungs sligthly adherent at both apices; heart healthy; kidneys, liver, and spleen intensely congested. Brain congested, but extremely firm. Arachnoid contained five and a half ounces of cerebro-spinal fluid; ventricles dilated with fluid. On removing the brain, "a glistening tumour was found at the base of the cranium, adherent to and apparently springing from the posterior surface of the petrous portion of the right temporal, and from the basilar process of the occipital bones." "Tumour was flattened from above downwards; two and a quarter inches in its longest, one and a quarter in its shortest diameter; its surface uneven, of a white silvery appearance, resembling mother-o'-pearl." It was invested with a membrane of the thickness of ordinary paper, which enclosed, as in a cyst, a substance of the thickness and consistence of wax, the central portion being of a dirty straw-colour, not unlike tallow, but becoming paler, of an almost pure white, and firmer as it approached the circumference of the tumour. It had no farther adhesion, and there was no trace of vessels in its structure. It lay against the under-surface of the right lobe of the cerebellum, where an excavation existed for its reception; it pressed on this lobe, also on the right side of the medulla oblongata, the right crus

cerebelli, and the right side of the pons varolii in its whole extent. A portion examined microscopically exhibited a number of cells, crystals of cholesterine, and fat-globules. A portion was examined by Dr. Beale and pronounced cholesteatoma. The author refers the unsteadiness of gait to impairment of the functions of the cerebellun by pressure; the amaurosis to pressure by the anterior extremity of the growth upon the posterior and inferior surface of the right thalamus opticus, and, consequently, upon the origin of the optic nerve (the double amaurosis being explicable by the decussation of the fibres of the optic nerve at the commissure); and the deafness to pressure on the portio mollis, as its winds round the corpus restiforme, and also at its entrance into the meatus auditorius internus. The author believes this tumour to be the largest of this description and in this situation recorded. The ataxia of articulation he thinks was probably due to pressure on the origin of the glosso-pharyngeal and hypoglossal nerves.—*Beale's Archives*, No. xvi, vol. iv.

Anatomico-pathological Considerations on Abscess of the Brain. By M. E. Gintrac.—Suppuration and ramollissement are the modes of alteration by which encephalitis terminates, or under which it most frequently presents itself, since out of 410 cases there were only 29 in which these conditions were not established; suppuration numbers rather more examples than inflammatory ramollissement. Ramollissement is notably more frequent in the cortical substance, in the anterior lobes of the brain, in the corpora striata and optic thalami, and in the pons varolii. Suppuration has been more frequently observed in the middle and posterior lobes of the brain, and in the cerebellum. It is also the most frequent termination of cerebral or cerebellar inflammation accompanying alteration of the bones of the skull, and principally of the petrous portion of the temporal. Pus which forms in the brain may be exuded on its surface, or disseminated in its tissue, or collected at one or more points of its extent. The first mode is rare; the second is sometimes observed with ramollissement, to which it gives a yellow-greenish tinge, great fluidity, and a peculiar odour. The third (abscess) is the most common. Lebert divides abscesses of the brain into multiple and solitary. But in circumscribed localities abscesses are not always solitary—sometimes two, but rarely a larger number are met with in the middle or posterior lobe, or in the cerebellum. Lebert has noted that the left hemisphere was more often affected than the right, twenty-three to eighteen. M. Gintrac has collected ninety-three cases of abscess of the right half of the encephalon, and eighty-five of the left. It is principally in the medullary substance of the different lobes of the cerebrum that purulent collections are found, whilst the cortical substance is almost exempt. But the cerebellum, which is abundantly provided with grey substance, is pre-eminently subject to abscess, and the pons very little so, although principally formed of white matter. Cerebral abscesses, however, arise principally in the medullary substance, which, although less vascular than the grey, is not, therefore, less apt to inflame and suppurate. The volume of the abscess is sometimes considerable, varying from the size of a nut to that of a

