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CONTENTS OF No. XCVII

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Analytical and Critical Reviews.

I.—Sanitary Legislation.

THAT session of Parliament cannot be called sterile which has produced the Act for constituting a Local Government Board, an Act resembling some other important and sweeping measures in its brevity.[1]

It may be as well to remind our readers that, for some time past, the central functions of Government, relating to public health, local governments, drainage, etc., prevention of disease, and medical poor relief, have been distributed among several departments of the State, only three of which are included in the new Act.

Briefly, then, to show what the Act covers and what it omits. (1) The Privy Council was previously empowered to direct scientific researches, to inquire into the causes and circumstances of any excessive local mortality, and to issue orders and regulations, at will, in times of pestilence; its action being directed by the distinguished medical officer who now advises the new central authority. (2) The supervision of medical poor relief was long ago committed to the chief destitution authority of the country, now transformed into the Local Government Board, which is also empowered to control local arrangements for public vaccination, previously administered under the direction of the Privy Council. (3) The Home Secretary was made the controlling authority in proceedings under the Public Health and Local Government Acts; the inspectors appointed under the latter reporting to him, and the definition of areas of local government, and the formation of local board districts and certain special drainage districts, being also submitted for his approval or correction. (4) The department which collects and compiles our national statistics of population, mortality and reproduction

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[1] 'The Local Government Board Act, 1871.'

was a mere dependency of the Home Office, as it is now of the Local Government Board. (5) Enactments relating to public baths and washhouses; (6) to public improvements; and (7) to artizans' and labourers' dwellings; each previously referred certain matters to the decision of the Home Office. All these subjects are now combined under the new Board. (8) Those large areas of local management formed under Land Drainage Acts, so important to the public health, are determined by the Inclosure Commissioners, who are practically independent of the Secretary of State, and report direct to Parliament. Moreover, by a provision in the Sewage Utilization Act of 1865, (transferred from that into Sir C. Adderley's Bill, clause 165) these Commissioners are empowered to control works for the distribution of town sewage; and they are also the central authority under a so-called "private" Act of 1867, for the same purpose. Yet this commission remains untouched by the Local Government Board, (9) Again, the Board of Trade, originally an offshoot of the Privy Council, has been authorized to control Gas and Water Companies, to issue provisional orders, and to direct the inspection of Alkali Works; neither does this central authority appear to be superseded by the Local Government Board.

Then, with regard to subordinate departments. (10) The working of the Burial Acts and the inspection of burial grounds, now under the Home Office, ought, according to the Royal Sanitary Commission, to pass to the new central authority; but of such change we see no notice in the new Act. (11) The same overburdened department still superintends the administration of sanitary enactments relating to labour; the inspectors of factories and mines reporting to the Secretary of State. It is to be observed that the Factory Acts Extension, and the Workshops' Regulation Acts of 1867, with subsequent measures in the same direction, have brought under inspection almost every description of collective industry. These measures, protecting the health and safety of millions employed in manufacture, "form," as the Royal Commissioners justly remark, "one of the most important parts of sanitary legislation," although they embrace educational and other requirements which, the Commissioners think, should constitute them "a distinct part" of such legislation.

(12) In the Memorandum prepared by some members of the Commission, the Lunacy Commissioners are specified as one of the departments "who in fact now act as Health Inspectors." (13) There are also the heads of the Army and Navy Medical Departments, deeply concerned with sanitary measures. And (14) the General Medical Council, under the Privy Council,

is authorized to determine the qualifications of those medical men who are to be employed in sanitary administration. The Privy Council has also powers under (15) the Pharmacy Acts, and is (16) the chief authority in Cattle Disease prevention.

Here, then, we have noticed some nine or ten departments and sub-departments of the State which superintend a host of measures intimately affecting the public health. All these, we hoped, would, as far as that object is concerned, have been co-ordinated more or less closely in the great work now before the nation.

But that curious incompleteness, that strange inconsistency, which so often characterize British legislation, have again prevailed, not only to cause the omission of all notice of the majority of these departments, but also to exempt several items of sanitary administration (dependencies of the Home Office and Privy Council) from the action of the new central Board. For, as we have stated, there is no intimation that the important powers which the Board of Trade either exercises singly, or shares with the Home Office, will be transferred to the Local Government Board; and the Inclosure Commissioners who have been proposed as the proper central authority for River Conservancy, may still go on forming areas of drainage, independent of and sometimes conflicting with town jurisdictions. The inspection of hospitals for different purposes of public medicine, may still remain unconsolidated; and the qualifications of medical and sanitary officers may still be left to departments unrecognized by the new Act.

This defect in principle is evidently due to the fact that no Council of Health is provided to advise and aid the Local Government Board in the extensive work before it,—a council which might include representatives from many of the ignored departments and institutions, and confer a scientific authority on the Central Board, to which at present it can lay no claim beyond the possession of a single medical officer, Mr. Simon. For the composition of the new Board is precisely the same as that of the late Poor-law Board, except that *all* Her Majesty's Secretaries of State are now members. And without questioning the administrative fitness and sanitary knowledge of the Secretary at War, the Secretaries for India and the Colonies, the Foreign Secretary, and the Lord Privy Seal, we may be allowed respectfully to doubt whether they are competent to afford anything like as efficient assistance to Mr. Stansfeld, as half a dozen gentlemen whom we could select, with general approbation, from the bodies and functionaries who have been passed over. It is of the utmost importance that the central sanitary authorities should command the confidence of the country. On this point we would refer to some observations in a former number (xciii, p. 58).

Sir Charles Adderley's Bill,[1] which stands over for consideration in the next Session, is in reality a collection of several measures which a Select Committee of the House of Commons might well separate and deal with singly as distinct enactments of one sanitary code. Several clauses of that enormous Bill have already been, in fact, so dealt with, and the Local Government Board Act is the result.

Another complete measure might be formed of those clauses which relate to local authorities and officers and areas of administration, beginning with Counties. The Rating Clauses and those which confer borrowing powers, those also which direct the course of legal procedure and which regulate appeals and other like matters, might form a third Bill. Those numerous clauses—more than 200—which treat of the powers of local authorities, under the various heads of sanitary administration, might well form so many separate measures. For example: one might relate to *dwellings and dwelling places*—under the following subdivisions; house, street, and building regulations, lodging-houses, closets, sewage, nuisances, and *water supply*. The pollution of water would probably belong to a distinct measure for *river conservancy* and land drainage, not dealt with in the new Bill. Another measure might relate to *occupations*, under the heads of offensive trades, noxious vapours, gas works and chemical processes, slaughter-houses, markets, sale of diseased and bad food. Probably, legislation against the *adulteration of food*, drinks and medicines might be distinct, as would also be the sanitary regulation of *mines, factories* and *workshops*. Another enactment would apply to means of *locomotion*, as far as the health of the public and safety of travellers are concerned—under the heads of roads and highways, ships, harbours, river and canal navigation, and railways. Lastly, a more distinctly medical enactment might include regulations respecting the registration of births, deaths, causes of death, and sickness; the supervision of hospitals, dispensaries, medical poor relief and vaccination; directions for disinfection, mortuaries, burial grounds and burials.

At all events, if the gigantic project of legislation, which the Chairman of the Royal Commission has with much skill and wonderful industry prepared for Parliamentary mangling, were separated into at least half a dozen consistent bills, covering the whole area of public-health legislation, we venture to say that a

[1] A Bill to consolidate and amend the Law relating to Public Health and Local Government in England and Wales, except the Metropolis.

more thorough and satisfactory consideration and settlement of each branch of the great subject would be secured.

To discuss at all fairly the numerous details of Sir Charles Adderley's Bill would be impossible within the limits of a review, and we think that the main object of this article will be better fulfilled by confining the following remarks to those proposals which concern the definition of areas of administration, the constitution of local authorities, and the appointment of officers.

But before supporting Mr. Hastings, and the Joint Committee so far as they agree with him, in advocating the extension of the sphere of sanitary administration to counties, we propose to call attention to the paramount importance of a general revision of the areas of local management. The error of definition, the want of coincidence, the conflict of boundary, which characterize the many systems of territorial division for sundry purposes of local administration in the provinces, have long been the butt of social and sanitary critics. The Report of the Commission itself shows very plainly the inconvenience and anomalies of the present system, in the following paragraphs:

"Unions, and sometimes even parishes, overlapping county boundaries; registration districts making incomplete correspondence with them in statistics of births and deaths; highway districts made optionally, and irrespectively of all other areas, or coinciding sometimes with one, sometimes with another; petty sessional divisions generally differing from all; cause altogether to a country whose life is self-administration, probably the maximum of embarrassment and waste of local government and the utmost loss of means and effectiveness.

"The same boundaries should as far as possible define the areas of all these kinds of provincial executive, and their officers should be, as far as possible, the same for all those purposes."—(p. 53.)

Yet with unaccountable inconsistency, neither among the final resolutions of that Report, nor in Sir C. Adderley's Bill, is there any provision for ensuring coincidence of area for these several objects in the same locality. Powers to combine, divide, dissolve, and incorporate districts are indeed to be granted to the proposed central and local authorities; but the exercise of these powers is to be optional; and the necessity for a general revision, so as to insure that coincidence of area and consolidation of functions which the commissioners recommend, is nowhere expressed. On the other hand, their almost fanatical *cultus* of a once popular idol has led them not only to oppose all interference with the small local-board districts which disfigure and perplex the country, as shown in the evidence,—and to leave them under separate authorities, called "urban" by courtesy,—but also even to propose their unlimited multiplication; and this, in opposition to the recommendation of the principal witnesses. The new Bill goes further in this direction than the Report, actually making it compulsory (Clause 32) on the central authority to form more and more of these *insulæ* of local government, without combining the various functions that are to be performed in each.

We have more to say about the project of districts and authorities put forth by the Commissioners, but must first explain the three different principles on which the areas of sanitary administration are or might be defined.

1. There is the principle sanctioned by the Commissioners, of maintaining and multiplying the small areas of management, formed, chiefly since 1858, under the Local Government and Sanitary Acts, each district being governed by a separate elective Board.

2. There is what may be called the scientific principle, recommended principally by those who regard town-drainage, sewage utilization, and river-conservancy as the paramount objects of sanitary legislation, and who would take elevations and depressions of land surface, with water courses, as physical indications for determining the extent and boundaries of administrative areas. The advocates of this method generally accept local-board and special-drainage districts as inevitable units of their plan, and would proceed without much regard to older divisions of the country, to group such units of drainage into sub-conservancy districts for the smaller or tributary streams, and these again into whole river-basins, under "intermediate authorities." The River Pollution Commission and many of our eminent engineers support some such method of district definition. It is an intelligible and defensible principle, and might be well applied to a newly-settled country; but it is clearly subversive of all the established traditions and divisions of an ancient kingdom, and, to carry it out with logical severity in England, some of our large municipal areas might have to be split up into "Special Drainage Districts," each with its separate unit of authority, while our old county governments would be overridden by independent River Boards.

3. There is the historical or statistical principle—that is, to take the counties, the boroughs, the parishes and townships of the kingdom, as they are—reconciling their old conflicts of boundary, and combining the parochial units into districts, or rather accepting, subject to revision, the present unions, the large boroughs and cities, as well-known combinations of those units, for local administration.

After carefully weighing the arguments for the adoption of each of these opposed principles, we decidedly support the third.

Many of the commoner objects of health must, *pace* Mr.

Hastings, be effected in smaller areas than those of counties. We venture to think that this able social reformer has somewhat missed the meaning of the term—unit; for by this we understand the smallest area or district in which a community may be empowered to act for itself in any matter of local administration. Now, for ordinary puposes, as scavenging, nuisance-removal, medical relief, &c., it will be, as it has been, found desirable to combine the parochial units into unions or borough-districts; although, for the higher purposes of sanitary inspection—so well described by Mr. Hastings and the Joint Committee—if not for the constitution of an intermediate authority, it is most important to establish a Board or Court for an entire county.

If it be said that the full recognition of time-honoured boundaries, in the definition of areas, does not pay sufficient regard to physical topography, we reply that it is easy for either a borough council or a county court to divide its territory with some regard to the natural features of the locality, and to appoint a committee of its body in every such division for those purposes which can be best effected within smaller limits. This is no new or untried principle of action. There is nothing utopian in it, which is more than can be said for the scientific principle. There is no empirical pretence of infinitesimal self-government in it, which is more than can be said of the principle advocated by the Royal Commission.

Now to comment briefly on the districts and authorities proposed by the Commission, and defined by Sir C. Adderley in his Bill. The list of "urban" authorities looks exceedingly like some of the schedules or introductory clauses of existing sanitary enactments. For we see, first the town council, or the commissioners in boroughs under Local Acts; then the town council in all other boroughs; then the local board in all places or districts under the Public Health and Local Government Acts; while in all other places having known boundaries, as well as in all areas to which defined boundaries shall be assigned by the central authority, the *minimum* of population for local government must be 3000; and the formation of a district in these places is made compulsory (Clause 32). An aggregation of only 3000 persons is thus held to be a criterion of competency for independent sanitary administration. What magic there is in that number to secure intelligence and public spirit enough to meet the emergencies of public health, we know not. But the new Local Government Board is not to be thus strictly limited in the manufacture of "urban" authorities; for, according to the Bill of the Commissioners, it *may* constitute a local board in any place not coming under any of the above descriptions. The

Central Board would thus have absolute discretionary power to form small areas of administration all over the country. This is apparently the design of the framers of the measure; for it is proposed to add to the above category all Special Drainage districts, constituting them Local Board districts. The country is therefore to be dealt with on a totally different principle from the great towns, in each of which the several wards or sub-districts, although rated separately and sometimes for different objects, are under a single board of management for the whole area. If the principle adopted by the Commission be sanctioned by Parliament, we may soon see, as the Joint Committee[1] predicts, the surface of the country honeycombed by more than a thousand limited and isolated areas under so-called "urban" authorities. We heartily join in the earnest protest of that Committee against such a project of local government.

These elective districts, be it observed, have no relation to parishes or other long-settled divisions of the country; and as they often consist of parts of one, two, or more parishes, they serve mainly to increase the complexity of local administration. All the residue, all places, parishes, and parts of parishes in each Union, not thus picked out, seem to be treated as a *corpus vile* for experiment under the Board of Guardians as "rural" authority. But these country parishes may be quite as well administered as the spots favoured by "urban" attentions, for no one now doubts what is asserted by the Joint Committee,

"that petty elective authorities in small separate districts are apt to obstruct rather than forward sanitary improvement, and that for the most part they render any uniform and efficient system of administration almost impossible" (p. 5).

The theory of the Royal Commissioners is that there shall be in every place one authority, and *one only*, for all public health purposes. This has been paraded in almost every channel of publication. And they have obtained immense credit for what does not prove to be the fact; for practically they sanction, if they do not propose, two authorities in each place.

Thus, we do not find that, in "urban" districts, the Local Board is to supersede the Board of Guardians in the relief of destitution. Medical relief and all cognate functions would therefore still be administered under the Board of Guardians, while the managements of roads, the removal of nuisances, and the prevention of disease would be under the direction of the Local Board or Town Council. And how, then, about vaccination? which of the rival local authorities is to act? Unless

[1] 'Report of the Joint Committee on State Medicine of the British Medical and Social Science Associations on the Report of the Royal Sanitary Commission.' London: Richards, Great Queen Street, 1871.

more complete measures are proposed, two bodies, concerned with the public health, would still exist in the same town; the one containing, the other excluding, the magistracy; the one having a complete medical staff, the other being compelled to appoint its own medico-sanitary officers. So also in rural districts, according to our interpretation of the new Bill, the management of the roads, including the appointment of a scientific surveyor, so necessary in sanitary management, may continue under a different authority, namely, a Highway Board, from that which is to administer relief, medical or other, and to execute preventive measures. Here again there would be a duplication of authority in the same districts.

Certain clauses (169, 170, 171, 172, and 196) of the new Bill foreshadow the difficulties and embarassments which will attend, as past experience has shown, the intrusion of small urban districts upon highway districts, to the disturbance of any effective system of road management in large areas, thus reproducing the very abuse which Sir George Grey exposed in 1863, and which led to the legislative disallowance of local-board districts with a population of less than 3000—even smaller places having then actually taken advantage of the Local Government Act to procure exemption from the operation of the Highway Act.

The oddity of the whole scheme is that the two authorities which are to coexist in urban districts, would exercise different groups of functions from those committed to the two authorities in rural districts.

"It cannot be doubted," adds the Committee, "that this complication of authority and duty would lead to much administrative confusion, and hinder, as it has already hindered, the progress of improvement in local government."

On the other hand, should the almost unanimous suggestion of the principal examinees, referred to in Resolution 12 of the Report of the Commission, be adopted, and should the Highway Boards and sanitary authorities in rural districts be consolidated by Parliament, which is not proposed in the Bill, an unanswerable reason would be supplied for a similar consolidation in urban districts. Again to quote the Joint Committee:—

"The great majority of town districts in England are included in registration districts of a somewhat larger area. It is extremely important that the two or more governing bodies within that larger area should be united, at the very least intermediately, by representation, for the purposes of the proposed Act."

Should the principle of an extension of area be admitted, we do not see it expedient, if it were possible, to stop short of our largest known sphere of local administration, *i.e.* the County. We say, unhesitatingly, that our county management, by the Great Unpaid, is distinguished by its disinterestedness, efficiency, ability, and freedom from party spirit; and we should be quite willing to leave the execution of certain sanitary measures in the hand of the Justices, who are real representatives of the country; but no one who has watched the progress of this question in Parliament doubts that ere long county authorities, of a more popular constitution than the present, may be established; and yet that they should not be entrusted with any functions relating to the Public Health, would indeed be a solecism in legislation, of which Parliament would hardly be guilty. The evidence taken by the Royal Commission, although not unanimously in favour of county sanitary administration, contains many important facts and opinions in support of the proposal to make use of any authorities which may be constituted over extensive areas, and which would consist of members unlikely to be swayed by petty interests and sordid views, for some higher and wider purposes, and as courts of appeal, to consider and report on applications from localities and if necessary to execute works.

We are reminded that the magistrates, in Quarter Sessions, have already to administer and enforce the law, to control institutions (as hospitals for the insane, reformatories, and prisons) and appoint their officers, also to perform certain duties connected with the health and safety of the people, as the control of coroners' inquests, the appointment of analysts, surveyors, &c. It seems, therefore, not only reasonable but practicable, either that the powers of the magistracy should be extended so as to comprehend the care of the public health, or that boards containing the magistrates and ratepayers in due proportion should be forthwith constituted for the direction and control of sanitary administration.

Mr. Goschen's dropped Bill was, in some respects, an improvement upon the Report of the Royal Commission, for it contained provisions for the establishment of County Boards. to which it is clear many such duties as we suggest might be committed. By the 26th clause of that Bill, the Local Government Board might delegate to the County Board the powers of the smaller local authorities, when these fail in their duties, until the default is remedied. It is unfortunate for this question that Mr. Goschen did not remain at the head of the Poor Law Board. We trust that the precedent which he established may be followed by his able successor, and that Parliament will pass no general sanitary enactment which does not contain a provision of this kind.

We have before noticed (No. xcii, pp. 299, 300) several important sanitary reforms which require a wider sphere of administration than that of an urban or union district, such as the provision of sites for labourers' dwellings, to be erected under a revised code of building regulations, and the water-supply of towns and villages, when it has to be procured from distant sources. On the latter point we observe that Lord Robert Montagu, in a separate report, adduces forcible reasons for the establishment of Boards in what he calls "watershed areas," meaning river basins; and these boards he would make intermediate authorities between the strictly local bodies and the central department. Agreeing with his Lordship as to the necessity for some such authorities, we believe that the first and safest step towards the formation of river-basin boards would be the constitution of county authorities, by means of which (singly or in combination, according to the extent of area within the watershed line) the river boards might be most judiciously appointed.

There are other purposes for which county authorities would, generally, be far more suitable than local boards; for instance, the control of the smoke and noxious vapour nuisances of mining and manufacturing districts; the allotment of sites for animal-refuse depôts and public *abattoirs*, in connection with measures against cattle diseases; the provision of recreation grounds, so necessary for the health and welfare of crowded populations; the appropriation of land for sewage utilization, where local bodies cannot procure it; the allocation of sites for hospitals to receive fever and other contagious diseases, always most safely treated in sparsely populated districts; and in some localities, "provision for the burial of the dead without injury to the living."

This last object is rightly specified by the Royal Commission as one of the requisites for civilized society; but local boards are not always able to make the provision on practicable terms; and neither the Burial Acts nor Sir C. Adderley's bill give compulsory powers of land-purchase for this object. A county authority for all such purposes is far preferable to a central department, as being more cognizant of the wants and circumstances of districts within the county, and more interested in the speedy and safe settlement of territorial questions. On the other hand, it "would not be subject to the self-interestedness and isolation of mere town legislation."[1]

But there is no plea for wide administrative areas more forcible than the last which we shall offer, viz., that they would provide greater opportunities and a superior machinery for the selection of scientific officers, whether medical or engineering,

[1] See Lord Robert Montagu's paper on 'Conservancy Boards.' p. 347.

of high and special qualifications, than would be at the command of any local-board district or union.

On the important question of the appointment of the higher sanitary officers, we would advise our readers to compare the report and memorandum of the Royal Commission with the report of the Joint Committee and the addresses of Mr. Hastings and Mr. Godwin at Leeds.[1]

Doubtless, the former "express very ably the principal needs and resources of existing local government in this respect;" but the latter state as forcibly their objections to a scheme which, if adopted, "would soon show," in the words of a leading journal, "that efficiency had been sacrificed to a spurious economy."[2]

The report of the Joint Committee, well supported as it has been by Mr. Hastings and Mr. Godwin, will amply repay a thoughtful perusal, though it may be too independent in its tone to obtain the support of a partisan press. It is well that the opinions of two eminent provincial physicians, and of one still more distinguished who has been taken from us, are given in their own words in the appendix to that report. When Dr. Strange, the earliest of living English writers on State Medicine; Dr. William Budd, the keenest of medical logicians in the contagion controversy; and the late lamented Dr. Symonds, than whom no higher authority could be quoted as to the ethical and official relations of the medical profession to the public, concur in taking the same view of the position, duties, and qualifications of the health officer, and when the Joint Committee itself—no mean embodiment of sanitary science and experience—endorses these opinions, and shows at length how they may be practically applied to existing circumstances, we feel on solid ground in supporting them in opposition to the Royal Commission.

Considering that on the Commission there were five gentlemen of the highest mark in our profession, it is a matter of equal astonishment and regret to find that their ideal of a country officer of health reaches no higher than the ordinary district medical officer of a Poor Law Union. It seems as if all that had been done and said by the leaders of this movement, among whom we may name Robert Baker, Edwin Chadwick,

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[1] 'The Recommendations of the Royal Sanitary Commission.' Extract from an address at the Social Science Congress in Leeds, by G. W. Hastings, LL.D., President of the Council. 1871.

'An Address on Public Health and its Modern Requirements,' by G. Godwin, Esq., F.R.S., President of the Health Department of the Social Science Congress in Leeds. 1871.

[2] 'Pall Mall Gazette,' June, 1871. This clever though short notice shows a thorough knowledge of the question, and deserves to be read by all who are interested in it.

Farr, Lankester, Liddle, Ranald Martin, Michael, Rumsey, Simon, Stewart, and Alfred Taylor, had been said and done in vain; as if all the costly work of the State Medicine Committee of the General Medical Council were to be fruitless; if, after all, a corps of health officers is to be formed on a principle which ignores the conditions long ago laid down for securing special and complete acquirements, thorough training, efficiency of action, independence of opinion, and devotion to public duty. If the Commission had referred to the Minute of the last General Board of Health, and had weighed as it deserved that category of the duties, qualifications, and conditions of the office advised by Mr. Simon sixteen years ago, and quoted with great effect by Mr. Hastings at Leeds, they could hardly have put forth so imperfect and fallacious a scheme.

Dr. Stewart, in his admirable address at Plymouth,[1] said in criticism of that scheme:

"We feel also that we have a right to—and we do—complain that, while they were not only empowered by the terms of their appointment, but expected to investigate with special care, and to report upon, the number of officers and the local expenditure required under the present disjointed, chaotic, and fragmentary plan of operations, as compared with the coherent, orderly, and comprehensive system, suggested in our Memorial to the Government, the Commissioners have adopted the existing chaos as the *summum bonum*, have quietly ignored the existence of any other plan, and have furnished us with no data for determining the cost, either absolute or comparative, of that propounded by themselves."

Except on grounds of a fictitious economy, indeed, we know not why the commission has pronounced so decidedly against the compulsory separation of private practice from public employment in the higher sanitary offices. The weight of evidence, whether from places in England where health-officers have been appointed, or from Germany, where the poorly remunerated *Kreis Physicus* has for many years been a state institution, proves that the union of the two occupations and the necessity of making an income out of curative practice to supply the fiscal deficiency left by a wholly inadequate salary for preventive duties, have failed to confer that influence upon the officer and to secure those qualifications for it which are necessary to its full efficiency.

An unfair attempt has been made to show that the Joint Committee and its supporters have underrated the qualifications of the Union medical officers, and treated them as unfit to be entrusted with any sanitary duties. This is simply a misrepre-

[1] An Address by A. P. Stewart, M.D., F.R.C.P., President of the Section of Public Medicine at the British Medical Congress in Plymouth, August, 1871.

sentation. The fact is that both parties in this controversy propose to commit the same sanitary duties to the poor-law medical staff, and for the same reason, namely, the obvious advantage of employing those engaged in medical attendance on the poor to report on the causes of their sickness and on their domestic conditions of health. None have expressed more clearly than the Joint Committee the ability of the poor-law staff to render this public service, and the great importance of securing their co-operation on equitable terms. If that committee has judiciously avoided the gross and transparent flattery which the authors of the 'Memorandum' have heaped on the medical officers of unions (Report, vol. ii, pp. 352, 353), they cannot be truly described as "attacking" those officers. Yet Mr. Hastings has been accused of this by a recent writer in the 'Times,' who signs himself "A Member of the Royal Sanitary Commission," although the former had by anticipation refuted so unjust an accusation, when he said at Leeds:

"No one had a higher sense (than myself) of the services and merits of a body of men signally underpaid for their work, denied the social recognition of enrolling in the civil service of the Crown, and struggling manfully, in the great majority of cases, though beset with difficulties, to do their duty to the poor. But I cannot believe that these meritorious officials possess the qualification so necessary for (the higher) sanitary work;" adding, after a reference to Dr. Symonds and to the Minute of the General Board of Health in 1855, "But I would not be supposed to argue that they may not be usefully employed in our sanitary organization. I believe, on the contrary, that they could render valuable aid as subordinates, with some increase to their present meagre salaries, under a highly trained superior, such as a county administration could supply."

In the sonorous platitudes which that Royal Commissioner employs when praising the recent statutory union of the "medical department of the Privy Council, the Home Office, and the Poor-law Board, with all their staff, central and local, inspectors, lawyers, engineers, and medical men," in one body, for the purpose of health legislation, he by no means succeeds in justifying the proposed use of an inadequate local machinery. If this be statesmanship, save us from such statesmen.

The choice, as regards sanitary inspection and reports, does not lie between poor-law medical officers and thoroughly trained experts acting in extensive districts; for those who object to the scheme of the Commission would employ both classes in sanitary work. The real question is, whether a superior class of health officers might not supersede the necessity for a vast increase in the staff of inspectors attached to the central office—a prospect which is carefully veiled by the Commission.

No one who is practically acquainted with the working of present arrangements would venture to assert that the generality of poor-law medical officers, compelled, as most of them are, to enter into practice with a mere legal qualification at twenty-one years of age, and absorbed in the struggle of life, could possibly discharge the arduous functions and responsibilites described in a former article of this 'Review' (No. xciii, pp. 66—69), or that any sanitary reform would be real, complete, and effective which left those functions and responsibilities undischarged. To mention merely two kinds of duty which ought to be performed by superior health officers; (1) the compilation and abstract of the vital and sanitary statistics of an extensive area from the returns made by medical charities, district officers, and registrars, and their local utilization;[1] (2) the superintendence of large preventive measures, especially during epidemics, and the visitation of asylums and sanative institutions of various kinds.

The altèrnative, therefore, would be to commit these and other duties of local supervision to a vast organization of inspectors at the beck of the central office. Let our readers understand the real issue of this controversy, well weighing the remarks made by Mr. Hastings at Leeds, and by the Joint Committee (Report, § 41) on the hypercentralization of the Commissioners' project, and we have little doubt about their decision.

Another side blow has been aimed at a normal organization of Health Officers by a London journalist, who reproduces an old and exploded fallacy in order to evade the crushing objections to a combination of public employment with private practice for the chief sanitary officer. This writer proposes to convert that officer into a poor-law medical officer, which could only be done by enlarging the district for medical relief, so that, with his sanitary functions, it might occupy the whole time of the compound officer and entitle him to a salary that would justify the prohibition of professional attendance on paying patients.

Had that proposal not been sufficiently demolished by a reply published in a contemporary journal,[2] we might have felt it necessary to do more than merely refer to it. As it is, we need only remark that an abrupt separation of curative duties among the destitute and dependent classes from that among the middle and upper classes would be a serious disadvantage to the sick in all ranks of society and disastrous to medical science. The evils would, perhaps, be less apparent in London, and in a few

[1] See an article on the "National Registration of Sickness" in this Review, No. xciv (April, 1871).

[2] 'British Medical Journal,' Nov. 11, 1871.

of our largest cities; but only a metropolitan writer, ill-informed about medical practice in the provinces, would have proposed such a measure for the whole country. Moreover, the project would be enormously more expensive than either that proposed by the Royal Commission or that advocated by the Joint Committee.

Thoroughly sympathising with the efforts of the Association of Poor Law Medical Officers, we hail the prospect of an amended system of medical relief, and the proposed introduction of Dispensaries on the Irish model, with a fair augmentation of the salaries of the surgeons,—provided that this great reform be founded on an improved county machinery for poor-law and sanitary administration, and be accompanied by such improvements in the qualification of the officers as we urged in the article before referred to (No. xciii). The measure which Mr. Corrance is to bring forward may possibly complicate the district question, unless the Local Government Board should determine on a general revision of districts, so as to insure coincidence of area for all purposes of local administration. It is highly satisfactory to be assured that the whole subject is under the consideration of Mr. Stansfeld, who is uncommitted to any project, and who ought to be heartily encouraged and aided by the whole sanitary party. If a better measure than that of the Royal Commission should be the result, no little honour will redound to the Joint Committee.

The first safe step, we repeat, is to constitute County Authorities, and to empower them and their officers to inquire into the extent and boundaries of districts in each county, preparatory to their readjustment. This could only be done satisfactorily by persons of special qualification and thoroughly acquainted with the localities; and these proceedings would, of course, be subject to general instructions from the Local Government Board, so as to insure uniformity of principle. It is obvious that the districts for dispensaries, medical relief, and sanitary visitation should correspond, singly or collectively, with the Registration sub-districts. The number of medical relief districts in the provinces (that is, exclusive of the metropolis) is said to be not less than 3100,—whilst there are 2062 Registration sub-districts therein. As the number of district medical officers may require to be increased rather than diminished, and as there seems to be no necessity for a multiplication of the Registration sub-districts, it follows that a large proportion of the latter would embrace two medical relief districts, or at least be attended by two medical officers attached to one dispensary.

We cannot conclude without noticing a very important body of medical officers, engaged in the industrial department of preventive medicine, who have been unaccountably passed over not only by the Royal Commission but also by others who have taken part in this movement.

The certifying Surgeons of Factories are performing duties most essential to the safety and physical well-being of children employed in manufacture. They should, therefore, form a part of any system of sanitary organization; for preventive and examining duties in factories and workshops are distinct in their nature from the ordinary duties of a health officer. No one who has read the annual publications of the Association of Certifying Surgeons can doubt that some of the gravest sanitary difficulties come under their observation, and that so far from its being advisable to limit or curtail their functions, it is important to develop and extend them.

We would direct special attention to their last Report,[1] and to the remarks of Mr. Inspector Baker on this subject. He shows clearly that these surgeons might constitute, under amended regulations, a yet more effective machinery for improving the health of the operatives and the sanitary condition of workplaces.

The President of the Association, Dr. Arlidge, concluded his interesting address with the following weighty observations on the then probable establishment "of a central Health Board, and the consolidation of the numerous sanitary acts in existence, among which the Factory Acts hold a place."

"This being so, the existing staff of certifying surgeons may find themselves either disestablished and disendowed, or otherwise transformed into District Sanitary Officers, with a multitude of duties over and above those they have now to perform; and I feel pretty confident that instead of the Factory Department being developed according to Mr. Redgrave's ideas, so as to ingulf sanitary legislation in general, it will have itself to be merged in a large scheme of sanitary organization."

Our view of the proper status of Factory Surgeons in such a scheme is, that they should become, like the poor-law medical officers and the sanitary officers of small towns, *Deputy Officers of Health*. As the district and town officers would have to do with *dwelling* places, so would the latter with *work* places. The visitation of the one would be domiciliary, of the other industrial. Aided and strengthened by a superior officer (appointed by the authorities of a county or first-class borough and made independent of private practice), both classes of deputy officers would become of far greater utility and efficiency

[1] "Report of the Association of Certifying Medical Officers of Great Britain and Ireland." Newcastle-under-Lyme: Hickson, 1870.

than is possible in their present unprotected and disjointed state of organization.

Finally, no great measure of legislation on any matter affecting the higher interests of the people can be expected to become law, or, if enacted, to be carried into operation in this country, without a vast amount of discussion,—rarely without sharp, if not acrimonious, controversy. Questions of Public Health form no exception to the rule. Our national discordance of opinion on this subject may be due to the facts that it has never until lately been thought worth the undivided attention of a distinct department of the State, that the elements of physiology have formed no part of a liberal education, and that money-getting, protection of property, party politics and polemics, have absorbed the thoughts and energies of the people, so that when called upon to enact and administer good health laws, they and the governments whom they support are "at sea." False maxims, private interests and established wrongs combine to obstruct and pervert sound principle and true progress.

Moreover, not only has the care of the national health, in its several sections, been unadvisedly distributed among various departments, but even in the present advance towards unity there is no little danger in its combination with Local Government and Poor-Law Administration. Still we may accept that combination in hope and trust, demanding only that the physical condition of the people and the improvement of the race shall be the primary object of the newly constituted authorities, central and local; and that however slow and cautious may be the steps taken to secure that object, each shall be consistent with the other, so that in the end we may witness the completion of a system of Sanitary Administration worthy of so great a nation.

II.—Spence's Lectures on Surgery.[1]

These two parts complete Mr. Spence's work upon Surgery. The general principles of surgery, and the diseases and injuries of special structures, were fully considered in the earlier portions of the work, which received notice in this review shortly after publication.

The portions now placed before the profession form not only the largest, but also the most important and the most interesting section of the work, inasmuch as they deal with that branch of

[1] *Lectures on Surgery* by James Spence, F.R.S.E., Surgeon to the Queen in Scotland, Professor of Surgery in the University of Edinburgh, &c., &c., Parts III and IV. Edinburgh, 1871.

surgery which will always have a special attraction to surgeons, namely, the practical relief which can be given to patients by operative means. This relief is in many cases so complete and immediate that it both gratifies our sense of humanity, and makes us feel proud of the scientific position which the healing art has attained. These considerations will induce us to presently lay before our readers, a few short extracts to enable them to judge of the ability of Mr. Spence, as a trustworthy and most experienced guide through the difficult and intricate paths of modern surgery. We will, however, for a moment direct attention to the general scope of the great work now completed.

If we peruse the preface (which is to be found at the end of the 4th part), we observe with pleasure that our author justifies the omission of ophthalmic surgery altogether, a proceeding in which we entirely concur. The surgery of the eye cannot be embraced within the limits of a chapter; it demands a separate treatise. It has made such enormous progress since the invention of the ophthalmoscope, that it is hardly possible to do justice to it in a work on general surgery. This we may see by looking at any of the modern treatises which profess to include it; for example, the new edition of Sir William Fergusson's 'System of Practical Surgery' would have been better if the chapter on the eye had been altogether omitted. And this leads us to speak of another point upon which Mr. Spence has, as it appears to us, exercised a wise discretion. He tells us that, while he has omitted no subject which ought properly to find a place in a course of lectures upon systematic surgery, he has dwelt at greatest length upon certain subjects for the study of which he has had unusual advantages. At the present day so extended is the field of surgery, and so diligently have its different parts been cultivated, that it is impossible for any one man to be equally conversant with all its departments. It is wise, therefore, that each should claim to speak with authority only on those points with which he is fully acquainted; and it is on this principle that Mr. Spence has spoken in most detail upon such subjects as the excision of joints, amputations, injuries of the air passages, hernia, and the diseases of the urinary organs.

We will now lay before our readers two or three extracts relative to these subjects upon which Mr. Spence can speak with the greatest authority, and in the first place we will quote his opinions as to excisions of joints, a class of operations whose history is specially connected with the Edinburgh school of surgery. On the subject of excision of the knee-joint and its advantages as compared with amputation, Mr. Spence says:—

"Excision of the knee-joint, so far as the operation has yet been tested, can scarcely contrast favorably with amputation at the lower part of the thigh for disease of the knee-joint. I am aware that favorable comparisons have been made by contrasting cases of excision of the knee-joint with amputations of the thigh generally; but the comparison, to be a fair one, must be between cases of amputation performed for diseased conditions similar to those for which the excision is had recourse to. My experience of amputation has shown me that not only the extent of the limb removed, but also the nature of the disease for which it is removed, make a great difference as to the success or fatality of the operation, and that amputation in cases of simple strumous disease of the knee-joint affords the greatest amount of success; and this is the class of cases for which we perform excisions. With all my feeling in favour of excision of the knee-joint as a substitute for amputation of the thigh, I cannot help believing that in certain conditions it is more dangerous than amputation, and that the character of the operation is likely to be perilled by its being indiscriminately resorted to. In reference to an absolute advantage of excision of the knee over amputation as regards the results in successful cases, no one can doubt that, and I think the question now to be considered in regard to the comparative mortality of these operations is to endeavour to discriminate between the cases in which the one or the other of these operations presents the least risk of life. Here, as in amputations, it is difficult or impossible to gain much from mere general statistics. What we want to know in considering such a point is, all the circumstances of the cases operated on, which may have influenced the result, and hence the surgeon will learn more from, and naturally be more impressed with, the result of his own cases, or of cases which he has opportunities of closely observing. My own experience of excision of the knee-joint is comparatively limited, and therefore in stating to you the results, any deductions I may draw from my cases are to be regarded as merely suggestive, not positive."—p. 710.

In continuation of his observations, Mr. Spence goes into a lengthy and careful analysis of eighteen cases in which he has performed excisison of the knee-joint—the sort of analysis which is urgently needed if we are to be able, in any given case, to strike a nice balance between the *pros* and *cons* of excision and amputation. Indeed, one of the most valuable features about the present work is the elaborate analysis that he gives of his own cases. This is still more apparent when he comes to speak of amputations. As a civil surgeon he has performed an unusually large number of such operations:—403 in all

This number is sufficient to form a fair basis for drawing conclusions; and our author analyses them carefully, giving the exact nature of the disease or injury in each case, and the precise result, whether favorable or unfavorable. It is obvious that statistics so prepared are of very great value; indeed, they are the only statistics that will in any degree satisfy the demand that there is at the present day for accurate and discriminating information.

Among other subjects which Mr. Spence has had singular advantages for studying are croupous and diphtheritic affections of the throat. These are always a difficult class of cases, for the practitioner is usually called to them suddenly; he finds his patient in urgent distress, something must be done, and whether that something ought to take the form of an operation is often a very perplexing question.

The opinion, therefore, of the Edinburgh Professor upon that *questio vexata,* the value of tracheotomy in croup, will be perused by our readers with special interest. He thus writes:—

"The presence of any extensive exudation of false membrane must always be a formidable objection to tracheotomy. If partially loose it may be so placed as to act as a valvular obstruction at the lower aperture of the tracheotomy tube; and if the membrane exist in the complete tubular form, as in that state it is often but loosely attached, it may collapse, on the trachea being opened, and cause immediate suffocation. If I am correct in holding the opinion that bronchitis or croupous exudation, when present to any extent, form objections to tracheotomy, it must be evident that very few of the cases which we see in young children admit of its performance with reasonable chance of success, except at a much earlier period of the disease than is generally thought proper to consider of its propriety in this country. In estimating the success of the operation for croup in France by M. Trousseau and other surgeons, we must keep in mind, not merely the comparatively great success of what has proved by no means so successful an operation in this country; but, taking into account the early stage in which it has been performed there, we must ask ourselves whether many of the cases might not have recovered under active treatment without such a hazardous operation. In very young children, under three years of age, besides other dangers incident to the operation at that period, the bronchitis which follows the operation must render the chance of success very small indeed, and the unfortunate results of such cases often prevent the surgeon being permitted to perform tracheotomy in cases proper for it."—p. 1097.

Again, in speaking of strangulated hernia, Mr. Spence pursues the same course as he has followed with regard to other subjects, such as excision and amputation, and presents an analysis of his own operation cases.

"Several years since (he writes) I looked over the results of 127 cases of hernia in which I had operated, and in these there were twenty-six deaths: in seventeen out of the fatal cases the gut was distinctly gangrenous, and therefore, though the operation was the only chance of saving the patient's life, the state of the intestine was such that a favorable result could hardly be expected. In seven cases peritonitis had commenced before the operation; and of these four were cases of congenital hernia, where peritonitis occurs rapidly. In one case pyæmia proved fatal on the eighth day, but this case was a very complicated one. In one recent case a fatal result occurred: here the operation was extra-peritoneal, and the hernia was very large and bulky, and had come down rather rapidly while the patient was at work. The man made violent efforts at the time to reduce the hernia, while afterwards attempts were also made, but without success. In this case the hernia had only been down for twelve hours. When I operated I divided the integument and the fascia down to the deep ring, and then, by dividing the textures external to the neck of the sac, I was able to reduce the hernia with ease. After a time the patient began to pass bloody stools, showing that hæmorrhage from the interior of the gut had taken place, evidently in consequence of the efforts the man had made to reduce the hernia at first. I believe that if cold had been applied to the tumour, and the taxis properly employed at first, the hernia might have been reduced without any operation. The real cause of the fatal result was evidently the mischief produced by the violent and ill-directed efforts made by the patient to reduce the hernia when it came down."—p. 1162.

In speaking of the important subject of urethral stricture we are glad to see that Mr. Spence dismisses the "spasmodic stricture" from the list of strictures proper. He says that the spasmodic or engorged constriction ought scarcely to be called a stricture. In this we quite agree with him. If the urethra is merely congested, as it frequently is when acute gonorrhœa is present, the condition ought not to be termed "stricture;" while if there is a real spasm of the muscular fibres surrounding the canal, we believe it always points to a pre-existent organic stricture; and in such a case the organic stricture and not the spasm is the matter to which the surgeon ought to address himself.

While we are on the subject of stricture it will be interesting if we lay before our readers the author's estimate of "Holt's operation." A few years ago the method which Mr. Holt introduced, or revived, was in the ascendant. Now, however, there seems a tendency to decry it. The opinion of Mr. Spence will, moreover, be read with more interest, inasmuch as he tells us that he was at first strongly opposed to the operation, and, when he gave it a trial, he did so under some amount of prejudice.

"I was at first prejudiced against this plan, owing to what I had seen of the effects produced by the forcible dilatation with the conical sound. In cases where that instrument had been used I had noticed that an irritable and resilient state of the contraction was the almost invariable result. From some opportunities I had of examining strictures dilated by this method on the dead body, I found the mucous membrane at and on either side of the stricture fissured, and I considered this condition so similar to that of fissure of the rectum, that the intense irritability seemed to me referable to this fissured condition of the mucous membrane. Hence the immediate dilatation, by Mr. Holt's method, seemed to me as likely to lead to the same disagreeable results; but, after trying it in some cases, and having had an opportunity of examining a stricture which had been treated by his method, I found there was a difference between its action and that of the conical bougie. I saw that by his plan the stricture was fairly ruptured, not only through the mucous membrane, but through the condensed submucous tissue forming the contraction. Now we know all that is required to cure the excessive irritability of fissure of the anus is division of the fissure fairly through its hard base; and whilst we usually effect that with the knife, still it can be effected by the coarser method of rupture with the fingers; so that Holt's method, by fairly rupturing the hardened basement texture of the contraction, prevents, or even cures, the irritable condition of the stricture, and thus acts very differently from the partial fissuring of the mucous membrane caused by the conical bougie. Forthe last nine or ten years I have practised Holt's method, and with great success, so that I feel no hesitation in recommending it as at once efficacious and safe—that is, as free from danger as any operation on diseased urinary organs can be." —p. 1319.

It is curious and instructive to study the general remarks which Mr. Spence makes with regard to the arrest of hæmorrhage in amputations and the mode of dressing stumps, remarks which we presume may be taken as applicable to a much wider series of cases than those with regard to which they are made. The hæmorrhage, he tells us, from large arteries is to be arrested by *the ligature or by acupressure*. Scotland is still loyal to the suggestion of Sir James Simpson, and carries out his method. But on this side of the Tweed it is almost, if not quite abandoned. We believe we are right in saying that no metropolitan surgeon uses acupressure except in certain rare cases, and in certain situations to which it is specially applicable. It has yielded the palm to *torsion*. But this latter method is scarcely alluded to by our author, indeed it finds no place at all in his practical instructions. There is a strong feeling among English practitioners that acupressure cannot be relied on—that the way in which it occludes the vessel is not trustworthy—and that there is more danger of secondary hæmorrhage after its employment than when we use either the ligature or torsion.

Again, it is curious to observe that in his directions for dressing stumps and tying large arteries in their continuity, nothing is said about the antiseptic treatment of wounds or the carbolised ligatures of animal membrane, about which we hear so much from another distinguished Edinburgh professor. This silence is ominous. Indeed, Mr. Spence's method of dressing stumps consists in laying them on a pillow and leaving them entirely alone, a method which is the very reverse of Mr. Lister's; so that the students of the Edinburgh University have the advantage of seeing two very different methods of dressing carried out in the wards of the infirmary.

Throughout Mr. Spence's work are scattered clinical cases, the details of which are given *in extenso*. At the end of each chapter a few such are to be found. They are for the most part very illustrative cases, and, at the same time, very curious instances of surgical disease. Indeed, such could hardly fail to be the case, for they are selected from a hospital practice of thirty-five years' duration. We would gladly lay one or two before our readers, but they are reported at such length that we must forbear, and content ourselves with the few extracts which we have given as illustrative of his teaching.

We cannot congratulate Mr. Spence upon the form in which he has brought out his Lectures. A work which comes out in parts, and which reserves the title-page, preface, and table of contents to the end of the last instalment is unsatisfactory to the reader, and is apt to place the author at a disadvantage. It is putting the cart before the horse, and that does not give fair play to either. It would have been better if Mr. Spence had waited till his whole series of Lectures was complete, and then have brought them out at one time. However, the mode of publication is a thing which may fairly be left to the author's convenience. If he brings out his work in a way that does not commend itself to the public, he will probably be reminded of the fact by a slow and tardy sale. But there is another point which is a fair subject for criticism, and that is the character of the illustrations which are scattered through the book. On this point we have before expressed an opinion in noticing the two previous volumes, and what we there said we can only reiterate now, and, if possible, with greater force and emphasis. Mr. Spence is undoubtedly a very able and experienced surgeon, but he is evidently not an artist. He has not the gifts which Sir Charles Bell possessed, and which enabled that great surgeon to use the pencil with fully as much skill as the scalpel. But Mr. Spence has not even an artistic eye; he does not seem to know good illustrations from bad ones. It is evident that he has gone to great expense in getting up the chromo-lithographs, engravings and woodcuts with which his pages abound, but his outlay has been productive of a very poor return. The chromo-lithographs in these present volumes are as coarse and bad as those which preceded them, and coarse chromo-lithographs are very bad indeed; while of the woodcuts we may safely say that not a few convey no clear idea of what they are intended to represent. The illustrations in works of this kind should speak for themselves. They should be so sharp and clear, and the *point* of the drawing ought to be brought into such prominence, that a mere tyro can tell what they are intended to show. But if Mr. Spence's illustrations are tried by this rule, a great many will be found to fall grievously short. When his Lectures reach a second edition, there is nothing in which he could so much improve the work as by expunging many of the present illustrations and introducing new ones. And we sincerely hope that it will not be long before he has an opportunity of carrying out this suggestion. His work is quite worthy to stand on our shelves in company with "Syme," and "Miller," and "Pirrie," and other modern ornaments of Scottish surgery; it is quite entitled to hold a place with the "Systems," which have lately issued from the hands of English surgeons, and we hope that that large number of students and practitioners who look to Edinburgh as their Alma Mater, will not be slow to show their appreciation of it.

III.—Medical Reports of the West Riding Asylum.[1]

We believe the volume of Asylum Reports under review to be the first of its kind. It inaugurates a new era in psychological literature, and the originators may be congratulated on their enterprise.

Several attacks have recently been made upon the medical staff of the public lunatic asylums of this kingdom. Superintendents have been charged with not contributing their due share to medical knowledge; with failure to advance materially the British school of pathology; and with so devoting their best powers to executive and organisation, as largely to sink the physician in the manager. Some of these attacks were in part levelled against the assistant physicians. We believe that a correct appreciation of the position and responsibilities of asylum officers will show that these charges are in the main unfounded; that, though true with regard to a few, they are not applicable to the majority. *Much has* been accomplished by our asylum medical officers. They have issued works on

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[1] *The West Riding Lunatic Asylum Medical Reports.* Edited by J. Crichton Browne, M.D., F.R.S.E. Vol. i, 1871.

psychiatry; their contributions to the medical serials, including their own 'Journal of Mental Science,' are numerous; their association meetings educe much scientific labour; their experimental investigations into new views, and trials of new remedies, are often elaborate; while they have enriched the therapeutic art, and widened the application of its resources.

That greater results have not been achieved by them may be partly from remissness, but is largely to be accounted for by urgent reasons. To effect the curative and mitigatory objects of an asylum, its whole arrangements must be under medical supervision, and form part of a comprehensive treatment. Without a general directing control, the best efforts of the asylum physician may be frustrated. The moral element in the treatment, and especially the importance of personal influence on the insane, must be kept in mind. Now the general scheme of treatment, the regulating moral influence and tact required, make large demands on the powers of the "silent worker" for unobtrusive ministry of love, and philanthropic but exhausting labours. The *average* lunatic asylum contains several hundred patients, whose treatment must be undertaken by the superintendent and assistant medical officer. Many of these patients live on year after year, incurable, changing but slightly, affording no basis for special continued medical observation; yet often perhaps troublesome or turbulent, and then necessarily absorbing much attention. If the first thoughts of the medical officers are for the recovery and comfort of their charges, the time left available for original and minute investigations must be limited.

A systematic enunciation of the experience of asylum physicians is not stimulated by conjunction of the lunatic hospital with medical schools, or by its wards becoming the theatre of flourishing classes. No "registrars" or "hospital dressers" exist here to record minute observations, and furnish the crude material for lectures and publications of "the staff." For the above reasons it would be unjust to compare the contributions to medical science which issue from asylums with those which issue from general hospitals; especially from the Metropolitan hospitals, with their large staffs, their attached medical schools, their rich resources and variety of cases.

But there are further considerations. From a purely medical point of view, the difficulties which the physician to a lunatic hospital has to encounter are of no common order. They may be exemplified in the obstacles to diagnosis and treatment of the bodily and mental disorders of the insane;—obstacles which are at once appreciated by those who pass from private to asylum practice. The *diagnosis* of the ordinary (bodily) diseases of the insane is often beset with peculiar difficulties. Mental impairment may lessen sensibility to pathological processes, and the lethargy of the excito-motor powers may curtail the usual perturbation of disease, and obscure its phenomena. Unwillingness to be examined, anomalous complaints, maniacal incoherence, or fatuous utterance, too often are sad substitutes for the desirable correct communication of subjective feelings. It is only necessary to recall how frequently disease in the insane is latent; how phthisis may make ruin of the lungs, while slight emaciation, slight failure in strength, and listlessness, are the only perceptible symptoms; how pneumonia may advance far, uncomplained of, and presenting nothing beyond the physical signs, save slight dyspnœa, anorexia, and frequent pulse. Or suppose a rib to be broken; the diagnosis will not be facile in a storming raving maniac. Nor are the difficulties in the *treatment* of the bodily diseases of the insane of inferior gravity to those of diagnosis. These unfortunates already labour under disease of an organ of exquisite structure and primary importance, whose marvellous sympathies are coextensive with the whole organism, and whose disorders, as a rule, bear adversely on every part and function. Constitutional degeneration is too often concomitant, and any special disease occurring tends to partake of the asthenic character, and its products incline towards devitalisation. Insane refusal of food and medicine, and dietetic anomalies may neutralise the best directed efforts. The management of fractures may be grievously complicated by manaical furor, melancholic delusion, or demented intolerance on the part of patients, leading to ceaseless efforts to cast off all surgical appliances. Such illustrations might be vastly multiplied. The asylum physician finds these conditions hostile to his efforts; his results will not be so quickly attained, nor average so satisfactorily, as they would if he was physician to an ordinary hospital.

But lunatic asylums are especially expected to elucidate psychical disorders. In this connection we need only mention the impossibility of an anatomical diagnosis in most cases of mental disease, and the difficulty of gaining a clue to the maze of words or pantomime of action; while, with regard to the treatment of insanity, we must bear in mind the frequency of incurable organic lesions therein, and the subtle fallacies that attend any conclusion drawn from its treatment by drugs.

We believe, therefore, that too much has been expected from public lunacy experience. But a mass of pathological record must exist, locked up in the case-books of our asylums, and while the medical officers to these have, as a rule, done much in search and observation, it may be that they have not made commensurate efforts in exposition. There is special interest, therefore, in the volume under notice, the first of an annual series of medical reports. It will to some extent answer the strictures made on asylum medical officers of all grades; for eleven of the thirteen contributions are from the pens of gentlemen who are or have been clinical assistants or junior medical officers at Wakefield, while the two other articles are written by the medical chiefs there.

Most of the papers testify to the scientific character of the work done at the West Riding Asylum, to minute accuracy of observation, to patient multiplication of experiments, and to the rich resources of the records.

We will say a few words about each of the articles included in the volume, in the order in which they occur.