pigeon's egg, a hen's egg, and an orange. The author has seen the hemisphere converted almost entirely into a sac full of pus. The form of the abscess is usually rounded or ovoid. When two or more abscesses are contiguous, they generally remain separated by partitions of medullary substance. But communications may be established between them. In some instances several foci communicate and run together, producing a cavity of irregular form, and of anfractuous or multilocular appearance. The pus contained in cerebral abscesses is usually greenish-yellow or olive-green, thick, creamy, or liquid and ropy. It may contain a pultaceous reddish matter; it may be inodorous, but in a large number of instances it is very fetid, although it has had no communication with the air. It is generally alkaline; under the microscope it presents ordinary characters, but granules are more abundant than the proper pus cells. The matter is sometimes in immediate contact with the cerebral substance, the surface of which may be softened or hardened, injected, red, or greyish, or yellowish, or granular; or sometimes it is enclosed in a cyst. This cyst is formed of a vascular membrane of variable thickness, white, reddish, or greyish in colour. The more recent the cyst is, the thinner and softer it is. From the thirtieth to the sixtieth day and later it becomes thick and dense, and appears formed of two or three membranes. Of these, the external has a cellular appearance, the middle may be dense, resistant, fibrous, or cellulo-fibrous and vascular; the internal is soft, and like a mucous membrane. This is sometimes covered by a sort of thick concretion of pus. The cyst is sometimes easily detached from the cerebral substance, at other times it is closely united. The cysts differ from those which are formed round hæmorrhagic clots. The latter are agents of secretion, absorption, and resolution. Those which enclose abscesses do not fulfil like functions. Their membrane is rather pyogenic than absorbent. Abscesses are rarely found diminishing and on the road to cure; but although such a termination is exceedingly rare, it is not impossible. Cerebral abscesses have a tendency to open towards the points where there is the least resistance. Pus generally makes its way towards the ventricles, and these cavities are generally found more or less filled with it. Very rarely the matter appears at the surface of the brain. Cerebral abscesses evacuated by the nostrils have been reported, but the minute details fail, or pus is said to have been evacuated by the ear. But it is worthy of note that in certain cases where the cavities of the internal ear enclosed pus, the cerebral abscess itself did not communicate with the interior of the petrous bone, of which the surface was entire, and the dura mater intact. Nevertheless, it is more common to trace the connection between the contemporaneous affections of the ear, the membranes, and the brain.—*Journal de Médecine de Bordeaux. Gazette Médicale de Paris*, July 27.

On Convulsive Tremor. By Dr. William A. Hammond.—Under this term Dr. Hammond describes a paroxysmal affection, of which the following case may be taken as an example:—J. S—, a gentleman, æt. 35. Ordinarily had nothing to complain of on the score of health. Appetite was good, and all functions performed with

regularity; but two or three times during the day he would be seized with severe and unrestrainable muscular tremor, involving the head and all the muscles of the trunk and arms. At the same time there would be slight headache and vertigo, and an intense feeling of anxiety. There was no loss of consciousness, not even for an instant, nor inability to walk or to direct any muscle, and no confusion of thought. After the paroxysm had lasted fifteen or twenty minutes it gradually passed off, leaving him in a profuse perspiration. During his first interview with Dr. Hammond he was seized with a paroxysm which is thus described:—"He was seized with as much suddenness as though he were struck with an epileptic fit. His head shook violently, the muscles of his face were convulsed, his arms and hands trembled, and his gluteal muscles contracted so powerfully as to cause him to move convulsively up and down in the chair. His lower extremities were altogether free from spasm or convulsion. Upon putting my hand on his wrist, I found that every tendon was in action, and in the arm, hand, neck, and face, the vibration of the muscular fibres could be distinctly seen and felt. The thermometer applied to the axillæ marked 101° Fahr., and the æsthesiometer showed an increased sensibility of the skin of the face, neck, hands, and all the upper parts of the body examined. The respiration was quickened and the pulse was increased from 80 to 95 per minute. During the paroxysm he conversed rationally, but with some difficulty, owing to the action in the muscles of the neck, mouth, and chest." In this case the attacks seem to have been brought on by venereal excesses, the first paroxysm having supervened during coitus. He rapidly improved under treatment, which consisted of a seton in the nape of the neck (to counteract supposed cerebellar irritation), thirty grains of bromide of potassium three times a day, and the application of the constant galvanic current. Two other cases of somewhat similar character are related, in which a similar treatment proved also of use.—*New York Medical Journal*, June, 1867.

On the Influence of Narrowing of the Pulmonary Orifice on the Formation of Pulmonary Tubercles. By M. Lebert. Presented to the Academy of Sciences.—The author, in the course of his researches on mechanical influences in the etiology of tuberculous affections, has been struck by the frequency of the occurrence of pulmonary tubercle in cases of congenital narrowing, either of the cone or the orifice of the pulmonary artery. This coincidence has been observed, in isolated cases, by Favre, Travers, Gregory, Louis, and Creveld, and during the last twenty years it has been noticed in one third of the observed cases, often under circumstances in which no other etiological element of tuberculisation could be supposed to exist. The author has been able to collect twenty-four facts of this kind, a large number if the relative rarity of this affection be taken into account. The frequent development of tubercles in connection with this condition is the more striking, as nothing is more rare than to meet with pulmonary tubercles in alterations of the orifices of the left heart. Three forms of congenital narrowing of the pulmonary artery are to be distinguished—primitive