I. *Cranial Injuries and Mental Diseases.* By J. Crichton Browne, M.D.—This article awaits completion in the next volume. In the present portion the importance of cranial injuries as causes of mental disease is insisted on, and the suggestion is made that such injuries inflicted on the child during parturition are a prolific source of mental defects and derangements; while attention is drawn to certain rarer causes of cranial injury during childhood. This prominence, claimed for cranial lesions in the etiology of mental derangement, is apparently negatived by the custom of many American tribes. From long antiquity they have taken precautions to deform the skulls of their infants. The tribes which follow this ancestral custom are not known to generate mental defect or disorder thereby, but share, with other similar tribes, that singular immunity from psychical diseases which is enjoyed by races of inferior civilisation. But this non-effect of the artificial cranial deformity on the mental integrity of those subjected to it, will not contravene the position that cranial lesions do really play an important part in the causation of mental disease; for, as Dr. Browne points out, the procedure obtains survival of the fittest infants, by extinguishing the more weakly beings; it causes displacement of the brain, but neither arrest of its development, nor diminution of cranial capacity; it exaggerates the conformation of the skull natural to the race; it leaves the brain free to exert a growing pressure from within, and to resume more or less its pristine site; it inflicts more osseous, than nervous, damage on these uncultured tribes, whose skulls are thicker, and whose nervous systems are much less susceptible than those of long civilised nations. European crania would not undergo, with safety to their more exquisitely delicate contents, the distortions common to the head forms of the dwellers on the mighty pampas, or within the shadows of the Andes. Nor could the European brain sustain with impunity the shocks to which that of the African is often subjected. The negroes of the American states, in their quarrels, make fierce onsets, charging with lowered heads, so that the vertices of the opponents often come into violent collision, rarely, however, causing any injury to their cerebral functions.

Dr. Browne points out that, with the advance from barbarism to civilisation, we also find advancing in a parallel manner an increasing insanity, an increasing average size of head, and an increasing difficulty of childbirth. These three coincident facts possibly bear a more intimate relation than is usually supposed;—a relation which would seem to be supported by the more frequent dystocia and cranial lesion in male births, dependent on the larger average cranial measurement in these. It would seem probable, therefore, that cranial injuries received during birth, and resulting from disproportion between the fœtal head and the maternal passages, form one of the factors of the modern increase of insanity. Idiocy, insanity, or a proclivity to exaggerated predominance of the nervous symptoms in any acute disease, seem sometimes to be the effects of cranial lesions from the application of the forceps at birth.

M. R—, though feeble in mental powers and ineducable, possessed certain musical and other aptitudes, and is cited by our author as "typical of that kind of idiocy which generally results from tedious and abnormal but non-instrumental labours." The case coincides with a form of hereditary idiocy, and cannot be laid down as at all a type of what may be the outcome of abnormal pressure inflicted on the fœtal head in certain forms of dystocia. The injury caused to the fœtus by unsuccessful attempts to procure abortion, was found by Dr. Howe to be the cause of idiocy in several cases. Encephalitic changes, generated by the accidents of birth, may lay the foundation for future insanity. But in many cases, thought to exemplify this statement, it is difficult to assign its proper value to this possible factor. For diathesis and the degeneration of form accompanying the insane in certain families, would lead to more frequent dystocia, to more frequent cranial injury of the offspring, while at the same time there would be a perplexing concomitance of greater hereditariness of mental instability. Cranial injury is probably but a small factor of the increase of insanity amongst the civilised. Fortunately, in children, depression of bone from an accident, and consequent compression of brain, is often only temporary; the natural resiliency of the bone, at that age, with the pulsatory movements of the brain, lead to restoration of the osseous outline,—a result which may often be aided by the traction of an exhausted, and properly adjusted, cupping-glass. In such cases, the concussion effects alone remain to implicate the integrity of the nervous centres—a class of effects less prejudicial to that integrity in childhood than the effects of coarser physical lesions of the brain are. But in adults concussion is more imminently dangerous as a pathway to mental disorder. The well-known statistics of Schläger show a disastrous supervention of such diseases, frequently at a period long subsequent to concussion and cranial injury. But if the hereditary tendency to mental disorder is strong, a slight injury will often bring about a rapid catastrophe, as is well illustrated by Dr. Browne in the case of J. W. M—. He cites, also, two cases in support of the frequent observation that idiocy or insanity often afford an apparent immunity from the ordinary ill effects of blows on the head. In several cases where patients have the extraordinary habit of beating their own heads on the tables or forms, we have thought that their mental powers suffered some slight obscuration as a direct consequence, but this is difficult to decide.

II. *Observations on the Physiological Action of Nitrous Oxide.* By Samuel Mitchell, M.D.—Sir H. Davy compared the action of nitrous oxide to that of the diffusible stimuli, but operating violently, so as to cause death. It produced certain changes in the blood and organs of animals breathing it, at "first connected with increased living action, but terminating in death." This, he thought, was the result of its excessive stimulating property, aud he retained the view that it increased vital action, in spite of the facts that its inhalation does not obviate the debilitating effects of hydrogen, and that animals which have breathed it drown more quickly than others which have not.

Ziegler concluded that nitrous oxide is "a powerful arterial, nervous, and cerebral stimulant." Draper attributed its intoxicating effects to "the oxidizing action which it establishes." Mr. Holmes and Dr. Mitchell summarise Hermann's observations thus:—"Nitrous oxide neither enters into combination with, nor suffers changes from, nor produces changes in, the blood, though easily soluble in it. Blood saturated with it shows no sign of change, the spectrum change being the same."

In disproof of Sir H. Davy's conclusion, that nitrous oxide kills by its property of excessive stimulation of the animal tissues, Dr. Mitchell adduces several elaborate experiments, to show that the death of animals placed in jars of the gas is asphyxial, and that they die somewhat as they do by submersion in water. Yet Ziegler attributed to the gas in solution direct revivifying powers, antidotal to asphyxiating agents used experimentally on animals. In fact, here, as elsewhere, the practical possibilities of the agent must not be obscured by the effect of lethal dosage. Without entering into the possible chemical history of the gas after its absorption into the blood-current, or further discussing its effects on animals, we will briefly notice its effects when inhaled in mixture with common air, firstly, on man in health and sanity; and, secondly, on the insane.

1. *The effect of the dilute gas on the sane.* Dr. Mitchell lays stress on a peculiarity of the delirium caused by inhalation of the dilute gas:

"The ideas seem, as it were, to expand beyond the recognition of the mind, which at this point in its disordered activity also seems to vault over that interval which in its normal condition it recognises to exist between the wish and its realisation. Automatic acts succeed each other, or are repeated with great rapidity, and are performed in an exaggerated manner, the subject of the experiment shouting and gesticulating with the greatest vehemence should he wish to communicate any, even the most trivial thing, and often repeating the last word of a sentence many times, and each time in a louder key. . . . . On one occasion . . . . . on emerging from the state of delirium . . . . . I fancied that I either had shouted or was on the point of doing so, in a triumphant voice, 'I have filled it again' . . . . . I found that I really had turned the key to admit more of the gas into the bag. Had I not come to myself at that moment I have no doubt that in the next this trivial occurrence would have been magnified beyond recognition, and have left the feeling that I had discovered the secret of the universe."

At a certain stage of the action of the gas it was found that artificial contortion of the muscles of expression in imitation of laughing or crying, acted with peculiar potency in causing a paroxysm of either corresponding emotional display. We cannot coincide with Dr. Mitchell's suggestion, that the general propensity to laughter may be partly explained by the convulsive contractions of the diaphragm imitating an important movement in the automatic series accompanying laughter. This contraction would equally tend to produce crying, and "the actions of laughter and crying seem never to originate in the respiratory system" (Carpenter).

But to what is the exhilarant effect of nitrous oxide due? Assuming "that in nitrous oxide we have a chemical compound which, like chloroform, æther, and alcohol, possesses both stimulating and anæsthetic properties, our author discards "the doctrine that stimulants and anæsthetics owe their influence to the elective affinity possessed for them by the nervous substance," in favour of the one "that choloroform, ether, and similar substances, when present in certain quantities in the blood, have the effect of limiting those combinations between the oxygen of the arterial blood and tissues of the body which are essential to sensation, volition, and, in short, all the animal functions." He questions whether the primary exhilaration produced by these substances is not the first sign of the progressive loss of the power of the mind rightly to appreciate surrounding conditions, which culminates in complete paralysis of the cerebral functions."

2. *The use of dilute nitrous oxide in insanity.* Used in melancholia it often caused transitory feelings of happiness, perfect rest, or self confidence. Profuse flow of tears, and much miserable moaning, or even screaming, characterised one stage of the action in some, even in some of those whose black cloud of melancholia seemed lifted by the passing perturbation of the agent; and several afterwards denied having experienced any repressive emotion. Both melancholics and dements, under its influence, occasionally exhibited an unaccustomed power of remembrance, and more coherent speech in reference to long-buried memories. Dr. Mitchell compares this temporary return of partial lucidity to that sometimes seen in lunatics before death, and suggests whether both instances are not "due to the inability of the vital powers any longer to supply sufficient nutriment to the whole nervous mass, the functional activity of that portion of the cerebral substance built up under the most favorable conditions of life alone surviving." Now the rare ante-mortem lucidity of lunatics seems to occur especially in reflex insanity; or if observed in insanity with marked structural brain lesion, it seems to depend rather on the equilibration of lessened powers than on any restoration of vigorous action. The beneficial influence of nitrous oxide on melancholia, dementia, and mania, when it occurs, is unfortunately, like that of ether and chloroform, transitory and fleeting. The mind soon reverts to its usual state, the rifted cloud recloses, the spectral semblance of coherence is lost amidst the more steadfast ruins of mental processes. We would suggest its employment in puerperal insanity, where the exigencies of the special case may imperiously demand a short calm. The greater safety of the nitrous oxide, as compared with ether and chloroform, would seem to warrant its substitution for the latter agents, whose use in such cases has found many advocates. We join in the hope that, by a wider application of the uses of nitrous oxide, it may prove useful in insanity.

III. *The Sphygmograph in Lunatic Asylum Practice.* By George Thompson, L.R.C.P. Lond.—This is really a paper on the sphygmographic tracings in general paralysis of the insane, with an hypothesis as to the state of the arterioles in that affection. Mr. Thompson finds the sphygmographic tracing in various stages of general paralysis to be of one general character, and similar to that found in "chill." In both, he supposes the arteries and capillaries to be in a state of spasmodic contraction. Ergot, atropine, and bromide of potassium, can also produce contraction of the smaller vessels, and a parallel is drawn between the symptoms of "ergotism" and those of general paralysis. We may add that Heusinger's report of the Hessian epidemic of poisoning by ergot, confirms the superficial resemblance of ergotism to general paralysis. Calabar bean and inflammatory fever, which are presumed to paralyse (more or less) the vaso-motor nerves, and consequently relax the spasm of the capillaries and arterioles in general paralysis, restore a more healthy pulse-form in that affection; while Calabar bean consentaneously mitigates the general symptoms. Mr. Thompson therefore, sums up his views as follows:—

"General paralysis of the insane is a disease which may be presumed to be owing to a considerable extent to persistent spasm of the vessels which leads to change in their component elements, but more especially in the muscular substance. This persistent spasm, by reducing the amount of blood which can pass through the vessels to the parts to be nourished, prevents renewal of these parts, and consequently wasting. The most rational treatment indicated is to relieve this spasm. Further, the sphygmograph, by indicating the true nature of the disease at a period when it could barely be suspected by other symptoms, affords an opportunity of applying remedies when mere function is disturbed, before actual change has begun, and when the remedial means can be of the most avail."

But these conclusions are much too large for the basis on which they rest. Granting that the changes in the cerebral vessels indicate a similar change in the vessels of the entire body, and granting that Calabar bean produces its good effects in the occasional attacks of excitement in general paralysis, by relaxing the assumed spasm of the smaller vessels, how is it that bromide of potassium, having an antagonistic effect on the same vessels, is so useful in the same paroxysm? Again, Mr. Thompson finds the tracings, indicative to him of persistent arterial spasm, not only in the very early, but also in the late stages of general paralysis. Now, though the complete pathological anatomy is by no means settled, enough is known to show that the small vessels of the brain undergo various and widely different changes in this affection. Meschede describes the highly developed capillary network crammed with blood-corpuscles, as often dilated, atheromatous, or fatty. Rokitansky speaks of the capillaries as enlarged, twisted, doubled, or presenting aneurismal bulgings, and the increased growth of connective tissue as beginning, partly at least, on their walls.

Sankey states that, "there appears to be some amount of tortuosity in the capillaries in every case of general paresis," and he further speaks of the tortuosity as amounting to a sharp curve, or a kinking of the vessel, or to such a complex twisting as to form "little knots of varicose vessels."

Wedl notes a variety of changes (not confined to general paralysis) ; transverse markings, thickening from hyaline embryonic connective tissue, nucleated granular cells in the areolar coat of the dilated vessels, leading to contraction, calcareous deposition, &c.

Wilks describes calcification of the minute vessels; and Tigges declares the contractility of the coats of the arterioles to be destroyed by amyloid degeneration.

Thus we see how the calibres of the small vessels, their elasticity, and their relation to the blood current, may be so morbidly changed as to seriously involve the nutrition of the parts supplied, and finally lead to atrophy and dementia, without a condition of persistent spasm of the small cerebral vessels, or those throughout the body, being postulated. Such a state of spasm of the small vessels frequently occurs in Bright's disease, but usually leads to hypertrophy of these vessels and of the left ventricle of the heart (comparatively rare in general paralysis), and to a "square-headed" sphygmographic tracing. Again, Sankey's researches show that hypertrophic arterial change is more frequent in other forms of insanity than in general paralysis; in fact, the frequent varicosity of the capillaries in the latter, led him to a conclusion directly opposed to that of Mr. Thompson, viz.: "If the function of the muscular coat of the small arteries is rightly attributed to be that of checking or regulating the amount of the blood supply, the condition would indicate that this interposing function was at fault; that no alarm, as it were, was felt by those vessels; that this action, in fact, was involved in the general paralysis." Of course it remains to be seen whether *changes* noted by Wedl and Sankey, may not really be due to the normally existing perivascular lymph sheaths and spaces of Robin and His.

Instead of changes following persistent spasm of the vessels, the earlier pathological changes would rather appear to be degenerative processes in and around the vascular walls, checking circulation, impeding nutrition, and associated with overgrowth of connective tissue in the cortical substance. While we dissent from the theory of vascular spasm, we may accept the diagnostic value of the pulse-form in early general paralysis.

IV. *The Ophthalmoscope in Mental and Cerebral Diseases.* By CHARLES ALDRIDGE, L.R.C.P. Lond.—This is the longest article in the volume. It embodies the results of numerous and careful observations. Mr. Aldridge finds the ophthalmoscopic appearances after death from failure of the moving powers of the circulation (syncope) to be :—

Optic disc; of a papery whiteness, or faint pink tinge.

Veins; often small or medium in size, sometimes not seen on the disc, or beaded in form.

Arteries; thin, attenuated, often not seen over the disc, but appearing to spring from its edge.

The same appearances were noted before death, but in a lesser degree. In one case of death by apnœa (phthisis), the appearances, four hours before death, were: optic disc congested; veins dilated and tortuous, containing dark blood; arteries of good size; but no examination was made after death. Now, if in such a case the ante-mortem and post-mortem retinal appearances resemble each other, and if it was clearly established that such universal and marked differences existed in the post-mortem retinal condition, according as death was by syncope or apnœa, an important medico-legal test would be obtained, which, in a given case, might found a presumption that death had been due to asthenia on the one hand, or to suffocation on the other, according as the retina was anæmic or congested. This is broached at page 80.

Observations on the eyes of epileptics.—(1.) *During the paroxysm.* One case only, chanced to be examined during the stage of clonic convulsion. "The disc became very pink, . . . the arteries were very small, but there was no alteration in the size of the veins." This does not seem to bear out the author's remarks on the case that, "during the convulsive stage in the case of A. S—, No. 1, the optic disc was seen to be greatly injected, and the arteries larger than usual." Several epileptics were examined just after the clonic convulsions, and when wholly or partially unconscious, with spluttering respiration. The usual appearances were, paleness of the optic disc, and smallness and attenuation of the retinal arteries, while the veins were mostly unchanged. Afterwards, as consciousness became fully established, the capillary tint of the disc deepened, the arteries increased in size, and the usual retinal state succeeded to the anæmia of the state of stupor. But if maniacal excitement followed the fits, active hyperæmia also ensued, with the disc deep-red in hue, and the arteries and veins engorged with blood.

(2.) *Observations on the eyes of epileptics during the inter-paroxysmal period.*—Mr. Thompson records 102 cases fully. *Passive hyperæmia* of the retina and optic disc was found the most frequent condition present. The veins were often large, tortuous, numerous, much branched; the optic disc in such cases of a deep-red tint; the arteries usually not much changed. Taking Schroeder van der Kolk's evidence as to the dilatation of the capillaries of the brain and medulla oblongata of epileptics, he assumes "the ophthalmoscopic appearance of the retinal circulation as an index and guide to the condition of the cerebral." On this basis he investigates the influence of various drugs on the intra-cranial circulation. Finding, that the degree of passive retinal hyperæmia present is often in direct ratio to the number and severity of the epileptic fits; that a diminution of the number of fits by bromide of potassium lessens the hyperæmia; that when the fits recur with their former frequency the hyperæmia recurs also, our author concludes that "the reduction in the amount of passive hyperæmia does not take place immediately as a direct effect of the action of the bromide upon the blood-vessels, but is rather due to the reduction of the number of fits which cause the constant congestion." Now it may be that the less amount of hyperæmia found in the cases benefited by the bromide only means this,—that the bromide does not so often control the morbid tendency after the constant vascular disturbance of the fits has led to marked passive hyperæmia, but frequently controls it when the changes are not so far advanced. Among the 102 cases we find many have been treated by bromide of potassium. On analysing a number of the cases where no benefit resulted, we find the optic disc was in order of numerical frequency, (4) deep red, (3) pink; the veins, (7) tortuous, (6) large, (4) medium in size. Where benefit resulted the order was: optic disc, (7) transparent pale pink, (6) medium pink; the veins, (12) medium in size, (6) large; thus showing much more passive hyperæmia existing on the average in the cases not benefited by the drug.

Ergot.—The effect produced on the retinal circulation by ergot is considered. An epileptic woman, aged 40, who had never been treated, and whose eyes showed marked hyperæmia of the retina and optic disc, took ʒij of the liquid extract of ergot without appreciable ophthalmoscopic change two hours afterwards. "She was ordered to take the same dose of the drug three times a day, and fourteen days afterwards she was again examined with the ophthalmoscope. The fits had in the mean time become less frequent, as formerly she had had a fit every day, but during the last week she had only had three fits in all. The changes in the retinal circulation were now found to consist in contraction of the arteries, lowering of the capillary tint of the disc, and a decided reduction in the calibre of the veins." Mr. Aldridge adds: "This case, with the two recorded at page 93, will, I think, be sufficient to establish the fact that ergot produces contraction of the minute arteries of the retina; and also that such effect is not produced instantaneously, but that some time is required before any evident change can be seen." The case just given above seems strong evidence in his favour, but not so the two cases cited by him to support his conclusion. Both were cases in which epileptic fits were followed by maniacal excitement with marked active hyperæmia of the retinæ, and both had ergot in large doses, one returning to her usual quietude in a fortnight, and the other in four days; when, and not until when, the active engorgement was replaced by the usual vascular state. And this was to be expected as soon as the excitement passed off, and therefore we cannot see that these two cases prove the power of ergot to contract the minute retinal arteries. Our objection does not apply to the case first quoted.

Nitrite of Amyl.—A number of interesting cases are given where ophthalmoscopic examination was made of those inhaling the nitrite of amyl, and where, coincidently with the bright flush about the face and neck, the fuller and more frequent pulse, and the confusion passing into hilarity; the retinal circulation was also excited, the arteries increased in size, and the capillary tint deepened. Similar results were obtained by ophthalmoscopic examination of those inhaling nitrous oxide. The observations made on those who had taken chloral hydrate are too few to bear the weight of any conclusion resting on them.

This whole contribution is evidence of great industry and ability.

V. *A Contribution to the Statistics of General Paralysis.* By J. Wilkie Burman, M.B.—This is a careful statistical study of 341 cases of general paralysis which were admitted into the Devon County Asylum from its opening in 1845 to the end of 1870. Numerous interesting tables are given, for which we must refer the reader to that paper itself. Causes are assigned for less than one third of the cases. The "moral" and "physical" causes are in nearly equal proportion; mental anxiety, pecuniary distress, and domestic affliction, taking the numerical lead in the former class; drink, injury to the head, and sun-stroke, in the latter. These figures bear out to a certain extent the view that strong or imperfectly controlled emotion, intellectual exertion carried on under mental distress, or adverse strokes of fortune, often cause a naturally impressionable hyperæsthetic brain, to break down in general paralysis. Sexual and alcoholic excesses, seem to have played too prominent a part in the assigned etiology of this disease, and must be looked on as a part only of that "sensuality" and "fastness" to which again only one class of the cases can be referred.

Salomon wrote in 1863, "The diffuse periencephalitis (general paresis) presents incontestably a striking analogy to diffuse nephritis (= morbus Brightii). The former is anatomically characterised by a degeneration in the tissue of the cortical substance of the brain, destroying the nerve-tubes and nerve-cells. Clinically, it is characterised by a profound alteration in the function of the cortical substance of the brain. The latter is anatomically characterised by a degeneration of the tissue of the kidney, and by alteration in the urinary canals and malpighian bodies. Clinically, it is characterised by a profound change in the functions of the kidneys. In both diseases we observe stages of hyperæmia, increase of volume, degeneration (softening) and atrophy." Mr. Burman mentions the same homology between the brain disease in many cases of general paralysis, and that presented by contracted granular kidney and cirrhotic liver, and adds, "Why should not such morbid changes occur from *toxic* causes in the brain as well as in the 'gouty' kidney or the 'gin-drinker's' liver? and is there not reason to believe that alcohol is an important agent in producing the disease of the nervous system in many cases of general paralysis? The frequent occurrence of the disease amongst drunkards, and the fact that the morbid changes are in the first instance vascular, and occur in those parts of the brain where vessels most abound, tend to make us believe that the disease is often of toxic origin. These remarks only apply, of course, to one (yet a large) class of cases of general paralysis." This is not a new suggestion. Far from it. It was made by Dr. Hitchman (amongst others) twenty years ago ('Journ. Psych. Med.,' vol. iii, p. 244).—"The blood may be so contaminated as to give rise to a temporary general paralysis; and if to a temporary disease, then to a permanent one; for it is obvious that the general paralysis would last precisely as long as the alcohol in definite quantities should remain in the blood unremoved by the excretory vessels of the system. Now, if it can be produced by one extraneous element in the blood, it may be by others; and hence the importance which a diseased heart, especially endocarditis, or an affected kidney, may have in inducing this malady, in such very rare and isolated cases as those detailed by Lelut, where no appreciable lesion could be detected in the brain or its membranes."

Marked changes were found in the brains and cerebral membranes of drunkards, by Dr. F. Ogston, in his numerous autopsies. Wedl describes calcareous deposits in the small cerebral vessels of some drunkards. Dr. G. Johnson has shown that morbidly altered blood may cause contraction of, and subsequent changes in, the minute arteries of various parts, including the brain. Prominent amongst the pathological changes in general paralysis, are the changes in the arterioles of the pia mater and brain; and it is interesting, in this relation, that chronic alcoholismus often much resembles the paresifying mental disease. In short, it seems not unlikely that certain cases of general paralysis may be "toxic" in origin.

VI. *On the Treatment of Insanity by the Hypodermic Injection of Morphia.* By J. BYWATER WARD, B.A., M.B.—In many cases of insanity where the patient refuses and struggles against medicine administered by the mouth, or where gastric disorder might interfere with the passage of the drug into the circulation, the hypodermic use of certain remedies is valuable. Too much was claimed at first for this special method of exhibiting morphia. With all the advantages of a ready and certain access to the circulation, we have found the hypodermic use of morphia in insanity, subject to certain drawbacks to which all forms of opiate influence are liable. Nausea, vomiting, or headache, have not infrequently followed. Dr. Ward's evidence is confirmatory of this. In one of his cases of melancholia, vomiting occurred after the nightly injection of one third of a grain of morphia, while three grains were taken daily by the mouth without that unpleasant result. In such cases of vomiting, &c., after the morphia injection, Reissner recommends the substitution of narceia; and we have lately found the unpleasant effects of morphia much obviated by half-drachm doses of potassium bromide, given three or four hours beforehand, as recommended by Da Costa.

In one of Dr. Ward's cases of mania, the patient, aged seventy-four, had not been relieved by chloral and hyoscyamus, and the injection of a sixth of a grain of morphia failed to procure rest, but next morning "the countenance was extremely pallid, the pulse very feeble, and the surface of the body cold." He rallied under stimulants. Dr. McIntosh published a number of cases of insanity treated by this method, ten years ago. In one of his cases (a woman, aged thirty-four), the injection of one grain of morphia was followed by alarming symptoms.

Dr. Ward's cases are not numerous, nor are the results decisive. Some are scarcely illustrations of the morphia treatment. The exacerbations of excitement in chronic mania afforded the most satisfactory instances of relief. In two out of four cases these attacks were much shortened, in the third the injection soon lost its good effect, and in the fourth it caused obstinate vomiting. The treatment was not as successful as might have been anticipated in melancholia. In one case, the injection of one third or one half of a grain every night failed to improve the mental state, though continued for several months. A peculiar deferred action of the drug was noted in one case of puerperal mania. It usually caused sound sleep, but not until eight to twelve hours after the hypodermic injection of a third of a grain.

VII. *On Mollities Ossium and Allied Diseases.* By George Henry Pedler, L.R.C.P. Lond.—Eight cases illustrate this paper, and in several of them post-mortem examination showed the bone-changes and the deformities characteristic of the affection. One or two of the cases appear to be examples of allied fragilitas ossium. The general literature on the subject is much absorbed by the obstetric relation of these deforming affections, which are really worthy of study as bearing on the "rib breaking" cases in asylums. The cases met with in asylums, as a rule, have supervened during asylum residence, and, therefore, have not been complicated by the aggravating influence of gestations under miserable and insanitary surroundings. Several of the cases mentioned by Mr. Pedler had been under asylum care for years before the advent of bone-softening, and we have met with one case in which the mollities came on when the patient had been an asylum inmate for nineteen years. It may be useful to remember these cases when the nature of the disease is under discussion. The nature of the affection has been the subject of a variety of opinions. Leaving out of view several theories which have obtained less hold on the profession, the two which appear most prominently in the literature of the subject are; first, that mollities ossium is of an inflammatory nature, that it is a sort of slow ostitis; second, that it is due to arrested or impaired bone-nutrition, from loss of nerve power and failure of its normal influence on that nutrition. This was more or less set forth by Mr. Durham, and by Dr. Jones of Georgia. Mr. Pedler gives his adhesion to the latter theory, and argues that the dilated vessels and increased vascularity of the grumous and fatty bone, is due rather to paralytic vascular dilatation than to inflammation. He also coincides with the view that the diminution of lime-salts in the bones is due to failure of their natural deposition, as part of the failure of osseous nutrition, rather than to their increased atrophic absorption. Mollities ossium usually arises under exhausting, depressing circumstances of life. These circumstances are sadly too frequent. It remains to be shown whether malacosteon, in these cases, is brought about by the failure of the part played by nerve-agency in the nutritious processes. Minute examination of the nerves of the parts is required. Mr. Pedler's article indicates the point from which this work might be begun.

VIII. *On Locomotor Ataxy and some other forms of Locomotor Deficiency as found in the Insane.* By PATRICK NICOL, M.A., M.B.—Some years ago, Westphal drew attention to the mental disease following after long existing grey degeneration of the posterior columns of the spinal cord, as presenting a marked similarity to general paralysis of the insane. Common to both are the grandiose delirium, giddiness, loss of consciousness, and convulsive seizures. But the former differs strikingly from ordinary general paralysis, in the symptom that staggering and falling occur when the eyes are closed. Some of Dr. Nicol's cases resemble forms of general paralysis most strongly, if we accept some later observations by Westphal. The latter finds in the disordered gait of general paralysis two great clinical groups; the *tabic* and *paralytic.* General paralytics with the tabic form, lift their legs high, throw them out with force, advance in a staggering manner, and on closing the eyes when standing, they stagger or fall. Those with the paralytic and usual form raise the feet but slightly, advance slowly and insecurely, and do not stagger or fall when the eyes are closed. Moreover, the "tabic" form of gait may be traced to grey degeneration of the posterior columns of the cord, while the "paralytic" form has relation to the chronic myelitis found when the lateral, or the posterior and lateral, columns are the site of spinal disintegration. In a fully recognised set of cases of general paralysis, spinal disease precedes psychical disturbance, and this set includes those of early tabic gait, and (probably) some of Dr. Nicol's cases which appear to fall under this class. It remains to be proved, then, how far such "manie des grandeurs" or buoyancy, as frequently occurs in the ataxic insane, has special connection with Duchenne's disease.

The ataxia has been accounted for in a variety of ways by authors, and Dr. Nicol, accepting Trousseau's conclusion that "spasm" is an essential element in the locomotor disorder, propounds the view that ataxia is a disease of escaping force; that lesion of the incito-motor nerves of locomotion disturbs the efficiency of the corresponding motor centres of the spinal cord, subordinate to the brain; that increased volitional effort results, with copious irregular spasmodic supply of nerve force to the limbs, causing ataxic movements. Space will only allow us to add, in his own words, Dr. Nicol's views of "the connection of this disease of unstable power with insanity." He writes—

"Our active states are in part connected with a feeling pleasurable in quality, called the feeling of power. When, therefore, a dribbling or gushing away of power, shown as muscular activity, is connected with a disease of the cerebral hemispheres, and presumably arouses in its course the mysterious phase of life we call *mind*, what ought we to expect but that the peculiar emotion of activity—the pleasure of power—should come abundantly forth, like coruscations of electric light, distinctive of what medium a current is passing through?

"Granted that locomotor ataxy is a disease in which running away of power is an essential element, and granted that there is that openness for transmission of organic impressions towards the mental centres which is so often marked in the insane diathesis; or, on the other hand, that the necessity for decay which lay upon the afferent fibres of locomotion, and produced (as we say) the lesion there, may extend by anatomical analogy to other fibres which subserve the comparisons between objective circumstances constantly going on in our minds subordinately to our volitional life; fibres, namely, which, with help, collect all the impressions for us, carry them towards the brain, and often are privileged to draw power for an 'ideo-motor' action of their own; granted that these too are involved in the ruin, and that the stored-up power they formerly set in motion is left to rush off wildly with each mad attempt to volitionize apart from a true sense of externals, and it will no longer remain a matter of surprise that exaltation is the phase of emotional life which locomotor ataxy determines in the brain."

The article closes with some loosely connected cases and remarks (on other forms of locomotor deficiency), the omission of which would have been an improvement.

IX. *On the Artificial Feeding of the Insane.* By William Lawrence, M.B.—Several ways of feeding those insane persons who are bent on refusing food are here described, especially the method in use at the West Riding Asylum. We believe that for the majority of such patients, Dr. Moxey's plan, as detailed by him in the 'Lancet' of March 20th and 27th, 1869, will be found superior to any described in this paper.

X. *Arachnoid Cysts.* By Henry Sutherland, M.A., M.B.—Leaving out of view the possible formation of cysts in meningeal hæmorrhage, by splitting up the layers of the dura mater, as described by Abercrombie; or by the peeling off of the parietal arachnoid from the dura mater by the blood-clot; there are two leading views relating to the production of cysts in the membranes of the brain. The one most in favour attributes them to an effusion of blood into the arachnoid cavity; the peripheral layer of which blood soon encloses the effusion as a membraniform layer, or such membrane, at least, is formed around the clot without inflammation. These walls become stronger, organised, and form cysts; while various changes take place in the included blood. The second view, that the hæmorrhage is from the newly formed vessels of the organised layers and meshes of former inflammatory products, and becomes encysted in these, has not found much favour with English pathologists, but is probably the true explanation of some cysts found in the aged. Dr. Sutherland adheres to the former view, and gives ten cases in which arachnoid cysts were found after death; five in 'organic' dementia, three in general paralysis, and one each, in idiocy, and imbecility. In five of these cases the cysts were over both hemispheres, and it has been noticed by Dr. Ramskill that they are not always confined to one side. It is not a little interesting that "in cases where one cyst only exists, the entire brain seems to be pushed across towards the other side of the skull, which is found to be unsymmetrical," bulging in that direction under the pressure of the brain. The history of an injury to the skull existed in four of the cases, and in the others atheroma of the vessels of the base of the brain, or of the aorta, was found; and further, from the analysis of his cases, Dr. Sutherland concludes that some impoverished condition of the blood predisposed to cyst-formation.

"In one case repeated crops of boils harassed the patient; in another amenorrhœa with anæmia-chlorosis was present; in two cases, erysipelas; in three, purpura; in four, œdema of the legs, from some cause or other; and in no less than six cases tubercular deposits were found in the lungs."

In one case only, did hemiplegia exist, and was probably due to destruction of the opposite corpus striatum by an old apoplectic clot. In Dr. Ogle's case ('Journal Ment. Sci,.' vol. x, p. 525) the hemiplegia existed on the *same side* as the arachnoid cyst, and probably was not due to the latter, as changes existed in the pons varolii, and the corpus striatum and optic thalamus *opposite* to the side paralysed, were slightly softened. In fact, intra-arachnoid hæmorrhage with subsequent cyst formation, seems to be rarely associated with motor paralysis, but often with mental impairment tending to fatuity.

XI. *Phthisis and Insanity*, by PATRICK NICOL, M.A., M.B., *and* W. WATSON DOVE, L.R.C.P., M.R.C.S. This is mainly controversial. Dr. Clouston gives precedence to tuberculosis in the relation between it and insanity. Our authors would rather reverse the order, but as the gist of their paper is negative, "suggesting doubts and queries," we prefer a brief statement of their case to special criticism. Tubercle (say they) is a disease of the vegetative life, and such diseases rarely invade the territory of the animal functions, while diseases of the latter (mental diseases) often seriously modify bodily disorders. It is, therefore, *à priori* improbable that tubercle will markedly affect the functions of animal life (cause mental disease).

Against Clouston's conclusion that phthisis is a more frequent assigned cause of death in the insane than in the sane population (29:21), they bring the West Riding Asylum statistics to prove that the percentage is really lower among the insane. The bases of both the opposing conclusions are narrow and fallacious. Clouston found the age at death of the tubercular insane, below that of the insane generally. Messrs. Nicol and Dove point out that many of the insane are young, and youth is the special age for tubercle, so that "it is not a matter of surprise that it is in the young insane" that tubercle is most fatal. When they are found together which preceded? Phthisis or insanity? Phthisis (as a rule), according to Clouston. From his cases he thought the phthisis could not often have been engendered after the insanity had set in. Our authors bring the West Riding Asylum statistics to show that the greatest proportional number of those dying in the first stage of phthisis, when the tubercle, therefore, was not the sole cause of death, died within three years of admission; showing that the tubercular involvement was often probably during the course of insanity. But suppose phthisis and insanity contemporaneous in their onset. As both are largely hereditary, and the hereditary predisposition to insanity more frequent among the tubercular insane than the insane generally, "a strong tendency to tubercle may have been present, when the sudden exacerbation of self-neglect and exposure to sources of disease, that always mark the beginning of insanity, came on. Thus the cotemporaneous arrival of the insanity and the phthisical symptoms is accounted for in another way than by supposing the tubercle to cause the insanity."

Next, as to the "phthisical insanity" of Clouston, our authors urge that, granting the fact of the connection between *one* form of insanity and tuberculosis, "it proves nothing in itself as regards the causal arrangement of the association. It may be, indeed, that phthisis tends more to produce that form, but it may be that that form tends most to produce phthisis," and they press into view the habits of melancholics, and of suspicious monomaniacs, with the oft ill-regulated previous lives of the latter, as tending to phthisis. Moreover, phthisis is not always the same lesion; hence, if it occurs with a certain form of insanity, we should rather consider "the insanity as affecting the lung than the decay of the lung as causing the insanity, unless it could be shown that the phthisis was in all the cases at bottom one and the same lesion."

The effect of Insanity on Phthisis is the second great division of the subject under discussion; and here Messrs. Nicol and Dove enunciate the sources of vital depression springing from insanity, and so tending to tubercle. Their argument is in direct opposition to the statistics of Clouston, which seem to show that the tendency to tuberculosis decreases in proportion to the length of insanity. The latter writes:

"The fact that phthisis is not common in the last and deepest stages of dementia, when the nerve functions are carried on with minimum activity, is not favorable to the idea that the ordinary forms of insanity predispose to tuberculosis. The tendency to tuberculosis which we have seen diminishes rapidly in proportion to the length of the insanity, although partly explained by the rarer occurrence of phthisis as age advances, yet is pretty clear proof that on the whole insanity does not tend to the development of phthisis."

Cases of mania, or rather of the delirium of advanced phthisis, such as conclude the paper, *are* mentioned (the authors say not) by Dr. Clouston, who calls them "the connecting links between phthisical irritability and phthisical mania." The authors desire to show that Dr. Clouston's facts do not *prove* the insanity to depend (as a rule) on the tuberculosis, when these coexist. There is much to recommend their negative conclusion, that "this is a matter in which nothing is easier than to see that there is some causal connection, and nothing perhaps more difficult than to see what precisely is the order of the connection."

XII. *Acute Delirious Melancholia.* By Charles Henry Mayhew, L.R.C.P. Lond., M.R.C.S. — This contribution attempts to erect into a separate variety of disease one, of the forms of "acute delirious mania" (acute maniacal delirium, acute delirium, délire aigue), the writer's knowledge of which appears to spring from Dr. Blandford's recent work, though it has been described by English and Continental authorities. Mr. Mayhew would give the name *acute delirious melancholia* to those cases of the affection in which anguished emotion predominates, and offers two graphic examples in illustration. Such occasional excess of anxious fear, &c., has been recognised in former descriptions of the disease, and we fail to see the benefit of the divorce suggested.

XIII. *Ergot of Rye in the Treatment of Mental Diseases.* By E. Churchill Fox, M.B., C.M.—The use of this drug in insanity has been treated of by Dr. C. Browne in a late number of the 'Practitioner,' and the cases cited by Mr. Fox may be taken as illustrations of the views enunciated there. Epileptic mania, recurrent mania, and the paroxysms in the course of chronic mania, are represented. The very irregular course of such attacks, the manner in which they often suddenly subside without treatment, call for circumspection in conceding the power of arrest to any drug exhibited. The second and third cases related demand such circumspection, while the fourth is strong testimony to the value of the ergot (epileptic mania). We need only add the closing remarks:

"It has sometimes occurred to me that bromide of potassium, the value of which I would be far from depreciating, has a tendency, in some cases, to aggravate the attacks of epileptic mania. It seems to relieve the muscular at the expense of the mental element in the epileptic condition. The fits are reduced in number and severity, but the paroxysms of mental disturbance are intensified and prolonged. Under such circumstances, ergot becomes of the highest service; its use, alternated with that of the bromide of potassium, places the two phases of the malady under equal and powerful control."

IV.—A Manual of Medical Jurisprudence for India.[1]

(SECOND NOTICE.)

We now proceed, in accordance with the intention which we expressed in our first notice of this valuable work (October, 1871), to give a sketch of the vast mass of information which Dr. Chevers has collected upon the poisons employed in India, and especially in Bengal.

Secret poisoning seems to have been a common crime in India from the earliest times. Few of our readers are probably aware that the Suttee, which we have taken such trouble to suppress, was originally introduced (certainly before the time of Strabo, who lived at the commencement of our era) as a check upon the practice common amongst Indian women of poisoning their husbands. Passing over seventeen centuries we have the evidence of Captain Hamilton, who traded in India between 1688 and 1723 (quoted by Dr. Chevers) that the same system was then in existence:

"In Canara (he observes) there are several customs peculiar to itself, and many of them are spread abroad to remote countries.

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[1] *A Manual of Medical Jurisprudence for India, including an Outline of a History of Crime against the Person in India.* By NORMAN CHEVERS, M.D., Surgeon-Major H.M. Bengal Army, Principal of the Calcutta Medical College, &c. Calcutta, 1870. Pp. xix and 861.

Here it was that the custom of wives burning on the same pile with their deceased husbands had its beginning. It is reported that, before the Brahmins invented this law, poison was so well known and practised that the least quarrel that happened between a married couple cost the husband his life, and this law put a great stop to it; and now custom so far prevails that, if any faint-hearted lady has not courage enough to accompany her spouse to the other world, she is forthwith shaved and disregarded, and obliged to serve all her husband's family in all kinds of drudgery."

Purchas quotes two authorities, Plericus and W. Methold, who similarly explain "the cause of burning the wives."

The custom (prevalent apparently in India as formerly it was in Greece and Rome) of administering philtres or love-charms, in which the ingredients are stupefying and often dangerous, and a very frequent cause of poisoning. A writer in 'The Jubbulpore Miscellany,' a periodical which we have never had the privilege of seeing, but which is quoted by Dr. Chevers, states that this system is mostly practised "by jealous women, or desperate lovers of either sex, for the purpose of captivating affection, of infatuating and enthralling the object of desire. But it is also resorted to for baneful purposes, to cause disease, death, or some strange aberration; and whether employed in love or in hate, it has certainly been always intimately connected with some real knowledge of medicine, and has veiled a great deal of downright poisoning. The ingredients of which the philtres are ordinarily compounded are, to this day, not a whit less disgusting than the contents of the witches' cauldron in 'Macbeth,' and perhaps Shakespeare got from the East the idea of adding a tiger's entrails and a baboon's blood."

Owls' flesh seems to have a peculiar reputation in the East. Dr. Chevers was asked by a judicial officer whether a person could be rendered impotent by eating it; and a work of some celebrity, the 'Taleef Shareef,' contains the statement that "the women of India give it to their husbands, that by the mental weakness it produces they may obtain more liberty of conduct than might otherwise be agreeable."

Attempts have been made by the Government to obtain from the civil surgeons on the different stations a complete and accurate list of all the poisons obtainable in Indian bazaars; and, from the lists thus sent in, Dr. Chevers has compiled the table given in the following paragraph:

"Great (he observes) as is the obscurity which envelopes the history of many of the poisonous substances used in India, the present inquiry leads me to feel convinced that the number of poisons which are used freely by the natives of the three Presidencies is very limited indeed. The chief of these are comprised in the following table:—

"I.	The preparations of Arsenic	For assassination and suicide.
	Aconite	
	Nux vomica	
	Opium	
	Lall chitra	
	Oleander	
II.	Datoorah	With a view to producing intoxication, insensibility or fatuity, but not perhaps with intent to kill, although death frequently results from their use.
	Gunjah	
III.	Lall chitra	For abortion.
IV.	Sulphate of copper	Given as medicines in poisonous doses.
	Arsenic . } In the *Bish Baree*[1]	
	Snake poison . }	

"Doubtless, further experience may call for additions to this list, and especially to the third and fourth classes; but it must be repeated that the number of poisons commonly employed with criminal intent in India, probably does not much exceed that given in the first and second classes of the above list."—p. 108.

Arsenical poisoning is first considered; and as the arsenic trade, which exceeds 2000 hundredweights in Calcutta alone, is unrestricted, and is almost entirely in the hands of natives, it may be easily supposed that the preparations of this article are often used for improper purposes. Arsenic is legitimately employed in India for numerous objects, as to poison rats; to protect timber, and other substances against the ravages of insects; to destroy animal life in the holds of vessels; to prepare certain kinds of thick leather, &c.; but besides these purposes it is frequently employed by native doctors to a very considerable extent for not only intermittent and remittent fevers, but for every form of fever, including typhus; also as an aphrodisiac in cases of recent as well as of long standing impotence; as an alterative in rheumatism, gout, and secondary syphilis; as a substitute for opium in the case of opium-eaters wishing to reform; and, lastly, as a useful external remedy, and, mixed with milk, used as a wash in various skin diseases, especially the scaly varieties. It seems also to be employed both externally and internally as a remedy for scabies.

With the best intentions on the part of native doctors, this form of practice must inevitably often lead to fatal mistakes; but independently of accidental poisoning, Dr. Chevers finds that in the fifteen years preceding March, 1870, no less than *two hundred and eleven* cases in which white arsenic was used as a poison, were brought before the Calcutta chemical examiner.

The diabolical crime of poisoning well or tank water is not unknown in India. In the winter of 1815 the army in Guzerat,

[1] The *Bish Baree* or *Bish Boree* is a popular preparation amongst the Bengalee practitioners.

on reaching their camping-ground, were told that the enemy's cavalry had poisoned the wells with arsenic, and although orders were given that water should not be drawn from them, several of the followers suffered severely. Dr. Chevers gives us a later case of well-poisoning that occurred in 1868. The water from a well, in the station of Kyouk Phyor, when used, produced vomiting, giddiness, and irritation over the surface of the body, and on examination was found to contain arsenic in considerable quantity.

We are not aware that any such cases of arsenical poisoning as the following have ever previously been put on record.

"The late Mr. Henry Piddington, coroner of Calcutta, sent me two cases, in which unmistakable and very violent symptoms of irritant poisoning distinctly arose from eating the *salted ox tongues* commonly sold in the bazaars of Calcutta. It is certainly also remarkable that I was, two years ago, consulted in the fatal case of a European lady in this city, who was attacked with cholera shortly after having eaten some cold slices of such a tongue at breakfast. I should have considered that the poison here was an animal one, except that Mr. Piddington notes—"I have heard from several persons that, in hot weather, *arsenic* is used to make the meat take the salt, a portion being mixed with the salt." Thus used it would, doubtless, also assist to prevent the meat from becoming tainted. It is remarkable that, in one of Mr. Piddington's cases, where several of a family were poisoned, a dog, which it was believed had eaten of the tongue, was also sick" (p. 120.)

Dr. Chevers throws more light than any other author whom we can call to mind on a peculiar form of post-mortem appearance that seems always, at least in India, to be connected with arsenical poisoning, namely, the presence of ecchymosis, in patches or diffused, in the endocardium. The following, in an abbreviated form, are his remarks on the subject.

In 1866 Dr. Bonavia, of Lucknow, stated that, in several cases of arsenical poisoning, he had invariably found livid patches in the inner lining of the heart, more especially that of the left ventricle, about the *columnæ carneæ*, so much so that, in cases of suspected poisoning, he always examines the heart first; if he discovers these patches, he invariably finds arsenic in the stomach. The greater or less size and depth of colour of the patches appear to bear some proportion to the more or less extent and intensity of redness in the mucous membrane of the stomach.

He refers to an observation by Andral, that arsenic is one of the causes of *endocarditis;* he, however, notices that Dr. Taylor, in his work on Poisons, rejects the idea that there are any heart changes which indicate arsenical poisoning. Dr. Bonavia remarks that "when the natives use arsenic for poisoning they do it thoroughly," and suggests that, where death is caused by comparatively small doses, this cardiac lesion may not be produced. Dr. Bonavia's observation has since been confirmed by several Indian authorities.

Dr. Kenneth McLeod, then of Jessore, gives the case of a young man who, not being able to bear the disgrace of having contracted syphilis, killed himself by swallowing half a tolah (90 grains) of arsenious acid.

On post-mortem examination, four hours after death, the left ventricle of the heart was found empty, with the *lining membrane of a dark livid colour*. This tint was deeper on the *columnæ carneæ* than between them, and was well marked over the whole surface of the cavity. The deep colour extended about an eighth of an inch into the substance of the heart, and seemed to be owing to a layer of blood extravasated beneath the lining membrane, which was quite smooth; otherwise the heart was not in any way affected by the poison.

Mr. McReddie found, in a Mussulman of Hurdui, "patches which might be termed *sub-endocardial ecchymosis* in the left ventricle, about the *carneæ columnæ*, not in them. These patches extended about a line in depth; they were not numerous, but were well marked." The chemical examiner for Oudh found "unequivocal proofs of arsenic in the stomach."

Dr. Harris's remarks on those appearances, in the case of a young Mussulman who committed suicide by swallowing white arsenic are very suggestive:

"It has been said that the size of the patches in the heart appears to bear some relation to the extent of those met with in the stomach. I think, however, it should rather be said that they both are related to the greater or less amount of the poison taken. As some small doses of arsenic cause congestion of all the internal organs, it does not require a great stretch of the imagination to conceive that a larger dose may produce a stage of disease in advance of this, in the form of effusion from the already congested vessels of these organs, constituting in the brain serous or sanguineous effusion into the meshes of the arachnoid, and in the heart sanguineous effusion into the endocardium; the last, perhaps, immediately due to the bruising of the congested capillaries of the endocardium by the powerful muscular action of the left side of the heart. It will be very interesting in future to observe the stethoscopic sounds in these cases during life" (p. 122).

The preceding observations apply specially to white arsenic or arsenious acid (*Sumool-khar*); the yellow sulphide (*Hartal*), and the red sulphide (*Mansil*), are also occasionally employed as poisons. Two cases of chronic poisoning, caused by sleeping in rooms papered with arsenical green, have been recorded in India, one by Dr. Shortt, and the other by Dr. Horace Day. The case reported by Dr. Day is singular for its complications. The Patient had symptoms resembling dysentery, which was so rare a disease in the locality that another cause for the symptoms was sought for and found in the bedroom paper:

"But, besides the symptoms of poisoning by arsenic, a periodic headache, lasting about six hours, came on every day. It appeared that he had formerly suffered from Guzerat fever. Here, then, was a cause for the headache, which lasted after the other symptoms had abated; removal from the bedroom having removed those, quinine soon cured the headache. Attention is drawn to the fact that the arsenic and the fever-poison were simultaneously at work. Dr. Day suggests that, but for the arsenic, tertian fever might have occurred instead of mere headache."

Dr. Chevers has only one slight note of a case of paralysis from arsenical poisoning, viz. that of an officer, who came before the Medical Board at Calcutta in a state of considerable emaciation and cachexia, and with paralysis of the forearms, almost as complete as ever occurs in painters. He quotes, however, a case of this affection, which gives us, in so far as treatment is concerned, a curious glimpse of native therapeutics:

"Mr. Jayaker admitted one Foola Mona to hospital at Ahmedabad (Bombay Presidency) with Carter's disease of the foot in an advanced stage. The leg was amputated, and, except that there was an attack of secondary hæmorrhage, the stump did well; but anæsthesia of both hands, which had been complained of on admission, continued to increase. The hands were partially paralysed, and the flexors of the fingers were strongly contracted. It was discovered that, two months before his admission, he had a poultice applied for about a week by a hakeem, which contained nearly three ounces of arsenic, and an incredible quantity of cayenne pepper (seven pounds). This having given rise to constant vomiting and purging, the arsenic was omitted after the second (?) application. It was followed by a burning sensation throughout the body, which, after the operation, continued to be present in the extremities, the stump not excepted. The symptoms in the hands made their first appearance a fortnight after the last application. He gradually improved under peroxide (*sic*) of potassium and tincture of belladonna, until he was discharged cured in about nine weeks" (p. 127).

The antidotes used by the native doctors are almost as bad as the poison itself. In Madras a lump of sulphate of copper, of unknown weight, is rubbed on a cut lime, which is then squeezed into the patient's mouth. The froth of the soap-nut is also used as an emetic in these cases, but it is very irritant and uncertain in its action. The natives of Bengal use the washings of stinking fish and human ordure as emetics in cases of poisoning; and in a case of arsenical poisoning that occurred lately at Tepherat Dr. Chevers was informed that "the symptoms were not relieved till the man had swallowed some human fæces."

In a section on "Cattle Poisoning" we have a mass of very curious facts. After pointing out, on the authority of Mr. Borrow ('The Zincali,' 4th ed., p. 12), that gypsies to the present day occasionally poison cattle, horses and swine, for the sake of obtaining the carcass of the animal, Dr. Chevers gives a case in which the horses of our artillery were poisoned in considerable numbers for their skins. Elephants are sometimes poisoned, but cattle afford by far the greatest number of victims. They are poisoned in various ways, of which the following is, perhaps, the most ingenious:

"In August, 1852, the first magistrate of Pubna was informed that the Chumars of a certain village had been for some months in the habit of killing cattle in the neighbourhood for the sake of their skins, by thrusting into the rectum certain poisonous balls. Some of the poisoners confessed and produced several balls, two of which were sent to the chemical examiner. They also produced a pointed stick at which the end of the ball, it was understood, was fixed and thrust into the body. When the stick was withdrawn the ball remained; the consequences were that the body swelled, and in some cases a small quantity of blood was passed, and the animal died in about twelve hours in great pain. The balls were found to be perforated with an aperture for the pointed stick, and contained a resinous matter, kept in form by being mixed with a large quantity of hair rolled up somewhat like a silk-worm's cocoon with one end off. The nature of the resinous matter could not be decided by analysis" (p. 130.)

In 1854 Mr. George Campbell, then magistrate at Azinghur, discovered that a surprisingly extensive system of cattle-poisoning was carried on by a leather dealer, who employed certain agents to administer arsenic in balls of flour to cattle in their hay, &c. We fully agree with Dr. Chevers that it is fully time that "the importation, or at all events the sale, of arsenic in India should be regulated by a stringent legislative enactment."

From the consideration of arsenic the author passes to *Aconite*, to which about a dozen pages are devoted. The following are some of the uses to which it is applied by the natives.

"The preparation of the root of the *Aconitum ferox*, or *Bish.* is much used in all the hill districts of India to poison arrows for the destruction of wild beasts.

"In a tank of water destined for part of the British army on a halt in pursuit of the retreating Burmese, the water had been poisoned by the *Aconitum ferox*, bruised and thrown in by the enemy before they evacuated the place; undoubtedly fatal consequences would have ensued, had not Dr. Wallich discovered it. Dr. Wallich says of the *Vishavish* or *Bish*, that the Gorkhalese pretend that it is one of their principal securities against invasion from the low countries.

"In medicine the Bish is chiefly employed by the natives in the treatment of leprosy, fever, cholera, and rheumatism. A Lepeha described the root to Captain Walter Sherwill as being 'useful to sportsmen for destroying elephants and tigers, useful to the rich for putting troublesome relations out of the way, and useful to jealous husbands for the purpose of destroying faithless wives'" (p. 136).

The active principle of the Assam poison, known as *Mismee Bish*, is undoubtedly the juice of the root of this plant, although the composition of it is kept a profound secret. Considering that even a scratch from an arrow so poisoned is followed by almost instant death, we can fully recognise the noble self-devotion of Dr. White, now of Debroghur, who, when his men were wounded by the Abors (an Assam tribe) with these arrows, "sucked the wounds, and suffered to a marked degree from that numbness of the lips and tongue which characterises aconite poisoning."

The case of a boatman aged 35, who had taken a small portion of aconite root by mistake, and was successfully treated in the Hospital by Baboo Tarprosunno, a late native assistant of Dr. Chevers, illustrates very clearly the ordinary symptoms of aconite poisoning, of which no less than thirty-six instances came under the notice of the Calcutta chemical examiner in the ten years from 1860 to 1869 inclusive.

"The peculiar action of the poison was fully exhibited; the acridity was manifested by the obstinate retching and vomiting, constant spitting of saliva, and burning sensation in the pit of the stomach. The depressing influence of the poison rendered the pulse small, slow, weak, and intermittent, and gave rise to the hurried laborious breathing and the sense of void within the cardiac region. Its narcotic action was illustrated by the immediate impairment of sensibility, characterised by the tingling and numbness of the lips and tongue, almost coincident with the chewing of the root. At first the action was exerted locally upon the peripheral distribution of the nerves, but it subsequently affected the central ganglia, as proved by the tingling and numbness becoming universal, and the inability of the patient to stand on his legs owing to the paralysis. He could move his arms about without any difficulty, and could steady them in any position at will. The disordered sensibility continued more or less till the other symptoms of poisoning had subsided. The great peculiarity of the poison was that it left the mental faculties perfectly clear, even during the height of the symptoms. The pupils became dilated up to a certain degree, but never reached that extreme dilatation which is characteristic of the solanaceous plants. They were totally paralysed, without affording any sign of that irritability of the nerves of the iris indicated by alternate dilatation and contraction" (pp. 145-6).