narrowness of the vessel, which is furnished only with two valves, narrowing of the pulmonary arterial cone, and narrowing of the orifice. The last two are the results of endocarditis or myocarditis, and as ordinarily the interventricular septum is deficient, this inflammation must take place before the end of the third month of intra-uterine life. The foramen ovale remains often also open, the arterial duct more rarely. Hence, the circulation through the lungs is irregular and incomplete. Dilatation of the bronchial, œsophagean and coronary arteries of the heart, also of the subclavian, furnishes only an incomplete collateral circulation, whence arises imperfect nutrition of the lungs, so that they are observed to be small and incompletely developed. This irregular, unequal, incomplete circulation in places, gives rise to pulmonary tubercle, whilst the most intense and extended pulmonary hyperæmia from alteration of the bicuspid and tricuspid orifices does not conduce to its development. The author knows neither age nor disease which presents this proportion, *i. e.* of one third affected with pulmonary tubercle, and hence he can only refer it to pulmonary stenosis. The character of the tubercular attack is protracted, progressive, and fatal. Hæmoptysis is frequent, and it is ordinarily the left lung that is first attacked, and not the right, which the author thinks is the lung which ordinarily suffers first. The anatomical characters are the same as in other forms of tuberculisation.—*Gazette Médicale de Paris,* July 27, 1867.

Rupture of the Tricuspid Valves of the Heart from Fright. By Alfred Hitchcock, M.D.—M. H—, æt. 2 years and 9 months, of a sanguine and very nervous temperament, was frightened in the middle of the night of July 16th, 1865, by an alarm suddenly sounded by the steam fire-whistle in the immediate vicinity of the house where she was sleeping. She awoke in terror, screaming and panting for breath, with suffocating paroxysms. She died seventy-eight hours after the fright. The symptoms present were vomiting, dyspnœa, lividity of skin, absence or great feebleness and irregularity of the radial pulse. The heart sounds were too feeble to be heard, or too tumultuous and irregular to be identified. There was loud moist bronchial and tracheal rattle.

Autopsy.—"The right auriculo-ventricular valves were lacerated and broken in their substance; also several of the *carneæ columnæ* and *chordæ tendineæ;* and the endocardiac serous membrane in the vicinity was ecchymosed and elevated by the infiltration of blood underneath and around the points of ruptured tissue. The right auricle and ventricle, by the destruction of the tricuspid valves, had become one continuous pouch, which contained a small quantity of coagula."—*Boston Med. and Surg. Journ. New York Med. Journal,* May, 1867.

The Condition of the Blood after Death from Snake Bite. By Professor G. B. Halford, M.D.—In a case of poisoning by the bite of the cobra-di-capella, and in animals bitten by poisonous snakes, Dr. Halford has found that the blood contains a number of nucleated cells. In a dog killed by the injection of cobra poison under the skin, these cells were of a perfectly circular form, with a diameter on the

average of $\frac{1}{1700}$ of an inch. The nucleus was nearly round, and $\frac{1}{2800}$ of an inch broad. On the second day, on applying magenta, a minute spot became visible at some part of the circumference of the cell. This was visible in all. Of these cells there were many millions; they were present in the blood of every part of the body, but in much greater quantity in the dark congested parts of the lungs. In the poison of the snake itself, Dr. Halford found, besides epithelium and much molecular matter, nucleated cells of the same size as those seen in the blood of the dog, but no spot or macula was seen in the circumference. Dr. Halford believes that when a person is mortally bitten by the cobra-di-capella, molecules of living germinal matter are thrown into the blood, and speedily grow into cells, and as rapidly multiply, so that in a few hours millions upon millions are produced at the expense, probably, of the oxygen absorbed into the blood during respiration; hence the gradual decrease and ultimate extinction of combustion, and chemical change in every other part of the body, followed by coldness, sleepiness, insensibility, slow breathing, and death. Dr. Halford suggests that cholera may be due to the presence of foreign cells in the blood, and refers to an observation by Dr. James Cowan, published in the 'Edinburgh Monthly Review,' of some peculiar bodies in the blood of a cholera patient, and also to an observation by Virchow that the white corpuscles are increased in cholera, in support of the hypothesis.—*Pamphlet, Melbourne.*

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NOTICE TO READERS.

THE Editor is particularly desirous of having all Reports of Hospitals, Asylums, Sanitary Boards, Scientific Societies, &c., forwarded to him, as also Inaugural Lectures, Dissertations for Theses. Medical and Scientific Addresses, &c.

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