There is a prevalent belief that an antidote for aconite, prepared from the roots and barks of trees, is known to the hill-men; but no trustworthy facts on the subject have been adduced.

Thuggee by poison is the subject of a very interesting chapter. Dr. Chevers tells us that there is scarcely a village in which a hag of low caste is not to be found "suspected as a witch, professedly a midwife, equally ready at all times to practise as a doctress, a sorceress, or a bawd, and carrying on a systematic trade in the procuration of abortion by the use of the most deadly poisons; and that it is the belief of persons well acquainted with the habits of the natives that these women are professional poisoners." Besides these there is a class of thieves who eke out their other atrocities by occasional recourse to drugging or poisoning. There does not appear, however, to be any organized system of road poisoners in Bengal, although two or three may sometimes act in concert. In the north-west provinces there appears to be a class of miscreants who drug their victims by employing children as cats'-paws, and several illustrative cases are given by our author; while there is another class whose occupation lies in administering drugs to women of the town, and then robbing them of their ornaments, in which these women generally invest their gains. After some acquaintance a drinking bout is proposed, to which no objection is ever made, and as the victim becomes intoxicated, the drug (generally *dhatoora*) is mixed with the liquor, and whilst she is insensible her property is carried off. In one of the cases quoted the woman became insensible in the evening, and was unconscious till 3 p.m. next day; in another she was insensible for two days.

In Scind poisoning seems to have been a regular profession not many years ago. In 1856 'The Scindian' reports that in Upper Scind "a gang of notorious characters have just been unkennelled, who have been in the habit of disguising themselves as fakeers and administering poison to people possessed of wealth, which they appropriated to themselves after they had succeeded in putting them to death. Some time back Government offered a reward of one thousand rupees for the discovery of the guilty party, and this seems to have had the wished-for result, as the whole gang have been apprehended."

Dr. Chevers has, in the present edition, collected a large mass of facts on this form of thuggee since 1856. Dhatoora is, he believes, the poison usually employed. It is given in various ways, one of the most common being to mix it in flour, or to give it with bread itself, and great ingenuity is often shown in the manner in which the poisoner manages this.

"Dr. Irving speaks of many ways of giving the seeds, as mixed whole, with *brinjal* (*Solanum melongena*), between the seeds of which and those of the drug the victim does not observe any difference, and pounded in flour, goor, dhall, rice, milk, infused in spirits and in tea, and made up in sweetmeats. The leaves are also given as *sag* (spinach). These miscreants also use arsenic, opium, hemp (bhang), aconite, and oleander (kunere).

"In 1868, the 'Police Gazette, N. W. P.,' published the confession of one Ramadheen, not quite twenty-one years of age. who, for the previous twenty months, has followed the calling of poisoner. A local writer says, 'There is no nonsense about Ramadheen; he does not pretend to scruples or remorse of any kind. He calls his victims "shikar" (game), and alleges no other excuse for his practices than that it was very dull at home in his village. So far as we can enumerate the persons he poisoned in the year and a half, they are about twenty-seven; but he is very cavalier and careless in figures, and talks of a family whom he may murder with a lordly negligence as to the number of its members. Ramadheen is not in the least superstitious. Most of his victims were either brahmins or fakeers, and his favourite hunting grounds were what he calls "holy places"—Bindachull, near Mirzapore, and the Megh Mela here'" (p. 175).

Many curious details of "poisoning for plunder" have been collected from reports, relating not only to the north-western provinces, where Dr. Irving has specially considered the subject, but to Bombay, Madras, and the lower provinces.

There seems reason to believe that since the more murderous form of thuggee by strangulation has been suppressed, cases of drugging for the purpose of robbery have increased.

A judicial officer, up-country, obtained about four years ago a box containing "samples of different preparations of datura employed by the professional poisoners of Upper India for the purpose of robbery," and it contained the six following articles:—(1) datura seeds; (2) powdered prepared datura seeds, parched, but not cleaned or fit to mix with food to drug a victim; (3) ditto, fit to mix with food—dose, half a tolah; (4) distilled essence of datura, used with tobacco, sugar, attah, &c.—dose, ten drops to a chillum, or a quarter teaspoonful in a meal of attah; (5) attah drugged with datura flour; and (6) suttoo similarly drugged.

The chapter on "Thuggee by Poison" is followed by one on *Datura* generally, in which the author, after quoting the early historical references to poisoning by this drug, and describing the three species which are used, viz. *D. fastuosa* or purple flowered, *D. alba* or white flowered, and *D. ferox*, discusses the botanical characters of the seeds, their physiological action, the means of testing chemically for them, poisoning by datura leaves, drugging or slow poisoning by datura, datura a cause of insanity, symptoms of datura poisoning, the physiological differences in the signs of poisoning by datura and aconite, &c. There are many of these topics to which we should gladly refer if space permitted, but we must confine our remarks to a few of the most important.

The following is a good, although very brief, *résumé* of the symptoms consequent on eating the cakes known as chupatties, when the flour had been mixed with coarsely grown brownish-yellow particles of the datura seeds:

"On the 24th January, 1866, two patients were brought to the Chupra Dispensary suffering from the following symptoms:—Chattering delirium, with a tendency to perform strange antics; they were stupid and unintelligible; their pupils were widely dilated; the skin natural, pulse quick and small, tongue white and moist. They both recovered under appropriate treatment. On subsequent inquiry it was found that these curious symptoms supervened rapidly after eating chupatties made of flour, some of which remained."

This flour, on microscopical examination, showed distinctly the presence of a peculiar structure characteristic of the exosperm of the seed coverings, which is fully described in p. 187 of this work.

When the seeds have been so well ground that the microscopic test fails, we must extract from the vomited matters, or the mass found in the stomach if death ensues, the alkaloid, and apply the physiological test. The essence containing the alkaloid must be dissolved in a little water, and injected into the stomach of a puppy or kitten, if the animal will not take it mixed with the food. In September, 1866, one half of the extract of the contents of the stomach of a woman who destroyed herself by this poison was given to a kitten at noon, and occasioned the following symptoms:

"The little cat soon began to breathe with difficulty and froth at the mouth; in ten minutes her pupils were dilated, and they continued to remain so, only to a still greater extent, the rest of the day, never for a moment being contracted or even less dilated when exposed to a strong sunlight. After twenty minutes she was placed in the middle of the room and encouraged to walk, but she staggered and fell on attempting to do so. In half an hour from the time of administration she was quite unconscious; up to this period she had felt pain when pinched with a forceps, but now a severe pinch only caused a slight movement of the limb without any expression of pain. The respiration was still laboured, she continued to froth at the mouth, and the pupils remained very widely dilated. Consciousness began to return at two o'clock; she then got up, sat staring widely, and commenced to perform a series of grotesque actions, uttering a low moan from time to time. When pinched, she felt pain, but not very acutely. She appeared very irritable, almost wild, but was neither vicious nor bad tempered. At 3 p.m. the pupils were dilated extremely, the iris being a mere thread. By 4 p.m. she had recovered so far as to be able to come when called, and to feel acute pain when pinched, the pupils continuing as large as ever.

"A small portion of the same extract or essence was applied to the eye of another kitten; it caused the pupil to dilate in half an hour. Another portion of the same was applied to the experimentor's lips without producing any numbness" (p. 190).

Dr. Chevers tells us that when he first published his manual "there existed among the missionaries in this country a very strong conviction that *datura* was frequently administered by their relatives to natives who evinced an inclination to embrace Christianity. In 1856 I was authorised to quote the following passage from a note lately written by an eminent and experienced missionary, then residing in Calcutta, but now deceased, in reply to queries on this subject:

'I always understood that the drug administered in the cases referred to was the *dhatoora* in small dose. The symptoms have been heavy dull eyes, with a prostration of mind, rendering the victim an idiot, and *looking* very much like one, with a listless heavy countenance, indicating that the brain has somehow or other been affected. I believe the victim can be for months in that listless, heavy, dreamy state; but gradually under a proper treatment and a change of scenery I always heard recovery was possible. There is no mistaking the symptoms of the poison, as it transforms the victim in a *short time* into a *totally different being* from what he was in his normal state'" (p. 204).

Dr. David B. Smith gives notes (quoted by our author) of the case of Mr. ——, æt. 42, a European of very regular and temperate habits, residing at Mussoorie, the subject of "irritative" or "phosphatic dyspepsia," but not otherwise diseased, who was nearly drugged to death by datura. On the morning of the 2nd November Mr. —— was ill and in a confused state. On the 5th the following facts were elicited:—"Went to bed on evening of 1st November perfectly well. Got up at usual hour in morning with a sensation of extraordinary giddiness and of rolling motion. In getting up to bathe found himself swaying from side to side. All the limbs felt perfectly powerless, and also the tongue. Could not speak properly. No pain in head or spine; no sickness of stomach. Could not see at all to read or write. Could see large objects, but not small ones, such as letters. Any one approaching him seemed to have a white muslin net over him. Face was puffy under the eyelids. Tongue moist-looking, but he complained of dryness of mouth and throat." On the evening of the 6th word was sent to Dr. Smith that Mr. —— was delirious. On visiting him, however, he was asleep. On being roused his pulse was steady, and he answered some questions. His pupils were broadly dilated, and he was evidently drowsy. "Still," says Dr. Smith, "I had no suspicion of his having been unfairly dealt with." On the morning of the 7th Dr. Smith received a note from Mr. ——. It appears that the beginning of it was steady, legible, and sensible, whilst the conclusion of it was rambling, confused, and almost illegible. Dr. Smith found him in a very peculiar state. Face somewhat purplish, eyes bright, the pupils greatly dilated and insensible to light. Pulse tolerably natural. He wandered about in a confused state, arching his eyebrows, rubbing his hands, and complaining of cold and numbness down the right side of his body. He went from room to room, and showed an inclination to wander outside. His daughter led him about, and prevented him from going out of doors.

He spoke incoherent nonsense. Looking out he suddenly exclaimed, with a placid but startled expression, "See, doctor, there is snow on the ground." On being told "It is sunlight you see," he replied, with an air of confusion and disappointment, "Oh! sunlight is it? I thought it was snow;" and immediately he rambled incoherently about other matters. He then wandered into a room where Dr. Smith was writing a prescription; came up to the writing-table and began touching various things without any definite object. He looked towards the pigeon-holes, where he had private papers, and stumbled in their direction, but took nothing out. His gait was peculiar, and he walked in a sort of stealthy manner, mumbling to himself. He appeared in a feeble and pitiable state. He was not the least violent.

Dr. Smith perceived that this condition was very peculiar. He apprehended a paralytic attack, but he did not yet suspect *foul play*.

On this day (7th) Dr. Smith heard that one of Mr. ——'s servants had expressed a suspicion that his master had been poisoned; Dr. Smith replied that, if so, the poison was probably *dhatoora*. On the 8th he was found lying perfectly sensible, but weak, and still somehat confused and unlike himself. On the 10th he wrote, still complaining of "dryness of the mouth and throat." On the 13th he went to Dr. Smith's house, and talked the whole matter over. He then, for the first time, said that he felt convinced that he had been poisoned. He believed there were four occasions on which he had reason to suspect that poison had been administered to him. He could not recall dates, but came to the final conclusion that the first occasion was on the evening of the 1st, the second on the evening of the 6th, the third on the morning of the 7th, and regarding the fourth he was uncertain. Shortly after the meals on the 6th and 7th he lost all recollection of what happened round him. Meanwhile, however, he experienced a feeling of intoxication and giddiness, difficulty in swallowing, confusion of ideas, a coldness and numbness of the surface, a pricking sensation in the nose, and an irresistible inclination to rub it violently. He had also convulsive twitchings of the legs after dinner on the 6th. He had no fever and no vomiting, but considerable drowsiness. In addition to the symptoms noticed above, there were frequent coughing; attempts to hawk and spit; haziness and confuson of objects, as if everything were badly focussed; a sensation as if smoke or fog were rising around him. The moment he touched any object he went off in a purposeless manner to some other object at a distance. As he did this, he was led and supported by his daughter, and looked the picture of feeble nervous agitation. After recovering to a certain degree, he still evinced a partially incoherent mental state; his vision continued indistinct, the eyes were bright and glistening, and the pupils continued to be widely dilated. He also experienced a sense of very considerable exhaustion, walked about feebly, and was altogether sadly unlike himself.

Hence Dr. Smith concludes, that he was powerfully under the influence of *dhatoora*. Mr. —— appears to have had considerable causes of mental distress. Dr. Smith adds that, taking it for granted that an excessive quantity of *dhatoora* was administered in this case, it is not easy to determine with what specific object it was given, whether to kill at once, or to effect the same end by slow poisining, or whether it was intended, by degrees, to stupefy and weaken the intellect. It is to be observed that the natives fully believe in the possibility of rendering a person fatuous by such means.

Reporting the case a year subsequently, Dr. Smith adds that his patient was then "perfectly well and happy."

This case, which from its peculiarity we have described at considerable length, seems to have occurred in 1867, and, especially if taken in association with another also given by Dr. Chevers, that occurred in 1865, to which a European railway inspector at Jubbulpore was the victim, tends to show that a condition resembling dementia may be induced and kept up for a considerable time.

This gentleman saw his servant squeezing the juice of a pounded *dhatoora* fruit into a stew that was being prepared for his supper. Stating that he was not hungry, he told his kitmutgar to put it by for breakfast, and intended taking it the next morning to the doctor. In the morning, however, his breakfast was obviously poisoned by the same drug, for on his ride to the doctor's he was obliged in consequence of giddiness to stop at the house of a friend, who could not make out what was the matter, as he was talking incoherently, going reeling about the room, and every now and then squeezing and twisting his coat tails, and looking about the room as if to illustrate his meaning. Having taken only a single dose, he recovered on the following day.

The next poison described by Dr. Chevers is *Cannabis sativa*, known also as Indian hemp, bhang, ganjar, &c. We learn for the first time that this drug may cause aphasia.

In 1860 Dr. James Wise, the present civil surgeon of Dacca, wrote to the author from Muttra, that he had lately met with several cases in which *complete loss of speech, not of voice*, followed intoxication from hemp, in confirmed smokers of that drug. One of these occurred in a kidmutgar, who had been addicted to this vice for years. Having been deprived of it for some time while out in the district, he recommenced smoking it on his return to the station. One morning he was found lying insensible, with a cold, clammy skin, breathing slowly. He recovered from this after emetics and purgatives had been given, but his speech was lost. He understood what was said, and made vain attempts to speak. He continued so, at least up to the time at which Dr. Wise wrote to Dr. Chevers, five weeks subsequently, well in physical health, but dumb. Another case was that of a syce. He went to the maidan one morning; while returning he fell down. When picked up he was dumb, and could not walk. He had a vacant stare; had no signs of paralysis, and was otherwise in good health. After strong purgatives and blisters to the nape of the neck he recovered his speech and was discharged to duty.

Dr. Chevers remarks that "several narcotic poisons, and especially datura, interfere greatly with articulation, even to the production of complete aphonia; but considering the lamentable frequency of the vice of ganjar-smoking, it is remarkable that, in my own practice, I have only met with one case similar to those described by Dr. Wise. Possibly hemp only produces this effect in persons whose brains are prone to those neurolytic conditions which lead to aphasia."

"Dr. E. C. Bensley has noticed that very moderate doses of tincture of cannabis are liable to cause rather serious symptoms in delicate women in India—fainting followed by torpor and collapse, pallor, coldness of the surface, exceedingly weak pulse, and dilatation of the pupils" (p. 224).

In the chapter on *Opium* there is less original matter than in those on arsenic, aconite, and datura. This drug is largely used to destroy the female children of the East, the mother either giving the infant a small pill or rubbing her own nipples with opium before putting it to the breast. European children are not unfrequently drugged with opium by their nurses. A physician, whose name is given, when at Dum-Dum found a piece of opium as big as the end of the little finger in his infant's mouth.

Under the title of *Poisonous Fungi* we have a remarkable case recorded by Dr. C. Palmer, of Calcutta, which occurred in 1853, and, as far as we know, is unique. The action of these unknown mushrooms resembles that of the well-known *Amanita muscaria.* Mr. C. S—, an accomplished civilian of very temperate habits, was ordered out of the Collector's Court at 11 a.m., in consequence of being apparently drunk. On the following morning he was seen by Dr. Palmer, who reports as follows:

"I found him in a state of great depression and distress, from the recollection of the occurrences of the preceding day. He informed me he had had his breakfast as usual, had a small bottle of claret which he always took, and was not aware of having partaken of anything unusual, and certainly not to excess. That he went to Cutcherry at the usual hour, and felt unable to control his actions; felt drunk; everything and person appeared ludicrous; he laughed immoderately in open court, joked with the Omlahs, and ridiculed in an absurd way his superior officer, the collector, by whom he was taken to his house. After some hours he recovered, considerably depressed from the effects of the stimulant, and from the feeling of shame at having made such a ridiculous spectacle of himself before his court, but more so, as he assured me, that this was the third time a similar attack had seized him, and he feared he would go mad, for he could not with the utmost effort control his actions, and had been seized in precisely the same manner at the same time of the year three years in succession" (p. 281).

Dr. Palmer, to console him, promised to return to tiffen, at which, amongst other dishes, were stewed mushrooms. Before the meal was finished he became very excited, and, as he had taken very little beer and no other stimulating drink, it suddenly flashed across his mind that the mushrooms were the cause both of his own and of his friend's intoxication. His symptoms were identical to those described by Mr. S——, and were very similar to the exhilaratory effects of alcohol; every person appeared ridiculous, the most ordinary remark was full of fun and wit, and his immoderate laughter provoked equal merriment in others. "I took (he writes) a drive in the evening, and I never before or since have seen the lights and shades cast by the setting sun so brilliant, and every object looking so perfectly beautiful. These exaggerated sensations continued for some hours; until, at the request of my friends, I put an end to them by taking a full dose of ipecacuanha, and thus got rid of a considerable quantity of the fungi still undigested. I experienced no after ill effects whatever."

Amongst *poisons acting mechanically,* Dr. Chevers notices powdered glass, diamond dust, and finely chopped human hair which, when administered in curry or other soft food, is a recognised mode of slow poisoning in Singapore, especially by women intent upon destroying their husbands.

A remarkable, and we believe unique, case of poisoning by chloride of cadmium, that occurred in the Calcutta Native Hospital, is given at p. 297. From experiments made in relation to this case, it appeared that fifteen grains would kill a cat in less than five minutes.

The toxicological part of this valuable work, for which we offer our hearty thanks to Dr. Chevers, concludes with a chapter on the poisonous grains and legumes occurring in India, some of which give rise to very singular forms of disease.

V.—Physical Degeneracy in the United States.[1]

The question of physical degeneracy has for the last ten years occupied so much of the attention of physiologists, of statists, and even of politicians, that we make no excuse for bringing a subject which has so many social bearings before the notice of our readers. As Dr. Arthur Mitchell, the accomplished Deputy Commissioner for Lunacy in Scotland, has observed—

"If we carefully study the literature of the subject, we find that it abounds in unsupported assertions, and that important conclusions

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[1] 1. *Physical Degeneracy.* By Nathan Allen, Esq., M.D. New York, 1870.

2. *The Intermarriage of Relations.* By Nathan Allen, Esq., M.D. New York, 1869.

3. *Population; its Law of Increase.* By Nathan Allen, Esq., M.D. Lowell, Mass., 1870.

4. *The Physiological Laws of Human Increase.* By Nathan Allen, Esq., M.D. Philadelphia, 1870.

5. *On Genealogy in Connexion with Anthropology.* By G. M. Marshall, L.L.M. London, 1865.

6. *Blood-Relationship in Marriage considered in its influence upon the Offspring.* By Arthur Mitchell, M.A., M.D. London, 1866.

7. *Inquiry into Consanguineous Marriages and Pure Races.* By Dr. E. Dally. Paris, 1863. Translated by H. J. C. Beavan, F.R.G.S., 'Anthropological Review,' May, 1864.

8. *Sur les Dangers des Unions Consanguines.* Par Dr. Boudin. Paris, 1863.

9. *Un Mot sur les Mariages Consanguins.* Par Dr. E. Dally. Paris, 1863.

10. *On the Stature and Bulk of Man in the British Isles.* By John Beddoe, M.D. London, 1870.

11. *Influence of the Climate of North America on the Physical and Psychical Constitution.* By E. Desor. 'Centralblatt für Naturgeschichte und Anthropologie,' 1863.

12. *Hybridity in the Genus Homo.* By Dr. Paul Broca. Translated by C. Carter Blake, Doct. Sci. London, 1864.

are very often made to rest on a basis which is undefended and clearly too narrow."

The powerful advocacy of Dr. Dally in favour, and of the late Dr. Boudin against, the prevailing doctrine that consanguineous marriages are productive of physical degeneracy, has had, across the Atlantic, fresh attention called to it by Dr. Nathan Allen. It will be our duty to consider carefully some of the conclusions to which this author has been led, in order that they may be compared with those of investigations in the Old World. The author confines his investigators chiefly to persons having their origin and nativity in New England, and among whom certain physical changes have been observed.

It is first necessary to eliminate all changes which are presumed to have taken place from what M. de Quatrefages terms the *influence du milieu.*

"Among these external agents may be mentioned the effect of changes in private and public institutions, in the style of dress and state of society, in the kinds and modes of doing business, in the changes of soil, of vegetation, of air, of dwellings, in methods of education, habits of domestic life, &c., &c.; but then many of these external agencies cannot be considered separately from the internal, which may be summed up under three general heads, viz. exercise in all its diversified forms; food, including drinks, medicine, and whatever enters into the system; and the last, though by no means the least, the effects growing out of the laws of hereditary descent."

Such changes are effected either by natural laws of growth, by the violation of natural laws, by disease, or by the operation of the inexorable rules of hereditary descent. Dr. Nathan Allen assumes that man in the beginning was created perfect, *i.e.* free from disease; and we think that both the perfectionists and derivationists will grant him this premiss. For on the hypothesis of the separate creation of man it is difficult to conceive an individual in whom the tissues were imperfect; and on the theory of his origin from an inferior form, the high degree of athletism which it is necessary to presuppose that the first simioïd possessed would have been obviously incompatible with the view of physical weakness or disease. It is therefore convenient to assume this proposition.

Dr. Nathan Allen quotes at great length the investigations of Dr. B. A. Gould, under the direction of the Sanitary Commission, into the physical characters of over a million of soldiers. In the old world, Quêtelet and many authorities have satisfactorily proved that the maximum stature of man is reached at the age of twenty-five. It was shown by Dr. Gould, on the other hand, that it was not till thirty years of age that the typical American reached his maximum height. The backwoodsmen of Tennessee and Kentucky were, as might be expected, the tallest in stature, far exceeding those from the manufacturing states. Dr. Allen considers that this excess is due to the exercise of certain muscles and bones, while in a state of growth and under favorable circumstances. Whilst the American soldiers exceeded in stature those of other nations measured, they were far inferior to the Irish and Germans in weight, strength, girth, and certain other properties, and, in fact, may be said to have "run to stalk."

The results of the measurements have only confirmed the induction which has been already thoroughly established, that pure races are always superior to mixed races in their physical stamina. In all these investigations the volunteers were carefully omitted; for in America, as well as elsewhere, they are composed, on the average, of more healthy and better-fed men than the members of the regular army. Dr. Allen believes that among the American race at the present time a gradual loss of muscle and a rapid increase of the nervous temperament are observable. This is not advanced as a mere whim, like the fancy sketch in the amusing work 'The Coming Race,' but appears to be borne out by the most careful research. The increasing migration of the American people from the country to the city is decidedly unfavorable to the physical well-being of the population; and the statistics collected by Dr. Beddoe, respecting the enormous differences which prevail between the urban and country populations, are amply corroborated by Dr. Allen. He furthermore proves that the invention of machines, whereby physical labour is replaced, has tended enormously to depreciate the strength of the population. Fifty or a hundred years ago, the exigencies of the settlers required an enormous amount of hand-work to be done in clearing the forests, in tilling the ground, and in the erection of dwellings, but this work once accomplished, within the area, relieves the following generations from the like necessity for labour, and the result is a visible deterioration of the physique. Desor has well shown this in an argument of which we extract the most striking points:

"When a German or Swiss emigrant arrives in New York, the climate appears to him much the same as that of his native country. But if he takes up his residence in that country, he soon finds it necessary to change his mode of life and habits.

"It is about two hundred and thirty years since the first colonists arrived in New England. They were all true Englishmen, endowed with all the characters of the Anglo-Saxon race.

"Another chief characteristic of the American is the length of the neck; not that it is absolutely longer than amongst us, but appears longer on account of leanness. The Americans, again, soon recognise the European by the opposite characters. 'He is a stranger; look at his neck, an American has no such neck.'"

The physical difference between the American and European is not only manifest in the muscular, but also in the glandular system; a matter that especially deserves the attention of the physiologist, as it concerns the future of the American race. The most intelligent Americans perceive that the increasing delicacy of form (especially in the women) ought, if possible, to be arrested. Despite of their instinctive aversion to the Irish (forming the largest contingent of immigrants), they are aware that the development of the glandular system of that race is well calculated to neutralise the influences of the climate for a considerable time. It has been observed that the finest women are descended from European parents.

There are few Europeans who get fat in the United States; the Americans, on the contrary, who reside for a considerable time in Europe, become more healthy and portly. This occurs, also, to the European when, after a lengthened stay in America, he returns to Europe. The author (Desor) quotes himself as an example of the kind. We look in vain among American children, despite of all the care taken by their mothers, for curly-headed children, so frequently seen in England. The hair of the European becomes, in America, drier, and requires pomatum, &c., to keep it glossy and soft. The want of depth in the voice of Americans is also ascribed to the influence of climate. Every European who arrives at New York, Boston, or Baltimore, will be struck with that feverish activity the American displays. Every one is in a hurry; the people do not walk—they run. Something like it is, no doubt, seen in the large commercial towns of England; but the activity of the Englishman seems more under the control of reason; that of the Yankee is instinctive, or at any rate the result of habit or of an innate restlessness. They even exhibit this accelerated activity during their meals, which, even if they have nothing important to do, are despatched in equally short time.

Dr. Allen is also of opinion that the use of spirituous liquors is more destructive in America than in our climate, and in evidence quotes the fact that Europeans who are accustomed to strong drinks in their own country have either to renounce their use or to limit the quantity in America, to avoid their ill consequences.

At this time the characteristics of pure English breed are no longer seen among the inhabitants of the United States, but have been replaced by a new type. This type is not the product of intermixture, since it is seen in the most marked form in the eastern states, where the race is less mixed; but it must be attributed to external influences. One of the first physiological characters of this American type is an absence of corpulence.

On perambulating the streets of New York, Boston, Philadelphia, and other towns, scarcely one portly individual will be encountered among a hundred persons, and the one met with will frequently turn out to be a foreigner.

In the time of the pilgrim fathers of New England there was relatively much more acute disease, far greater violence in its attacks, and a decidedly higher grade of inflammation, than exist at the present day. Venesection and cathartics were, according to Dr. Allen, not only administered but required. Since that time scrofula, general debility, dyspepsia, anæmia, and neuralgia, which formerly used to be extremely rare, are now very common.

An analysis of the death-rate of Chicago from 1852 to 1868, during which time the population had increased from 50,000 to 250,000, led Dr. S. W. Mitchell to conclude that the diseases classed as strictly nervous had increased threefold. In Massachusetts, likewise, there has been a great increase of diseases of the brain and nervous system; and, generally speaking, cases of inflammation and congestion of the brain, of apoplexy, of paralysis, epilepsy, and convulsions, are far more common now than formerly; and the two former, instead of being confined to the aged, from sixty to eighty, frequently occur among those from forty to sixty years old.

The loss of muscle and increased nervous excitability, and the change in the type and character of diseases, are conditions much more pronounced among women than men. Dr. Allen considers that, in the case of females, the time bestowed upon what passes in America for school education would be better spent in housework. This appreciation of the value of female education in America may well be disputed, and we believe that Dr. Allen must seek for some deeper cause to account for results he deplores among American women.

The same writer considers that the increased use of tonics, especially the preparations of iron, in the case of females particularly, is evidence also of physical debility; but he would have established his case better had he given precise statistics to show the augmentation of diseases, instead of quoting the increase in the quantity of certain remedies employed.

The amount of uterine diseases, due in great part to customs nearly peculiar to the United States, are mentioned by Dr. Allen as further strong evidence of decline in the physical status of the American people. The deficiency in quantity, and still more the poor quality, of the mammary secretion is assigned as a cause of the small number of the American children reared and healthily developed.

Dr. Allen, who adopts an obsolete classification of temperaments, considers that the lymphatic and sanguine temperaments are not properly developed in the American race. There is a marked deficiency in the organs of nutrition and secretion, and, in connection with these facts, it is to be borne in mind that all imperfect developments, morbid weaknesses, or strong predispositions, are transmitted in an intensified form to successive generations, unless other opposing favorable conditions intervene.

The question of longevity in the American population also affords elements of criticism. In the history of nearly all nations there will be found a slight excess of births of males over females, to provide for the greater exposure and liability to sudden death in the former. Professor Loomis, after a comparison of New York and the New England states, proves that in the census ot 1860 there were in these seven states 850,000 boys and 830,000 girls under ten years of age. At the age of twenty there were 15,000 females in excess of the males, and at the age of thirty there were 75,000 more females than males. Now, making proper allowance for emigration, these figures follow very closely the law of life, as seen in the following six states—England, Scotland, Ireland, Belgium, Norway, and Sweden; but at the age of forty, instead of this excess of the number of females continuing in America as it does in those six European nations, we find that these 75,000 females have all disappeared, with 2000 more, and, at the age of fifty, 20,000 in addition have followed them. Thus, 97,000 females have passed away, as they would not have done had they lived in those other countries named. The census fails to give statistics of the health of a community, but it is well known that when vitality is so depressed as to cause such a fearful increase of deaths, results are more perceptibly seen among the living.

The descriptions and portraits of the Puritan women of past times represent them for successive generations as possessing well-developed bodies, in many instances of large size. Even the last generation of New Englanders are larger and stronger than the present race.

With regard to other unfavorable conditions of life enumerated by Dr. Allen, these may chiefly be summed up:—in undue hurry and excitement in all the affairs of life, intemperance in eating, drinking, and smoking, and in general disregard of the true laws of health. To these may be added the enormous consumption of patent medicines, the hasty manner of eating, the practice of eating an excess of fine-flour bread, the abuse of tea and coffee, the increased use of a rich, highly-seasoned, and stimulating diet, and the constant employment of iced water. The dental records of America show that to the last cause especially may be attributed the excessive prevalence of diseases of the teeth; at the same time no inconsiderable influence must likewise follow from the frequency of the scrofulous diathesis.

On the subject of alcoholic drinks, tobacco, and opiates, Dr. Allen repeats the same arguments, if they can be so called, which have been urged, *usque ad nauseam*, by numerous writers in the old world. That excessive drinking and smoking are injurious is a truism; but that, on the whole, Americans drink or smoke more, or that there is a greater relative amount of narcotics consumed in America than in Europe, are assumptions which up to the present time do not rest upon well-ascertained and recorded facts, and in dealing with so important a question something more might justly have been required of Dr. Allen than mere unsupported assertions. Such might have been obtained from the statistics of liquors, tobacco, and opiates, used for home consumption, or from criminal statistics. That a large consumption of corn spirit takes place in America there cannot be the slightest doubt, but before we bring the charge of drunkenness against the inhabitants we must not forget that the amount of beer and wine consumed by them is comparatively small.

The most serious matter, however, which Dr. Allen adduces in proof of degenerescence among his countrymen is based upon more tangible ground, resting on the undoubted fact that the birth-rate is continually diminishing. The registration returns of births, deaths, and marriages have now been made in Massachusetts for over twenty-five years; and no fact from these returns, as well as from other sources, is more apparent than that the birth-rate of the American people has been constantly diminishing. From 1850 to 1860 the average birth-rate, by these returns, was 1 to 33, omitting the fraction; but during the five years of war the average was 1 to 39, and since the war the average has been 1 to 36. In 1850 the census returns the population of Massachusetts 994,514; American, 830,066, and foreign 164,448; the registration reports make the whole births 27,664; American, 16,189, foreign, 8197, with 3278 mixed, most of which are of foreign descent. The last census, 1865, returns the whole population in the same state, 1,267,003; American, 1,002,545, and foreigners, 265,486. The registration report makes, for the same year, the whole number of births 30,249: American, 13,276; foreign, 14,130; and mixed, 2406.

In the census column headed "American," a large number of persons are included simply because born there, whose parentage is strictly foreign; and so in the registration reports, under the column "Births, American," there are enrolled some of foreign descent. Hence it is very difficult to obtain separately the exact birth-rate of the two classes.

After making the best attempts we can to analyse these returns, and to compare the births of the two classes since the war, we find the birth-rate of the foreign (that of the whole averaging 1 to 36 for several years) below 1 to 30; whilst that of the American ranges considerably above 1 in 40, and, perhaps, extends nearly to 1 in 50.

In the history of European nations it has been found that, in order for any nation to increase much in population, the birth-rate must reach about 1 in 30. The last United States' census report gives the birth-rate of the following nations thus, omitting fractions:—Saxony, 25; Prussia, 26; Austria, 26; Sardinia, 27; Bavaria, 29; Netherlands, 30; England, 30; Norway, 31; Denmark, 32; Sweden, 32; Hanover, 32; Belgium, 34; France, 37.

The foregoing are the principal facts which appear to show beyond a doubt that the American race is suffering from a process of physical degeneration. Whether the causes which Dr. Allen has assigned may not fairly be pronounced inadequate for the most part, we will not discuss. Many of them contain their own answer so entirely, are so inconsistent and so little supported by statistics, that they are unworthy of serious criticism. The late Dr. Knox remarked, "Already the United States man differs in appearance from the American; and America will still require English blood to keep up its people, and then be a kind of European settlement,"

The mixture, for example, of the Chinese, Irish, Negro, Mohawk, Dutch, German, Spanish, French, and English races in the *synthesis* which is called the American population contains too many heterogeneous elements to possess perfect vitality. Dr. Paul Broca, in his researches on hybridity, has very well pointed out the limits within which it may be said that "mixed breeds die out;" and whether we consider the complicated *métis* which exist between the various members of the human family, especially in the Southern States and in Central America, the rule becomes perfectly prominent, that in the same ratio as the breed is complex, or as it is derived from ancestors far or nearly related, so the infertility can be predicted.

The consideration of this loss of human hybridity appears to be at the foundation of the whole question. Hybrid races have been divided by Paul Broca into Agenesic, Dysgenesic, Paragenesic and Eugenesic. In the first case, mongrels of the first generation are entirely infertile, either between each other or with the two parent species, and are, consequently, unable to produce either direct descendants or mongrels of the second generation. The dog and cat, and most allied genera of animals, may be cited as examples. In the second or dysgenetic class, the mongrels of the first generation are nearly altogether sterile, as in the mule between the horse and ass. The third or paragenetic class are mongrels of the first generation, which have a partial fecundity. They are sometimes scarcely fertile or infertile *inter se*, and when they produce direct descendants these have merely a decreasing fertility tending, to necessary extinction at the end of some generations. Sometimes, however, they breed easily with one at least of the two parent species. The mongrels of the second generation issued from this second breeding are themselves and their descendants fertile *inter se* and with the mongrels of the first generation, with the nearest allied pure species, and with the intermediate mongrels arising from these various crossings. The various human mixed breeds of Central America may be taken as examples. In the fourth or eugenesic division mongrels of the first generation are entirely fertile. They are fertile *inter se*, and their descendants are equally so. They breed easily and indiscriminately with the two parent species; the mongrels of the second generation in their turn are themselves and their descendants indefinitely fertile, both *inter se* or with the mongrels of all kinds which result from the mixture of the two parent species.

Such being the laws, it is easy to see that the mixed character of the races which inhabit the United States has induced an amount of cross-breeding which naturally tends to terminate in the extinction of the mixed progeny. The researches of Count Oscar Reichenbach have shown that the coloured race in the United States, leaving the pure blacks out of consideration, are rapidly becoming extinct. Emancipation has caused a decrease of coloured population in the Northern States, for there is no natural increase of the coloured population in those states, and there are no emigrants, manumitted and fugitive, augmenting them. On the contrary, some have migrated to the South, where climate, economical conditions and society, are somewhat more congenial. In fifty years hardly any mulattos or quadrovers will be found in the present Northern States, and over the whole extent of the country their numbers would probably not amount to more than 9,000,000, a number more likely to decrease than increase from that time forward, from causes still more powerful than those which operate on the transmutation of people in Ireland. These facts are of the very highest importance to those who take an interest in the future of the negro race in America. One of the evils which has been frequently pointed out as incidental to the coloured population is the great mortality amongst the children of the African race as seen in Virginia. It has been attempted to show that this was peculiar to the negroes in the Slave States; but it is now clearly demonstrated that the mortality was far greatei in the Northern States, where there is absolutely no increase at all, while in the former Slave States the negroes increased twenty per cent. The real cause of this mortality will not be found in the political institutions of slavery or freedom, nor in the social incidents of work or starvation, but in climate. And the great mortality observable in negroes, and to a still greater extent in the mixed breeds of the Northern States, appears due to pulmonary diseases alone, the result of those abnormal climates to which they are exposed. In Maine the decrease between 1850 and 1860 was 2·14 per cent., in Vermont 1·25 per cent., in New Hampshire 5 per cent., and in New York 0·13 per cent; whilst, on the other hand, in South Carolina the increase was 10·63, in Missouri 36·44, and in Alabama 19. The fortieth degree of north latitude is, perhaps, higher than any negro can exist in in a normal condition. It was absurd to talk of Virginia as a "breeding state," for that state was never so well suited to the negro constitution as the states further south. Nature has thus gradually done the work for which the advocates of emancipation fought so long. Where the white man can labour there is no chance for the negro and still less for the coloured population. It is a sufficient fact that the inexorable and changeless fiat of the natural law of hybridity has done more to extirpate the mulatto, races than all the blood and treasure which was wasted in the American war. Whilst armies were fighting and countries were being devastated, the *teterrima causa belli* has nearly become extinct, and a very few more years must mark the termination of the unfortunate race.

The fact that no distinct hybrid race of men can be found to exist anywhere in the old or new world (the Griquas of the Cape of Good Hope having been long since dismissed to what Professor Owen calls "the limbo of all hasty blunders"), must impress students of physical degeneracy with the real laws which influence the apparent diminution of the vitality in the American races. When we turn to the old world, we see with what strength, vigour, and vitality, the pure races are endowed, who through centuries have kept apart from the surrounding population. The very existence of such a race *e. g.* as the Basques in North Eastern Spain and South Western France, is a stumbling-block to the student who imagine that races have their cycles of greatness or decay. The purest races in the old world, unmixed with any other blood, appear, so far as our time or our history can estimate, to be eternal and unvariable. As the late Robert Knox said—

"The sterility of hybrids is the check which nature employs for the preservation of her primitive forms of life. There is a consanguinity, no doubt, in all that lives; for life, being a property inherent in matter, must at its origin have been one, but this consanguinity does not extend to, or include, specialities. It goes no further than genera, and most commonly not so far."

In 1846, when Knox applied these biological doctrines to political events, much contempt was poured upon him, yet events in Italy, Austria, and America, have shown the supremacy of simple physiological facts connected with the viability of the species over political theories. In 1872 we are able to estimate the value to be placed upon generalizations like those of Knox, Nott, Gladdon, Broca, Pouchet, or Quatrefages. When considering the diminution of births amongst the coloured populations of America—when watching the peculiar features which mark the mixed population of Central America,[1] features which appear in strong contrast to our own ideas of the proportions of the sexes—we are compelled to admit that we must seek for an explanation of the laws which regulate the extinction of great masses of human beings somewhere else than in the mere speculations of the politician. It remains to be seen whether the mixed progeny of Celt, Teuton, Scandinavian, and Romano-Latin origin found in America is destined to live many generations longer than the mulatto or the quadroon. It is true that the ancestors of the present white races were not so far removed from each other as the negro and the white man; yet even this is a mere question of degree, and it remains to be seen whether or not many more hundred years they will last than the mulatto or the quadroon.

It is attempted by Dr. Nathan Allen and others to show in some degree that the degeneracy of which we have spoken at such length is due to the intermarriage of relations. We consider that in this respect the supporters of M. Dally's theory have entirely failed to make out their case. That consanguineous marriages under certain conditions might produce a feebly viable population is beyond doubt, but whether such a fact obtains universally, or whether consanguinity in all cases is the precursor of scanty and unhealthy progeny, we think that Dr.

[1] In February, 1868, the births in a mestizo population of the hill districts of Nicaragua were 4; the number of deaths from natural causes 188. There was no epidemic. It was impossible to estimate precisely the population, which amounted to about 2000. The women in the district were under fifty in number. Of course this is given as an extreme case. Such a race cannot be expected to increase. In other parts of Central America Mr. Crowe (not a very trustworthy authority) says that the proportion of females to males is "four or five to one."

Allen has not clearly proved. He appears to have written in perfect ignorance of Dr. Arthur Mitchell's large memoir on the subject, as well as of the voluminous controversies which took place before the Société d'Anthropologie de Paris. Dr. Mitchell's paper in the 'Edinburgh Medical Journal' for 1812 is, of course, referred to. Dr. Mitchell arrived at the following results:

"1. It is no law of nature that the offspring is injured by consanguinity in the parentage. 2. That this injury assumes various forms. 3. That in all classes and conditions of society its manifestations are not alike. 4. That the evil appears in some measure under control. 5. That isolated cases or groups of cases may present themselves where, in addition to consanguinity, all the other circumstances are so unfavorable that a confident prediction of much evil would be expected, but where no such evil appears. 6. That where the children seem to escape, the injury may show itself in the grandchildren, so that the defect may be potential where it is not actual. 7. That as regards mental disease, unions between blood relations induce idiocy and imbecility more than they do other forms of insanity; that with reference to Scotland it may be estimated with safety that about nine or ten per cent. of existing idiocy is referable directly to consanguineous marriages. In forming this estimate, the proper deductions were liberally made, so as to avoid an over-estimate."

But the literature of the whole subject is so vast that it will be years before any one can venture upon general conclusions. As the research and knowledge of nearly every writer on the subject is in inverse ratio to his confidence that he has closed the discussion once and for all, the whole subject of consanguinity demands a generation of students possessed of far more knowledge than the present to solve the problem. The fact of the physical degeneracy of sundry mixed populations in the New World is painfully evident to the student of man, and the amount of vitality which remains to them has to be ascertained in the great tontine which regulates the vital statistics in this planet.

VI.—Sir J. Y. Simpson's Selected Obstetrical and Gynæcological Works.[1]

A NEW edition of Simpson's works, long out of print, became almost a necessity when the great obstetrician had been taken

[1] *Selected Obstetrical and Gynæcological Works of* Sir JAMES Y. SIMPSON, Bart., M.D., D.C.L., Late Professor of Midwifery in the University of Edinburgh. Edited by J. WATT BLACK, M.A., M.D. Edinburgh, 1871.

from us, and the first volume, fitly edited by one who was privileged to be his assistant for many years, now lies before us. This is, in the main, a reprint of the articles collected by Priestley and Storer, with some judicious omissions, and with the addition of a portion of his lecture-notes, which, if they serve no other purpose, will remind Simpson's old pupils of the hours passed in the class-room in Edinburgh, where the contents of the "boards" which formed so marked a feature of his tuition were so eagerly scanned. For this reason, for the many memories they evoke, we think that Dr. Black has acted wisely in inserting them; but independently of this sentimental consideration, which can appeal to Edinburgh men alone, the notes are valuable as giving some insight into the clear and logical nature of their author's mind. They form a terse and vigorous synopsis of all that is known, or rather was known at the time of their compilation, of the topics they discuss, and our obstetric teachers would do well to study the method of tuition used by so great a master in their art.

What can we say of the remainder of the book? Here we have the labours, so far as written work went, on which Simpson's vast reputation was founded, and those who did not know him personally, who had not the opportunity of seeing for themselves, as no one who ever came into contact with him could fail to see, what a marvel of enthusiasm and energy he was, will naturally look to these writings for the explanation of a celebrity unparalleled in the history of medicine. All who were honoured with Simpson's acquaintance will at once admit that a knowledge of him gleaned from his works alone must necessarily be a most imperfect one. Indeed, his most remarkable characteristics are not shown in them. His great fault probably was the want of the system and method so essential for success in literary work. Had he added these qualities to his genius, he would probably have left behind a still larger and more enduring reputation. But, even as it is, the stranger will find no want of material for study, and will experience no difficulty in understanding why the author of these papers had gained for himself such widespread fame.

A mere glance at the index will show how vast was the field over which Simpson's researches extended. The quality of his mind was peculiarly unfitted for steady and continuous work. Some subject presented itself to him in which he became interested. He laboured at it with hearty good will for a time, always illuminating it with the light of his genius as no one else could, and then he strayed to "fresh fields and pastures new." The result is a number of separate essays, on all sorts of subjects, of which it would be simply impossible to attempt anything like an exhaustive review. We have in this volume alone, and it is only the first of a series, no less than eighty-eight different papers, some of them of great length, many of them starting subjects quite original; and these are only the selected essays, the editor having omitted many that he deemed of secondary interest. It is curious, on looking over them, to note how greatly they have influenced obstetric practice, to observe how much of the immense advance which this branch of the profession has made of late years, can be directly traced to these writings. It would be hopeless to try to show this forth within the limits of one short article. All we can do is to refer briefly to some of the leading contributions, which either were most celebrated at the time of their publication, or have had the most influence on the present state of obstetric practice.

The first division of subjects refers to pregnancy, and it commences with a well-known essay on its duration. In it are collected pretty well all the known facts on this subject, commencing with the celebrated Gardner peerage case, which gave rise to so much difference of opinion amongst the leading obstetricians of the day. After relating several cases of protracted utero-gestation, the facts of which were such as to leave little doubt as to their authenticity, and the longest of which extended to 313 days, Simpson tabulates and discusses the cases recorded by Murphy, Merriman, and Reid. Some of these facts vary curiously from those observed by more recent writers; thus, out of 186 cases recorded by Murphy, no less than 35 were delivered after the 282nd day from the cessation of menstruation; while in the recent table published by Attfield, out of 30 cases very carefully observed, with the exception of two, of which the dates were doubtful, all were below that period. It is, we believe, now a pretty generally received opinion that 280 days, which is commonly reckoned as the usual period of gestation, is too high an average, and that a calculation of 275 days is less likely to prove wrong. In Simpson's tables, a large proportion of the cases, from 12 to 16 per cent., terminated from 267 to 273 days after the last menstruation. Simpson distinctly admits, what is now rarely questioned, the possibility of pregnancy being occasionally considerably prolonged, even to 30 or 35 days beyond the average period, and brings forward as an argument in favour of possible protraction the researches of Lord Spencer and Tessier on the length of utero-gestation in the cow, where the exact period of conception was known. Finally, he starts here, for the first time, his well-known hypothesis of fatty degeneration of the decidua being the determining cause of labour.

The only other section we need remark on is that in which the oxalate of cerium is recommended as a remedy for the sickness of pregnancy. Although still often used, and sometimes of decided service, it is certainly not found entirely to answer Simpson's expectations with regard to it. The paper is, however, interesting as illustrating Simpson's habit of seeking experimentally for new and valuable drugs, which sometimes produced a happy hit among many failures, a habit which eventually led to the great discovery with which his name is most associated.

In the next section, which concerns the fœtus, the two most important papers are, one which refers to the disease of the fœtus itself, and another which treats of diseases of the placenta, and their secondary effects in interfering with the nutrition and respiration of the fœtus, and thus causing its death. Neither of these subjects had received much attention at the time Simpson wrote; that on the disease of the fœtus is still almost on a level with our present knowledge. The greater portion of it treats of fœtal peritonitis, bringing forward a number of cases where post-mortem examinations had been made on still-born children, in which the products of inflammation had been found more or less abundantly in and about the peritoneum. With regard to its cause, Simpson is inclined, and no doubt rightly, to attribute a prominent place to syphilitic taint, and he lays down the practical axiom that, when a mother gives birth to several dead children in succession, in whom traces of peritonitis are found, syphilis may with certainty be assumed to exist in one or other parent.

An interesting chapter on this subject is that in which Simpson discusses the reproduction of rudimentary limbs on the stump of intra-uterine amputations. This curious fact, which is far from rarely witnessed in such cases, Simpson explains by supposing that the intra-uterine amputation took place at a period of fœtal life when "the physiological powers of the young human being are more assimilated to the reparative and other powers of animals of a lower type in the animal scale," and that, therefore, the attempts at reformation of the lost parts resembled the reproduction of lost portions commonly observed in many invertebrate animals, such as the salamander and the triton.

Disease of the placenta, and its effects on the life and nutrition of the fœtus, was always a favourite subject with Simpson, and was one of the first through which he brought his name into public notice. Until he took it up comparatively little had been done in the matter; since that, however, a good deal of attention has been paid to it, and the writings of Barnes, Druitt, Spaeth, Hyrtl, and many other pathologists, have thrown much light on doubtful points, have still further proved the important influence which a morbid state of the placenta has on the health of the fœtus, and have in various respects corrected the views held by Simpson.

With regard to congestion of the placenta, and its frequently attendant extravasation of blood into the substance of the organ, Simpson makes the observation that it is a much more frequent cause of death of the fœtus in the earlier than in the later months of pregnancy, although even between the seventh and ninth months it is a not uncommon cause of premature labour occurring in successive pregnancies. This remark has been quite recently confirmed by the researches of Hegar, of Fribourg. Simpson is inclined to attach considerable importance to placentitis, the existence of which he considers unquestionable. More recent writers, however, throw doubt on the existence of placentitis as a pathological condition, attributing the phenomena ascribed to it to secondary changes taking place in extravasated blood-clots, or, to quote the words of Robin, "What has been taken for inflammation of the placenta is nothing else than a condition of transformation of blood-clots at various periods; what has been regarded as pus is only fibrine in the course of disorganization, and in those cases in which true pus has been found the pus did not come from the placenta, but from an inflammation of the tissues of the uterine vessels and an accidental deposition in the tissues of the placenta." With regard to fatty degeneration, the importance of which has year by year been more fully established, and the precise changes connected with which have been very fully worked out by Robin, it is worthy of remark that in his earliest writings on the subject of placental disease, published as early as 1836, Simpson suggested that the stearoid matter frequently seen in diseased placentæ might in reality be the results of the transformation of blood-clots, a view maintained by Scanzoni, but opposed by other writers.

In noticing Simpson's labours on this subject we must not forget his original suggestion that the administration of various alkaline salts, especially the chlorate of potass, might improve the chances of the fœtus surviving in cases of placental disease, by effecting a better oxygenation of the blood, and thus making a smaller aerating surface to do more work. Probably the plan is not quite so serviceable as its originator believed, and its assumed *modus operandi* is very open to doubt; but those who have tried it are generally inclined to admit its occasional utility. At any rate, it has the merit of being the first definite attempt to influence the health of the fœtus through the maternal blood, a line of experiment which may yet lead to valuable results.

Passing over several papers, including the interesting case in which a decomposed and softened fœtus was pushed through a contracted pelvis, where the Cæsarian section would otherwise have been necessary, we come to the celebrated dissertation on "Placenta Prævia." This is one of Simpson's most important contributions to obstetric medicine, and is composed in his happiest style. Many of our readers will, doubtless, remember the sensation produced by the bold and thoroughly original proposal that in certain severe cases of placenta prævia the placenta itself should be removed, and the somewhat bitter controversy to which it gave rise. Increased knowledge of the anatomy of the placenta, and of the circumstances under which it comes to be implanted on the cervical orifice, teaches us that many of the views upheld in the paper are now untenable, especially those which explain the facts of the cessation of the hæmorrhage on the detachment of the placenta, by the supposition that the blood comes in great part from the detached surface of that organ, and not from the uterine vessels. We believe that, latterly, Simpson himself had to a great extent altered his opinion on this point, while still maintaining his practical advice with regard to the removal of the placenta. Admitting this, it is still impossible to read the essay without profound admiration of the ingenious reasoning on the facts, so far as they were then known, and of the extraordinary erudition displayed in searching for examples in authors whose existence had been almost forgotten. The reader will do well also to study the sort of essay within an essay which treats of the means by which nature seeks to control hæmorrhage after evacuation of the uterine contents. This is thrown in, as it were, amongst the other material of the monograph, and yet it forms probably the most suggestive and lucid explanation in existence of the means by which post-partum hæmorrhage is prevented. Greatly as recent researches have obliged us to modify Simpson's views, it must be admitted that he was the first to recognise the fact that the supposed danger of complete placental separation was imaginary, and to deduce from it a new method of treatment. This the more recent writings of Jacquemier, Barnes, and Cohen of Hamburg, have enabled us to explain with greater accuracy, and the practice recommended by the two latter physicians is, no doubt, at once more scientific and more practicable than the separation of the placenta *en masse*. Still, it is unquestionable that their procedure is but a modification of the plan first worked out and practised by Simpson. It ought to be noticed also, when talking of this paper, how persistent and unfair were the misrepresentations to which it gave rise. To read the criticisms which have been made on it it would appear that Simpson recommended complete detachment of the placenta in all cases of placenta prævia. Nothing can be farther from the truth. Over and over again he positively states, in language so clear that it is difficult to understand how it could have been misunderstood, that the plan is to be reserved for those rare and exceptionable cases only in which the ordinary methods of treatment are impracticable and unsafe.

The next important paper is "On the Sex of the Child as a cause of Difficulty in Labour." It is one which is, perhaps, more interesting in a physiological than in a practical point of view. Full of the most elaborate statistical deductions, it must have required much work in its production, and, although it is based on statistics the details of which may be open to question, the general accuracy of the conclusions arrived at can scarcely be doubted.

Passing over several papers of interest, such as that on turning, with its recommendation of bringing down one knee only; on difficult labour from dorsal displacement of the child's arm; and on intra-uterine hydocephalus; we come to one of Simpson's most important essays, that on "Turning as a substitute for Craniotomy." When this was written, craniotomy was not regarded with quite as much horror as it now fortunately is, and the proposal of an alternative operation which might occasionally obviate the necessity for resorting to it was not received with as much favour as it undoubtedly merited. Although turning in contracted pelvis is not a new thing, since it had been practised by many of the most distinguished accoucheurs in the last century, such as Pugh, Smellie, and Perfect, and with remarkable success, still it had practically fallen into oblivion. Simpson's remarkable essay not only again directed the attention of the profession to its value, but, for the first time, distinctly showed the precise reasons why a child brought footling might sometimes pass through the pelvis alive, when it could not be pulled through with the head first. His demonstration of the fact that the base of the fœtal skull, in consequence of its being narrower than the occiput, passes where the latter will not, and of the greater power of traction thus gained, of the adaptation of the narrowest diameter of the skull to the contracted diameter of the pelvis, and of the greater safety with which the child's head bears compression in the transverse diameter, were all new facts in obstetrics, recognised by him for the first time, and very forcibly insisted on as arguments in favour of the practice he proposed. Nowadays the value of turning in such cases is admitted by the highest authorities, and it is daily being more and more resorted to. The number of fœtal lives saved by it is now so great that it is hardly necessary to bring forward any argument in its favour. It is worthy of remark, however, that in spite of all the attention the subject has received, scarcely any fresh light has been thrown on it since the publication of this paper, so thoroughly and completely had Simpson worked it out in all its bearings. All that he left for future obstetricians to do was to illustrate the advantages of this recommendation by proving how frequently they had resorted to it with success. The bitter discussions that this paper gave rise to, in consequence of the use made in it of statistics from the Dublin School of Midwifery, will, no doubt, be fresh in the memory of many of our readers. In such a feud Simpson was an adversary not readily discomfited, and he certainly had no cause to consider himself worsted in the fight. Still it is much to be regretted that so fierce a battle of words should ever have arisen, the more so as this and similar discussions led those who did not know him to believe that he was of a quarrelsome disposition and addicted to controversy. Such an impression is, as all who knew him well will admit, a thoroughly erroneous one. The fact is that one who wrote so much and on so wide a range of subjects, who started so many novel theories, could hardly fail to meet with many who differed with him in opinion; and although he was always ready to support his own views with characteristic force and energy, still he at least never permitted controversies to merge into private enmity, and his personal sweetness of disposition always made him ready to hold out the hand of friendship to his strongest opponents.

The section on the puerperal state commences with a short but suggestive paper on death after delivery from entrance of air into the veins. The subject is one of much importance, which has scarcely as yet received any attention, and it is not impossible that it may yet be found to account for some otherwise inexplicable cases of sudden death. He (Simpson) noticed in the cases recorded a peculiar reddish rash upon the cheeks, which had already been observed in those recorded by Dr. Warren, of Boston, and which he believed might be due to direct oxygenation of the blood in the capillary vessels. The mechanism by which such an accident might occur is discussed, and the ingenious hypothesis is started that the uterus, by alternate contractions and relaxations, might act as a sort of force pump, especially if the os was obstructed by a clot, and so drive any air there might be in its cavity into the open mouths of the uterine sinuses. The explanation is certainly not an unreasonable one, and might account for an accident not otherwise easily explicable. The precise way in which the air proves fatal is not discussed, and is well worthy of study. The theory advanced by Bertin, in his learned but little known work on 'Embolism,' seems to us to afford the best explanation of a difficult problem. He conceives that the air bubbles become arrested in the pulmonary capillaries, the extreme points at which they have been found in post-mortem examinations, and so prevent the access of air into the lungs. They thus form, in fact, true gaseous emboli, and cause death in precisely the same manner as when the pulmonary circulation is arrested by fibrinous thrombosis and embolism.

Another very important essay is that entitled 'Puerperal Arterial Obstruction and Inflammation,' which deals with a class of cases still not sufficiently studied, but of great practical importance in the puerperal state. The changes which the blood undergoes in pregnancy, the still further alteration effected in it when delivery is over and involution has commenced, often lead to a variety of pathological conditions, the importance of which is being daily more and more appreciated. It is interesting to see how many opinions, now pretty generally received, were foreshadowed by Simpson long before they had attracted the attention of the profession. Thus so long ago as the year 1847, the same year in which Virchow published his essay on 'Inflammation of the Arteries,' and five years before the late Dr. Kirkes wrote his well-known paper on the same subject, Simpson had published a remarkable case in the proceedings of the Edinburgh Obstetrical Society, in commenting on which he proposed the following questions:

"1. Was the obstruction of the artery or arteries in this case produced by any mechanical cause, as one of the vegetations separated from the cardiac valves carried along, in the case of the arm for example, to the bifurcation of the humeral artery, and impacted there? 2. Was it not rather the result of an original puerperal arteritis? 3. Or might it be the effect of an effusion of coagulable lymph from phlebitic inflammation on the coats of the artery, a secondary phlebitic deposit, or living arterial membrane?"

Here we have a very distinct intimation that he had realised clearly the chief phenomenon of embolism, the carrying of an embolus along the current of the blood.

In the first section of his paper arterial obstruction is chiefly dealt on, and the causes likely to give rise to it, with illustrative cases gathered from various authors, are very fully entered into. The fifth section deals with obstruction of the pulmonary arteries, the importance of which, as a cause of the appallingly sudden deaths sometimes met with in the puerperal state, have caused much attention to be directed to it of late years. When the paper was written the subject was comparatively new, and only a few isolated cases were on record in which this cause of death had been recognised. In spite of this Simpson had clearly realised its importance, and had pointed out that all the previous theories of sudden death in the puerperal state would require to be revised, and that the so-called cases of "shock," "syncope," and "idiopathic syncope," terms devised as a cloak for ignorance, and meaning nothing, might generally be traced to obstruction of the pulmonary arteries, or its chief branches. Many facts connected with the natural history of pulmonary obstruction, which recent researches have more or less distinctly proved, are hinted at in the paper. Thus, the important predisposing influence of blood alterations due to the puerperal state is pointed out, the possibility of spontaneous thrombosis in the pulmonary arteries themselves, without any primary embolism, is recognised, and the fact that life may be prolonged for a considerable time after the formation of the obstructing clot is admitted. All these we now know much more about, but in estimating the importance of this paper we should not forget how novel the subject then was, and how little light previous researches had thrown upon it.

The section on gynæcology commences with a short but interesting paper on defective and excessive involution of the uterus after delivery. No one who sees much of female disease, now doubts the important influence of these two states, especially the former, in causing various morbid phenomena after delivery. Defective involution is of very frequent occurrence, and originates a large number of somewhat obscure cases, in which uterine disorders can be distinctly traced to a previous confinement. Superinvolution is not so often recognised, and is probably a much rarer occurrence. The reader will find recorded in this paper some interesting cases in which amenorrhæa after delivery was distinctly traced to this cause, and in which the uterus was reduced to much the same state as is not unfrequently observed in connection with amenorrhœa in the young female, when the uterus has not increased in size at puberty, and has retained its undeveloped or infantile state.

The next paper on the uterine sound is of extreme importance. There can be no doubt that the use of this instrument has greatly aided the remarkable advances which gynæcology has made in the last quarter of a century, perhaps quite as much or more than the speculum. It is certainly to the possibility of making a precise investigation into the actual state of the uterus and its appendages that we owe the immense progress recently made in this branch of medical science. As Simpson has so clearly pointed out in his paper, so long as the physician had to trust to functional symptoms alone, he must have been constantly misled, as the same symptoms are common to so many and such different uterine disorders. That an immense advance took place along with the introduction of the speculum into practice is certain, but it will doubtless be admitted that this was not entirely from what we directly learned by the instrument, useful as it is, especially in enabling us to apply certain methods of treatment to the uterus itself. Much of it can be traced to the increased carefulness with which physical examinations are made, and to the greater frequency with which they are practised. The uterine sound, however, has been *per se* of immense value. By it we are enabled to diagnose accurately certain conditions about which we were previously either altogether ignorant, or which could not be made out with certainty. It would be beyond the scope of this article to attempt to prove this at length, but every one conversant with female disease will recall many instances. Thus to take one or two examples only, we need only remember how little we knew or could know accurately about such conditions as defective involution after delivery, the various flexures and displacement of the uterus, the differential diagnosis of uterine and ovarian tumours, and the like, to be convinced of the enormous value of the instrument. Had Simpson, therefore, done nothing else than familiarise the profession with this important diagnostic aid, he would have done enough to cause his name to be remembered with honour. The essay itself is well worthy of careful study, as an exhaustive account of the uses of the instrument, and, although the first, it still remains by far the best monograph on the subject in existence. It has, however, the fault of advocating a free use of the instrument, which certainly would not be safe in careless or uninstructed hands. Invaluable as it is as an aid to correct diagnosis, it is beyond doubt that when carelessly or roughly used the uterine sound is capable of doing considerable damage, and there are probably few gynæcologists who have not occasionally seen some considerable inflammatory mischief following its introduction in unsuitable cases. The possibility of an instrument doing harm when used improperly is no argument against its proper application; but a word of caution on this point can hardly be out of place, especially as Simpson's paper contains no evidences of any possible ill effects that might follow its employment.

Another instrument devised by Simpson is discussed in the next short paper, viz. the small exploring needle, with which his old pupils must be familiar. It is a most useful contrivance for clearing up the diagnosis in certain doubtful cases, and is less known than its merits deserve. Then follows an article on the use of the medicated pessaries, a most useful means of treatment largely employed by Simpson, and now extensively used.

After this are two short notes on the very important subject of intra-uterine medication. This has of late attracted much attention, both in this country and in America. Its value, in properly selected cases, can hardly be over estimated, and it is to be regretted that Simpson had not then investigated the matter more than these brief notices would indicate. It is to be remarked, however, that he had apparently anticipated a method of treatment recently strongly recommended by Courty, of Montpellier, that of introducing a stick of lunar caustic into the uterine cavity, and leaving it there to melt and flow over the mucous membrane; and also that he had very clearly realised the proper explanation of the risks of intra-uterine injections of fluids, as the following sentence shows:

"He considered solid substances and powders as much more safe applications to the interior of the uterus than any form of fluid injection. The occasional danger arising from the latter was not, he believed, so much from their passing along the Fallopian tubes into the peritoneal cavity as from their sometimes over-distending the uterus, and fissuring and tearing through the mucous surface, and getting into the circulation."

In the next paper, treating of "Dilatation and incision of the cervix uteri in cases of obstructive dysmenorrhœa," we come upon very delicate ground. It is to be feared that Simpson's enthusiasm and parental fondness for an operation he had himself devised, somewhat biassed his judgment in this matter. Certain it is that incision of the cervix is not now practised with anything like the frequency it used to be, and all will, we think, admit that an increased acquaintance with it does not justify the high opinion which many have had of it. That it is occasionally of decided benefit in obstinate cases resisting other treatment is beyond all question. But it is equally certain that it should not be resorted to indiscriminately, and never without insisting on perfect rest and great care after its performance. When we hear of its being done in the consulting-room, of patients being sent home without any precautions to restrain possible hæmorrhage, or without any restriction as to exercise, we can scarcely be surprised at the occasional disastrous results which are said, from time to time, to have happened.

We are glad to learn that increased experience induced Simpson to modify his practice in this respect, as Dr. Black has very properly pointed out in the following note inserted at the end of the chapter:—

"In consequence of the gratifying results sometimes produced by incision of the cervix uteri in obstructive dysmenorrhœa and sterility, that operation became an extremely favorite one with Simpson. Certain risks connected with it, however—and in particular its liability to be followed by pelvic inflammation—inclined him ultimately to a rigorous selection of cases, and to the enjoining of recumbency several days after the performance of the operation."

The next paper, on "Retroversion of the unimpregnated uterus," is interesting because its subject is to this day a fruitful cause of dispute among gynæcologists. There is certainly no other uterine disease about which such varying opinions are held, and such diverse treatment practised. Those who believe in the all-importance of uterine flexions, and in the paramount necessity of straightening and retaining the uterus in its rectified position, necessarily stand on a very different platform from the decidedly larger number of physicians who believe in the secondary nature of flexions, and who teach the importance, above all things, of remedying the causes which have given rise to them. While those who try to shape a medium course between the two occupy, as we believe, a safer stand point with regard to the disease. Into this *questio vexata* it is not our intention to enter, especially as this subject has been discussed at some length in the last number of the 'Review.' It may, however, be well to recapitulate briefly the opinions advanced in this the first important essay on the subject, which, in the clash of modern warfare on uterine deviations, may have been somewhat neglected. There can be no doubt that the flexionists, if we may be permitted so to call them, may claim Simpson as one of their school. While, however, he distinctly asserts that the symptoms accompanying flexions are primarily due to the displacements, and that the congestion and other phenomena are the results and not the cause of the condition, he, at the same time, clearly admits, what many of the school are so apt to overlook, that the symptoms are by no means necessarily always met with. Nothing can be clearer than his words on this point:—

"The functional symptoms that I have enumerated may make us suspect the existence of retroversion of the uterus, but retroversion may be present without most, or almost any of them; and they may be present with other diseases besides retroversion."

These, it will be observed, are precisely the arguments brought forward by those who do not believe in the necessarily important effects of deviations. After a very clear account of the differential diagnosis of displacement, and of the use of the uterine sound in helping us to arrive at it, Simpson proceeds to consider the treatment. It is this part of the paper which will certainly be found most fault with, as he advocates an almost purely mechanical treatment, and that of a kind which increased experience has led us now rarely to resort to. It was the recommendation of the intra-uterine pessaries here described and figured which caused Simpson's practice in this respect to be so unfavorably criticised. All that can be said about it is that if Scotch uteri could bear such treatment with impunity, they must have been very differently constituted from similar organs in other parts of the world. But if half of the tales told be true, the bad results which followed their use were neither few nor far between. Happily they are now but rarely employed by any who know much of their action, and in very exceptional cases only, and we learn from a footnote that Simpson had, of late years, chiefly used a modification of Hodge's pessary instead. But the student, taking this paper as his guide in treating flexions, as many may be reasonably expected to do, would certainly not learn from it that such intra-uterine appliances were anything but perfectly safe and manageable.

Next we have a series of papers treating of fibroid tumours of the uterus, their course, termination, and treatment, including under this heading the ordinary pedunculated fibroid polypi. The chief value of these papers consists in their recommendation of the sponge tent as a means of dilating the cervix uteri. This is certainly one of the most useful and ingenious of Simpson's inventions, and we are, by their use, enabled to diagnose and successfully treat a class of cases formerly quite beyond our reach. Nor have recent researches done much to improve the original instruments, as the laminaria tents, although very serviceable under certain circumstances, are not so suitable in cases in which complete and wide dilatation of the cervix is required.

We must pass over for want of space the remaining papers, although there is much in them that is novel and original, such as those on "Vaginismus," and on "Fistulæ resulting from pelvic abscess." That on ovariotomy will always be of interest, not because of anything in it that is not now well known, but because it shows clearly how prescient Simpson was in recognising and advocating the value of the operation at a time when its opponents were far more numerous than its friends. It is impossible to bring these necessarily imperfect remarks to a close without once more expressing our admiration for the originality and profound erudition displayed in this book, which will always be the worthiest and most enduring memorial of its author's unrivalled talent.

VII.—The Deformities of the Human Body.[1]

This reissue of some lectures previously published in the 'Lancet' has been made, so the preface informs us, in compliance with frequent requests. As descriptions of orthopædic pathology they are good, and they give most of the main points in the treatment of deformities correctly and clearly; but they are too incomplete, too sketchy, to merit the title of 'A System of Orthopædic Surgery,' which the author has applied to his reissue. The book represents, no doubt fairly enough, the author's views in orthopædy, but it gives imperfect information of the progress this branch of surgery has made on the Continent and in America. This might have been usefully introduced when preparing the work for separate publication. The book is marred also by attempts to depreciate the labours of others practising the same branch of surgery — a kind of rivalry too prevalent in the medical profession.

The author has divided his treatise into three parts, the first devoted to contractions of the limbs, the second to affections of the joints, and the third to deformities of the trunk and neck.

In discussing the causes of deformities the modicum of knowledge which we have at present is correctly stated; "nervous irritation," with arrest of development, being, of course, held responsible for most congenital deformities, the *modus operandi* of the former cause being still as mysterious as it was when Marshall Hall endeavoured to clear away the darkness that surrounds it. Oddly enough, when speaking of *congenital* malformations, our author mentions "intestinal irritation from worms" and dentition. The first is probably an oversight; but for ascribing congenital deformity to dentition, Marshall Hall's comprehensive use of the term is relied on, that authority believing that the period of nervous irritation which sometimes accompanies the growth of the teeth may begin at or before birth. It is unfortunate that in this age of unbelief there are those who doubt the connection between the irritation of dentition and reflex cerebro-spinal action, but it is no part of our present object to discuss the correctness, probable or otherwise, of this doctrine.

Hereditary predisposition is, doubtless, a real and very curious cause of clubfoot. Arrest of development is a more satisfactorily comprehended source of deformity. Several forms, even talipes, being simply the continuance of an early stage of

[1] *The Deformities of the Human Body: a System of Orthopædic Surgery.* Being a Course of Lectures delivered at St. George's Hospital. By Bernard E. Brodhurst, F.R.C.S. London. Large 8vo, pp. 259.

fœtal development, which, by want of further progress in the formation of the limb, becomes its permanent condition. The immediate cause of this arrest of development still remains unknown, but that it is an efficient agent there is no doubt.

In describing the pathology of rickets it is somewhat vexing to find our author attributing the pliability of rickety bones to simply the softening of the hard bone by absorption of the earthy salts. Virchow and Kölliker have demonstrated that the softness of rickety bones is due as much to arrested development of their ossification as to reabsorption of their earthy salts.

The production of rickety bones is a very complex process. The primary cartilage remains in one part unchanged; in another it is transformed into fibrous tissue or into true bone; and this may become further altered by a form of osteo-myelitis. Hence, in rickety bones all these forms of development are present side by side. In treating rickets, the author's directions are very judicious. He rightly insists on the necessity for recognising rickets as a morbid condition, affecting not merely the osseous tissues, but all the tissues of the body, and urges the propriety of treating the local deformities before general disturbance, denoted by night sweats, tendency to diarrhœa, and other symptoms of disordered functions throughout the body have disappeared; for when morbid action subsides, development goes on rapidly, the flexible bones soon become strong and rigid, and it is then too late to bend them back to their normal shape. There is, however, one omission that, perhaps, it is not unnatural an orthopædic surgeon should make, namely, to point out that limbs that are slightly but still clearly distorted, constantly regain their proper form, without the aid of any surgical appliance whatever. Advanced cases can, of course, be only successfully treated with instruments. For example, when the amount of bend in the tibia is slight, and has not long existed, the simple precaution of removing all vertical pressure from the bone by preventing the child from standing on his legs until his rickety diathesis is altered, suffices to restore the tibia completely to its natural shape. Possibly the "continuous extension" of the weight of the feet on the curved tibia is an efficient cause in restoring the straightness. This natural tendency should never be lost sight of, even when it must be aided by supports.

The reunion of cut tendons is accurately described as taking place in two ways,—namely, by direct junction of the divided surfaces, and indirectly by the formation of a plastic material, on the ductility of which the surgeon depends when giving appropriate length to contracted muscles. Mr. Brodhurst, in this description, gives very briefly and clearly the results of earlier observations specially directed to investigating the nature of this interesting process, but the researches of Stricker and his assistants on the changes of fibrin and connective tissues in inflammation might have been advantageously referred to.

Three chapters are devoted to the different forms of talipes. These forms are well described, and are illustrated by good drawings, but the directions for applying the forces to overcome the distortion are little more than sufficient to indicate that these affections are curable. Certainly no novice could undertake the treatment of clubfoot from these lectures. For instance, "the treatment of talipes equinus consists of the restoration of the shape of the foot by the division of the tendo Achillis, and the subsequent gradual extension of the new material which is deposited between the divided ends of the tendon, until the foot can be well and sufficiently flexed upon the leg." This, no doubt, is correct enough so far as it goes, but few cases of talipes equinus are absolutely simple, as there is usually a shortened plantar fascia and a retracted metatarsus. Division and extension of the tendo Achillis would only remedy part of the deformity in such cases. The means of encountering these numerous little difficulties that cannot be explained in general works of surgery we expect to find duly set forth in a special work on orthopædics.

The author, in treating of the means for arresting pain in diseased knee-joint by the division of the hamstring tendon, has given some very instructive cases, and in the course of his remarks Mr. Brodhurst lays great stress upon it as a mode of relieving continued pain and of promoting the restoration of the joint. It is, we think, a proceeding hardly enough employed by surgeons. In such cases tenotomy is evidently an adaptation of the principle of "rest."

In treating of anchylosis, both fibrous and bony, or "false" and "true," as they are denominated, Mr. Brodhurst has hardly included in his teachings the methods successfully employed by others for combating these rigidities. No mention is made of Sayre of New York, whose very remarkable success in restoring motion to rigid hip-joints was probably well known to Mr. Brodhurst, for Sayre's earlier cases were published in New York as long ago as 1863, and in consequence of an erroneous quotation by Bauer of New York, they were republished in the 'New York Medical Journal' of 1869, *in extenso*, with corroborative testimony of their perfect success from the surgeons who had been present at the operations, or had examined the patients afterwards.

The concluding chapters are devoted to spinal curvatures, but either the space at the author's command, or want of the revision that the earlier divisions of his subject have received, render this the less satisfactory part of his book.

We hope that, should it become advisable to put forth a new edition of these lectures, they may undergo a complete revision, and receive the necessary amplifications to render them worthy of the title—' A System of Orthopædy.'

VIII.—Physiology of Blood-Crystals.

It is somewhat startling to find a book of nearly 300 pages devoted exclusively to the consideration of the red colouring matter of the blood, a subject which ten years ago received only a passing notice in the best works on physiology. The great development of this subject is attributable, first, to the discovery of the fact that blood is capable, under certain circumstances, of undergoing crystallisation, leading to a vast number of researches on the form of the crystals in man and animals, on their composition, and on the conditions favouring their production; and, secondly, to the application of the spectrum analysis to the examination of the blood, which has opened up a wide field of great interest. The value of the facts obtained can scarcely be estimated at present, but there can be little doubt that some aid will be obtained from them in medico-legal investigations, and perhaps it may one day become possible to ascertain, with absolute certainty, to what animal a minute crust of dried blood originally belonged.

The first notice of blood-crystals appears to have been by Hünefeld, in a prize essay published in 1840, who observed tabular crystals in the cracks of the balsam on the glass slide on which he had mounted some human and pig's blood; he was, however, unable to explain their nature. They were then observed by Leydig, in 1848, in Nephelis and Elepsine. The real merit of the discovery is due to Reichert, who saw them, in 1849, in the placenta of a guinea-pig; and though attributing their colour to the mechanical admixture of pigment, yet recognised their form and organic nature, and named them tetrahedral albuminous crystals (Albuminat-krystalle). They were subsequently seen by Kölliker, who called them globulin crystals, and by Budge, who, in 1850, described the blood-crystals of man, which he obtained from the stomach of the leech. None of these authors, however, followed up their observations, and it was reserved for Otto Funke not only to rediscover the crystals, but to show how they may be obtained at will. He stated in 1851 (' Zeits. f. Rat. Med.), that if to a few drops of blood which have been allowed partially to evaporate a little water be

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[1] *Die Blutkrystalle.* Untersuchungen. Von W. Preyer. Mit drei farbigen Tafeln. Jena, 1871. 8vo, pp. 263.

added, the borders of the small masses of corpuscles will be seen to undergo a sudden change, so that whilst some of the corpuscles disappear, the others retain their thick contours, and become angular, elongated, and well-defined rods. They thus form the embryoes, so to speak, of an immense number of crystals which are too small to permit their shape to be defined, but which gradually elongate, whilst their diameter increases but little, if at all, and form prismatic spherules which in part assume a jointed character, and ultimately cover the whole field with a close network of decussating or acicular crystals." He obtained blood-crystals from the horse, dog, and fish, and in the following year (1852) made the remarkable discovery that in the fish the crystals did not arise from the metamorphosis of the whole blood-corpuscle, but of only a part, each crystal being contained within the blood-corpuscle. In the same year Bisegger and Bruch observed the same phenomena in the blood of the rat. Funke further insisted on the fact that the crystallising substance was not the albuminous principle of the corpuscle alone, but the globulin in combination with hæmatin. The expression hæmato-globulin, already frequently employed, was first limited by Berzelius in 1840 to designate the red colouring matter of living blood as opposed to the colouring matter obtained by artificial means, which he named hæmatin. Lehmann applied the term hæmato-crystallin to the crystallisable red constituent of the blood-corpuscles. Schlossberger, in 1860, called it hæmoglobulin, and, when crystallised, hæmocrystallin. The expression hæmoglobin, which has the advantage of brevity, was invented by Hoppe Seyler in 1864. Bertin, in 1856, called the red colouring matter chromatin, and entertained the singular opinion that the crystals were really colourless, but owed their colour to their refracting properties. Stokes, in 1864, called the red colouring matter of the blood cruorine, and distinguished between scarlet and purple cruorine; the former, M. Preyer states, is the somewhat impure coloured oxy-hæmoglobin, the formula of which he gives as O_2-Hb or $\left.\begin{matrix}O\\O\end{matrix}\right\}Hb$; the latter is non-oxygenated or reduced hæmoglobin (Hb). Stokes' terms have hitherto only found acceptance in England, the French having adopted the German phraseology. In Germany, the terms red colouring matter of the blood and hæmoglobin are used promiscuously to indicate the coloured iron containing crystals of albumen obtained from the blood (Blutrothstoff Hämoglobin, Blutroth, Roth, Blutfarbstoff).

Teichmann's hæmin crystals, which are identical with Lehmann's hæmatin when this is in the crystallised condition, and with Virchow's hämatoidin when this is found crystallised in old blood extravasation, and Preyer's hämatoin crystals, may all be called blood-crystals, since they are obtained from blood, but are not rightly so named, since they are really all products of disintegration.

The colouring matter of the blood-hæmoglobin, as M. Preyer prefers to call it, occurs in the blood of all Vertebrata (appearing as early as the third day of incubation in the fowl), and of some Invertebrata, as well as in the greater number of the muscles of warm-blooded animals, though there is still some doubt as to whether the colouring matter obtained from the latter is not really due to the blood circulating through them. By spectrum analysis M. Preyer has demonstrated its presence to a very small extent in thick layers (4—6 centimeters in depth) of the serum of the ox, sheep, calf, horse, and pig, a faint absorption stria appearing on the right and close to Frauenhofer's line D. He thinks it quite probable that it is not naturally present in the liquor sanguinis, but is derived either from the disintegration of a few blood-corpuscles during the formation of the clot, or from the presence of a few corpuscles still floating, but escaping observation, in the serum.

A substance presenting all the characters of hæmoglobin, though it does not appear to have been submitted to the spectroscopic test, has been obtained by Rollett from the red blood of the larva of *Chironomus plumosus*, and this is so far of interest as proving that hæmoglobin can be directly prepared from plants by the animal organism, these gnats being exclusively vegetable feeders. Preyer has shown that the red blood of the Lumbricidæ, which has long been known to contain iron and albumen, presents the characteristic striæ of hæmoglobin in the spectroscope, and these animals, like the former, subsist on plants. Hæmoglobin has also been shown to be present in the Phyllopod Crustacean *Cheirocephalus diaphanus*, and recently, by Ray Lankester, in the blood and muscles of Planorbis and many other mollusks. Leydig has demonstrated it in Nephelis, and other Annelids probably contain it; but the blood of insects, though capable of crystallising in stable crystals, does not appear to possess any colouring matter allied to hæmoglobin. This substance has not been found in any of the Protozoa, Cœlenterata, or Echinodermata, nor does it exist in plants.

To obtain blood-crystals in quantity, various methods have been suggested. M. Preyer enumerates no less than six: by water, and the transmission of oxygen and carbonic acid gases and the addition of a little alcohol; by freezing; by the injection of cold water into the vessels of an animal whilst it is being chloroformed to death; by dissolving the red corpuscles with a solution of the tauro- and glycocholate of soda; by the addition of one volume of water and then of one fourth of a volume of alcohol to the defibrinated blood and exposure to cold; and lastly, his own plan, which is as follows:

The blood is received into a capsule or saucer, where it is allowed to coagulate, and placed aside in a cool place for twenty-four hours. The serum with the white corpuscles and fat, if any, are removed, and the clot, after fine division, repeatedly washed with cold distilled water. The fragments, hardened by freezing, are placed on a filter and a stream of cold distilled water allowed to play over them until the filtrate gives scarcely any precipitate with solution of corrosive sublimate. Water at a temperature of from 86° F. to 107° F. is now poured over the clot, and being filtered is received into a large cylinder standing in ice. To a small quantity of the red solution alcohol is added drop by drop, till a precipitate begins to appear, and thus an estimate is obtained of the quantity required to be added to the whole. The crystals appear in great abundance in the course of a few hours. They must be washed with ice-cold water containing a few drops of alcohol, and then appear to be almost if not perfectly pure. The blood of the horse is that which is by far the best adapted for the preparation of very large quantities of the purest hæmoglobin crystals.

The modes in which minute quantities of blood may be crystallised are various. One of the simplest is that discovered by Max Schultze, who observed that the blood of the guinea-pig, kept at 14° F., dissolves, and that from the solution crystals separate on evaporation. No crystals, however, can be obtained in this manner from the blood of man, calf, or rabbit, though Preyer has found they are furnished by the blood of the horse. Bojanowski obtained hæmoglobin crystals by the evaporation of the watery extract of the blood-clot of a rabbit by passing the interrupted, and A. Schmidt by passing the constant, current through the blood of man, the cat, dog, and guinea-pig. Pasteur obtained an abundance of crystals from blood placed in a flask to which air that had been passed through a red-hot tube was admitted, and which was then allowed to stand for some weeks at 70° F. Bernstein obtained blood-crystals from defibrinated blood through which air charged with a small quantity of chloroform had been conducted, whilst A. Schmidt showed that the mere addition of alcohol to the blood of a dog sufficed to produce crystallization. Ether acts in the same way. Funke, Bojanowski, and others, have suggested various other plans for procuring small quantities of blood-crystals, but M. Preyer recommends the following as being the most expeditious:—A few cubic centimètres of defibrinated blood is mixed with sufficient water to furnish a clear solution. A drop of the mixture, covered with a thin piece of glass, and evaporated in the cold, will often yield crystals; but if not, then alcohol to about the amount of one fourth of the volume of the solution, is to be added, and placed on ice in a platinum or silver capsule; crystals will then speedily form. In regard to the mode of formation of the crystals, M. Preyer thinks that Beale is in error in stating that he has seen a single blood-corpuscle (of the blood of a guinea-pig) become converted into a crystal. In his experience it has always required the hæmoglobin of several corpuscles to form one crystal.

The colouring matter itself in the circulating blood is probably not free, but combined with an alkali; it could not otherwise exist in solution, for, on the one hand, hæmoglobin is very insoluble, and, on the other, the red corpuscles are amongst the least watery of all the soft parts of the body; moreover, there is evidence that it exists, as, indeed, investigation with the highest powers shows, in the granular condition in the corpuscles, for the blood corpuscles, when examined with a Nicol's prism, are certainly not doubly refracting, whilst, however finely the crystals of hæmoglobin may be pulverised, the dust so obtained is indubitably doubly refracting. The stroma or colourless portion of the living blood-corpuscle appears to be combined with the hæmoglobin, and, so to speak, fixes it. Yet this union is so feeble that very slight influences, which may be rather of a chemical or mechanical nature, will lead to their separation, and occasion the formation of crystals. In every case an alteration in the structure of the corpuscles is produced, rendering them no longer capable of retaining their hæmoglobin; removal of the gases of the blood, agitation with fragments of metal, the addition of water, the variations of temperature, and other crystallizing agents act in this way; the red colouring matter separates from the stroma of the blood and undergoes solution in the liquor sanguinis; in order that it may crystallize out from this, however, a second chemical agent is required—the addition of some acid, which, if not added from without, is probably formed in the blood, for Zuntz has shown that after the withdrawal of the body the alkalescence of the blood steadily diminishes; in other words, acids are developed. With this diminution of alkalescence the capacity of the blood to undergo crystallization steadily increases. It is probable that all the chemical agents used to induce crystallization, as ether, alcohol, ozonized turpentine, the transmission of oxygen, promote the formation of an acid or acids, whilst in some methods acids (oxalic acid) are directly added to the blood. If, then, the hæmoglobin be admitted to be combined with an alkali, there can be little doubt that this alkali is potash, and the relations both of the corpuscles and of solutions of hæmoglobin in potash render it probable that the colouring of the matter of the living blood exists in the form of a very soluble hæmoglobinate of potash, which combines with and retains a certain quantity of carbonic acid, quite independently of the degree of tension of this gas in the atmosphere surrounding the blood.

M. Preyer next proceeds to consider the crystalline forms assumed by the hæmoglobin, and gives an excellent table divided into seven columns, of which the first gives the name of the animal; the second, the form of the crystals; the third, the system to which the crystals belong; the fourth, the place where the corpuscles are formed, whether within or without the corpuscles; the fifth, the solubility in water; the sixth, the facility or otherwise with which the crystals form; and lastly, a column of remarks, including the name of the discoverer, a reference to the paper in which it was first mentioned, and to illustrations of the same crystalline forms by other writers. The crystalline forms assumed by the blood of no less than forty-six animals is here given, including all the domestic animals and the more easily accessible birds, reptiles, and fish. The crystals obtained by H. Landois from many insects he excludes from this table, on the ground that they have not been proved to be really hæmoglobin. Of the six systems of crystals, the table shows that five have been stated to be represented in the different forms of hæmoglobin, namely, the regular (tesseral), the tetragonal, the rhombic, the monoklinic (klino-rhombic, monoklinohedric), and the hexagonal. The triklinic (klino-rhomboidic) has not hitherto been seen by any one. It may easily be shown, in addition, that hæmoglobin crystals really never belong to either the regular or to the tetragonal systems, since, on the one hand, all hæmoglobin crystals are doubly refracting, which is not the case with any crystals belonging to the regular system; and, on the other hand, it has been recently shown that the crystals from the blood of the guinea-pig do not belong to the tetragonal system. The monoclinous system may, in like manner, be excluded, as Funke is the only author who has placed the hæmoglobin crystals of man and of the cat in this system, and his statements have not been supported by others. There remains, then, only the rhombic and hexagonal systems, and to one or other of these all blood crystals belong.

In regard to the optic relations of hæmoglobin crystals they are always perfectly transparent from whatever quarter derived, and always doubly refractile. They have a silky lustre, are pleochromatic, and their colour varies from that of venous to that of arterial blood.

The spectrum of pure oxy-hæmoglobin crystals invariably exhibits two very characteristic absorption striæ in the yellow and green, which are visible when only 1-10,000th heat is present in solution in water. These striæ increase in breadth and intensity with the increase of proportion of hæmoglobin in the solution, whilst at the same time there is a considerable absorption of the violet rays, and, when very strong (0·6 per cent.), of the red also, the two striæ coalescing into one black band. With still stronger solutions all the spectrum is absorbed, with the exception of a band of orange and one of green, these colours being alone transmitted. Two years after the discovery of the absorption bands of oxy-hæmoglobin, Stokes pointed out that the presence or absence of loosely combined oxygen made considerable difference in the character of the spectrum. Hæmoglobin free from oxygen presents only a single absorption band with ill-defined borders between D and E, a portion of the red and nearly all the violet being absorbed. Such a solution is best obtained by the addition of a very small quantity of sulphide of sodium to a solution of oxy-hæmoglobin, or by agitation of the blood with fine iron filings.

Remarkable alterations occur in the absorption striæ, when hæmoglobin undergoes decomposition. A stria then appears in the orange, which was previously transmitted with the greatest intensity (methæmoglobin). M. Preyer gives two beautiful chromographs of the spectra, not only of the solutions above mentioned, but of those resulting from the action of acids and alkalies, &c., on the blood.

The hardness of the blood-crystals varies in different animals, but, as a rule, they are soft, and readily break down when pressure is made upon the covering-glass. When one of the long prismatic crystals is fractured transversely the surfaces are uneven and splintery. The specific gravity of hæmoglobin has not, as yet, been ascertained, but it probably amounts, when dry, to between 1·3 and 1·4. All the forms of hæmoglobin hitherto investigated are soluble in water, though the degree of solubility varies to a considerable extent; and whilst some are hygroscopic, some few, as that of the crow, dissolve with difficulty in cold water. According to Hoppe Seyler, 100 c.c. of water, at 5° C., dissolve two grammes of the hæmoglobin of the dog, and the solubility increases rapidly with the temperature. Lehmann states that one part of dry crystals from the guinea-pig dissolves in 597 parts of water. The crystals of the squirrel also dissolve with difficulty.

Hæmoglobin crystals are insoluble in alcohol, in ether, in etherial and fatty oils, in benzole, turpentine, chloroform, amyl, alcohol, and in bisulphide of carbon.

Their solubility in water is increased by the addition of a small quantity of most alkalies and alkaline salts, but these after a few days occasion decomposition. Acids have little influence in increasing the solubility, but soon occasion decomposition.

Hæmoglobin constitutes an exception to Graham's theory, since, though a crystallizable substance, it will not diffuse through parchment paper; and the opposite results obtained by A. Schmidt may be attributed to the imperfection of the septum employed. This fact is of considerable importance in regard to the views of Schmidt on the escape of a fibrino-plastic substance from the corpuscles when these are placed in an albuminous fluid, as of hydrocele, when it was supposed that the globulin exuded from the corpuscles combines with the crystallizable hæmoglobin. M. Preyer, however, shows that hæmoglobin is not in the slightest degree fibrino-plastic. As regards the effects of heat upon the crystals, M. Preyer shows that, if thoroughly dried, they are capable of supporting the temperature of boiling water without undergoing decomposition, and the colouring matter of the blood of the guinea-pig only begins to break up at a temperature of 320° to 330° Fahr. The moist crystals, however, decompose much sooner, with production of methæmaglobin. Watery solutions of perfectly pure dog's hæmoglobin began to be turbid at 176° F.

The composition of hæmoglobin has been very carefully analysed by C. Schmidt and Hoppe Seyler. M. Preyer himself appears only to have estimated the iron and sulphur. The following appears to be the mean of various observations:

Carbon	54·00
Hydrogen	7·25
Nitrogen	16·25
Iron	0·42
Sulphur	0·63
Oxygen	31·15
	100·00

The simplest rational formula deducible from this is—

$$C_{600}H_{960}N_{154}Fe_1S_3O_{179}.$$

One molecule of hæmoglobin requires to form non-coagulable combinations, three molecules of soda; its equivalent weight is, therefore, 4444, or $\frac{13332}{3}$.

Oxo-hæmoglobin reacts feebly acid.

M. Preyer then proceeds to consider at length the action of various substances on the blood, a work that must have been extremely laborious, and is very complete; it includes the action of hydrochloric, nitric, sulphuric, phosphoric, chromic, boracic, oxalic, acetic, formic, valerianic, carbonic, and many other acids, of which, perhaps, the last is the most important and interesting. He divides the acids into four groups, of which the first, like phosphoric and sulphurous acids, produce no precipitation in solutions of hæmoglobin, but only a change of colour; the second, like metaphosphoric acid, produce, both in cold and warm solutions, precipitation and decomposition; the third, like nitric and hydrochloric acids when very dilute, only produce a change of colour, but when stronger, a precipitation and decomposition; the fourth give no precipitate in the cold, but coagulum occurs on warming, and to this carbonic acid and weaker acids belong.

The action of the various alkalies is more uniform than that of the acids. Since oxy-hæmoglobin is an acid, its solubility in alkalies is easily intelligible, soluble chemical compounds being formed, not differing in colour if the alkaline solution be weak, nor in the spectrum it produces. If, however, the solutions be more concentrated, or if the more dilute solutions are warmed, the blood-red colour changes to brown, or when seen in thin layers, into green; it is, therefore, dichromatic, and a process of decomposition sets in, the rapidity of the progress of which is dependent on the energy of the base; potash acting more speedily, then soda, ammonia, baryta, and lime. In the spectrum the two hæmoglobin striæ have vanished, and instead, a blurred absorption line occurs in the orange constituting the spectrum of the oxy-hæmatin alkali. The solution does not coagulate on heating, but remains clear, even at the boiling point.

The details of the action of a large number of salts are given by M. Preyer, as well as those of both reducing and oxidizing agents, the spectra being in many instances given in coloured lithographs.

Though hæmoglobin gives some of the reactions of albuminous compounds, as the xantho-proteinic and that with Millon's reagent, he will not admit that it belongs to that series, but considers that albumen splits off from it, as it were, in consequence of the action of the reagent.

Spectrum analysis constitutes, perhaps, the most satisfactory test of the presence of blood known; it is, without doubt, one of the most remarkable instances of the delicacy of modern methods of research. M. Preyer states that though he has not been able to distinguish the spectrum of a single red corpuscle, yet a very few enable it to be clearly perceived, though he estimates that each corpuscle only contains the 0·000,000,000,02 of a gramme of hæmoglobin. If on examination two absorption striæ appear, some reducing agent, as solution of sulphide of sodium, must be added; the reduction band then appears, and this can be again resolved into two by agitation with air or oxygen. An attempt should always be made to obtain hæmin crystals, and before the presence of blood is positively affirmed, iron should be demonstrated.

A long chapter of M. Preyer's work is taken up with an account of the combinations of hæmoglobin with oxygen, carbonic oxide, nitric oxide, nitrides, cyanides, &c., containing points of interest that we have no space to give.

The products of the decomposition of the colouring matter of the blood, which is, perhaps, the most complex substance in nature, may, of course, be regarded as almost infinite, but the most important of those containing carbon may be arranged in three groups, *albuminous compounds, colouring matters*, and *acids*. M. Preyer shows that though the term globulin has been commonly used to distinguish the albuminous substance that can be obtained from hæmoglobin, yet that different substances have been described under this name. He prefers the term globin to designate the albuminous part of hæmoglobin, and states that it is the purest form of albumen known, as it leaves no ash on being burned.

The colouring matters are numerous. Some few are produced in the living body from the red blood-corpuscles, though they cannot be formed from them artificially. Five of the coloured products of decomposition of hæmoglobin are crystallizable, namely, hæmin, hæmatoin, hæmatoidin, hæmatochlorin, and hæmatolutein. All others, if we except the biliary colouring matters, are uncrystallizable; to these last belong methæmoglobin, hæmatin, hæmathion, and other less known pigments.

Hæmin, and what was formerly called hæmatin, are identical.

In regard to the physiological significance and importance of the red colouring matter of the blood little can be said.

The scattered and, as it were, accidental occurrence of red blood in the lower animals might lead us to suppose that the colour was an unessential feature, and when present, might even, as in the case of the larva of the Chironomus, be injurious to the animal possessing it, by rendering it a more conspicuous mark for the attacks of its enemies. When, however, we come to see how constant this tint is in the blood of the whole vertebrate series, it is impossible to avoid arriving at the conviction that, like the green tint of vegetables, it is of some service, though we are not at present in a position to explain what. M. Preyer suggests that it may stand in connection with the heat-producing and heat-maintaining powers of the blood. The colour may be seen in the heart of the chick at the third day of development, but no observations have as yet thrown any light on the place of origin. Dr. Fürbringer has made a series of experiments under Preyer's direction, in which mixtures of peptones and albuminates were made with oxide of iron, but he uniformly failed in obtaining a blood-red colour. Nevertheless the observations of Virchow, that lymph exposed to the air reddens, is worthy of particular note.

In conclusion, we cannot part from M. Preyer without expressing our commendation of the excellent manner in which his essay has been prepared. It may be regarded as exhausting all the facts hitherto discovered on this subject, and at the same time as paving the way for new researches, which no one is better able than himself to undertake.

IX.—Sir H. Thompson on Practical Lithotomy and Lithotrity.[1]

It has been well said that the medical books which are most valuable are those which are written by men the results of whose ripe experience and extended practice have placed them in a position wherein the temptations to go into print are diminished just in proportion to the success and value of their labours. This is just the class of books which are nowadays the least forthcoming. The Tom Tiddler's ground of the publishers' lists is occupied, not to say crowded, with the productions of aspirants to fame rather than past masters in the art, and the volumes of the wise and learned make no more, or hardly so much, show in the advertising column as the cloth-bound handbills of pretenders. It is not to be wondered at if the man of established position steps hesitatingly and doubtfully into the jostling arena.

As students of the art and science of medicine and surgery, we are all the more pleased, however, when we have an opportunity of studying the doings and reading the thoughts of an experienced teacher. The sentiments of early veneration seem again to be recalled from the past of our primeval student life, when the opinions of the professor carried an overpowering authority, and sank with great weight into the very foundations of our knowlege, to form the buttresses which were destined to withstand, more or less perfectly, the waves of conflicting opinion, and the ever changing tides of fashion.

We feel again, when we approach such a work, like the scholar in Faust:

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[1] *Practical Lithotomy and Lithotrity; or, an Inquiry into the best modes of Removing Stone from the Bladder.* By Sir Henry Thompson, Surgeon-Extraordinary to H.M. the King of the Belgians; Professor of Clinical Surgery and Surgeon to University College Hospital. Second edition.

> "Ich bin allhier erst kurze Zeit,
> Und komme voll Ergebenheit,
> Einen Mann zu sprechen und zu kennen,
> Den Alle mir mit Ehrfurcht nennen."

With the advantage of the prestige of such feelings the volume before us presents itself. It speaks "ex cathedra," and we sit at once at the feet of Gamaliel. If we approve, we are conscious that we add but little to elevate the chair from which the utterances issue; if we differ, we feel, as it were, compelled to do so with deference, and to utter our protestations "with bated breath."

We shall, notwithstanding, endeavour to handle as freely as may be permitted to the chartered waywardness of the critic's nature, the doings and maxims which we find in Sir Henry Thompson's work, of which, like other good things, it may be said, that indiscriminate praise would savour of the condemnation of contempt as much as immoderate blame would suggest the influence of motives not impartial. We hope and shall strive not to be betrayed into either one or the other, not to feel one whit lowered by the acknowledgment of the master's excellences, not one jot lifted up by obtaining a vantage ground from which to look down upon them.

There is a disadvantage attendant upon publishing new books and new editions upon a trite and well-worn subject, which has engaged the attention of the best surgical intellects for centuries.

If we were asked to designate a subject in which it would be difficult to arrive, in these days, at anything which was at the same time both new and true, we should probably say lithotomy. So well has the ground been turned over here, and so tempting is the field for every labourer of eminence to put in his spade and leave his mark, that we have arrived at the pass that surgeons of great position have not thought it beneath their reputation to strive for the credit that might be due to a variation in the size, shape, and direction of an incision, which can have but very small influence upon the total result of cases. In the first section of his work, accordingly, as might have been expected, the author has produced little which would justify an ambitious publication. We find a recapitulation of the history of the operations, the description and figuring of the different varieties of instruments, especially the gorgets, together with a reproduction of the outline of the pelvic opening, with the relation to it of the superficial and deep incisions in perineal lithotomy, which we well remember as figuring, years ago, on the demonstrator's black board in the dissecting room, like the red outlines of muscular attachments which have since been utilised as the characteristic feature of one of the most used manuals on the bones.

In the hands of Sir Henry Thompson, however, it must be said that the outlines of the perineal walls and the relative position of the incisions in the various modes of perineal lithotomy, have been illustrated in such a manner as to give to the student a more clear conception of the somewhat confusing differences of these operations than in any other description with which we are acquainted. The essential peculiarities of the lateral, the bilateral of Dupuytren, the medio-bilateral of Civiale, the median, Italian, or Allarton's method, and the incisions made by Buchanan, are all very distinctly set forth, compared, and contrasted, in a clear, methodical, and orderly manner, and the accessory instruments of Teale, Corbet, and Avery, and the expanding staff employed by Wood, all receive acknowledgment at the hands of the author.

The author, however, takes care to express his opinion that most of these contrivances rank rather as surgical curiosities than as valuable aids to the operator; and he inclines strongly, in common with most experienced lithotomists, to a partiality for the most simple instruments, handled with the "tactus eruditus" which it is the privilege of the surgical expert to dilate upon with so much unction as "the priceless and incommunicable heritage of experience,"[1]

The curved external incision made by Buchanan in his operation (which was also employed by Wood) does not excite any especial remark from the author, although undoubtedly giving more room in a limited space than straight incisions. He considers Buchanan's operation to be essentially a median operation, though upon what grounds does not appear, since the superficial cut, as well as the incision in the left lateral lobe of the prostate, are both certainly placed in the area of lateral incisions.

The illustrations in this part of the work are, like the text and description, of a high order of accuracy and clearness. Especially we would call attention to Fig. 20, in which the manner of holding the knife and hands in performing the deep incisions of the lateral operation is excellently shown by Bagg. Upon this point much has been said and written as to the manner of Liston, of whom Sir Henry was one of the pupils, but he has not, apparently, considered this trivial peculiarity

[1] This privilege, however, we must observe, "en passant," scarcely justifies the departure from the reading and grammar of ordinary mortals which we find in the footnote at page 66 in Sir Henry's work. "Tactus eruditum" must surely be due to the imperfection of the subordinate mechanical aids, rather than to the regulating intelligence concerned in the printing and publishing.

worth making a coil about. In enumerating the parts cut in median lithotomy, we find the usually correct anatomy of the author at fault in the omission of the anterior insertion of the *sphincter ani* which passes forward over the central tendon in muscular males, even as far as to overlap the fibres of the *accelerator urinæ.* These fibres are undoubtedly severed in the median operation, and very frequently in the lateral when the median raphe is closely approached or transgressed.

In speaking of the *pre-rectal* method of Nélaton, the author justly considers that "it is difficult to admit that it possesses claims to be regarded as a new type, or, indeed, that it manifests much improvement on any old one" (p. 67). He looks upon it as a modification of the bilateral with the incisions placed further back, with the design of leaving the bulb untouched.

In the few cases in which this method has been followed in this country, the results, we believe, have not been satisfactory.

In children "the lateral operation is generally, and, no doubt, correctly, held to maintain its superiority, as a rule, over other methods," though the median and medio-bilateral are both admissible. "But, especially where the stone is large, the lateral operation affords the opportunity of making a freer opening, and in a direction in which the incision is less liable than that of the median to injure the seminal ducts" (p. 69). The author recommends the employment of one knife only, to obviate the difficulty of placing the beak of a second knife in the exact opening made by the first. The deep incision should be made with clearness and decision, and with sufficient freedom to admit the tip of the operator's finger with tolerable ease, otherwise he may drive the neck of the bladder along the staff, or slide the finger into the cellular interval between the bladder and rectum. In the performance of the median operation in children, Sir Henry uses a tapering, blunt gorget, with a probe point, to pass into the median groove of the staff.

Of the two or three modes of performing the *high* or *suprapubic operation,* the author prefers that practised by Civiale, done by the aid of the "*sonde à dard*" passed into the bladder; a curved and probe-pointed "*aponeurotome*" to divide the linea alba; and the blunt-hooked gorget, "or *gorgeret suspenseur,*" to hold the fundus of the bladder in its place after it is opened, and to protect the peritoneum, upon which its laterally convex surface is laid, during the extraction of the stone along its concave surface.

Chapter V treats of the very important subject of *The Causes of Death following Lithotomy.* In doing so, the author states that it has been the custom to regard the fatal contingencies of all ages in the mass without making distinction between adult cases and those of children; and he remarks with great justice that "nothing can be more deceptive than a method of dealing with the results of lithotomy, whether numerically or otherwise, by which cases of all ages are treated indiscriminately in one category" (p. 76).

We were certainly under the impression that this distinction had been for years almost universally understood among lithotomists, but would not venture to gainsay positively the assumption implied by the author that such a tacit understanding, if it existed at all, was first by him declared, "in taking views of the question which differ somewhat from those generally held respecting it."

In adults he places first among the causes of death, as the most frequent, "inflammation of the tissues, especially of the loose cellular tissue around the neck, base, and sides of the bladder, always of a destructive character, and generally with a tendency to extend to the neighbouring peritoneum. This is caused by mechanical violence, by want of reparative power, from erysipelas, and some have supposed by urinary infiltration through the deep incisions.

He states (page 77), "The great majority of authors affirm that infiltration of urine is the most common cause of death, a statement that I venture not only to call in question, but to regard as the source of serious error in practice." This error, he goes on to show, is that of making the deep incision extremely limited, and not beyond the limit of the prostate in any direction, so as to prevent the possibility of urinary infiltration into the post-prostatic areolar tissue. The consequence which has resulted is the tendency in the present day to incur the danger which attends an attempt to drag the calculus through an opening of insufficient size. What the author terms "the purely anatomical view now in the ascendant," of limiting the incisions to a safe topographical area, has, in his opinion, led to "the vital attributes and dispositions of the organs involved" not being sufficiently regarded, and hence the no less dangerous injury which results from violence inflicted by the forceps and stone upon the neck of the bladder, and from powerful traction upon it, which often irreparably damages the loose, delicate, cellular connections permitting of the varying conditions of size required by its function as a reservoir of urine.

He proceeds to say, however (page 79), "Let it not be imagined from these remarks that any one can deprecate more strongly than myself the making of an incision in the prostate more deeply than the size of the stone demands, but I am very certain that it is safer to extend the incision when the stone cannot be extracted without exerting violence, than to inflict the injury which such a proceeding necessarily involves."

He considers that the advocacy of small incisions by Scarpa and Sir B. Brodie as the sole or chief means of preventing infiltration, has greatly influenced professional opinion on this subject; and he quotes from the writings of Martineau, who cut eighty-four cases consecutively (including many children) with only two deaths (but who was afterwards much less fortunate), and of Cheselden himself—their approval of the practice of enlarging the opening by the secondary use of the knife, guided by the blades of the forceps, rather than that of using undue violence in extracting. The latter eminent lithotomist, however, also recorded his appreciation of the susceptibility of the neck of the bladder to great dilatation, which may be safely accomplished if not done too rapidly and forcibly. And hereupon Sir Henry alludes in forcible but just terms to the contemptible vanity felt by some operators of achieving a rapid operation in the eyes of the bystanders, and thus producing rupture and not dilatation.

He further observes: "I am strongly inclined to think that in many hands the forceps, and not the knife, is the most deadly instrument employed in lithotomy;" and he fortifies his position by quoting from Pouteau, who wrote a century ago to Frenchmen, "I am persuaded that the thoughtless anxiety to acquire this false glory, that the public attaches to rapidity in operating, has killed more patients than any other evil manœuvre."

Keith, of Aberdeen, and Humphry, of Cambridge, both successful operators, add testimony in these pages to the importance of employing sufficient time and gentleness in extracting the stone, and Sir Henry shrewdly remarks that, while operators are apt to attribute success almost entirely to various peculiarities of the instruments or methods used, "one and all agree in the vital importance of extracting the stone with great care and gentleness, and of giving time in abundance to this part of the operation." It must always be borne in mind, however, that the degree of force used by the operator in extracting depends, not so much upon the opinions they express in recording their accomplishments, as upon the nature and degree of the resistance offered to extraction, and upon their accurate perception, and perhaps, somewhat, upon their actual possession of the wrist and arm power which they may be tempted, almost unconsciously to themselves, and still more to the spectators, to put forth in any individual case. The varying relations of the size, shape, and character of the stone to the age and development of the patient, and the form and direction of the incision, as well as of the extracting forceps, make, with the coolness, temper, and power of the surgeon, a complete problem of forces which vary in their effects upon the implicated viscera with each individual case; while the recuperative power of the patient, and his greater or less tolerance of violence, add still further to the uncertainty which hangs over the ultimate result in all cases of lithotomy.

The cause of death placed by the author next in order is rapidly spreading inflammation produced by urinary infiltration into the cellular interspaces between the pelvic viscera, when they have been opened up by too deep incisions. Here we have the Charybdis of the foregoing Scylla. But, says Sir Henry, this occurs less frequently than is usually supposed. The broken-up cellular connections, the sloughs bathed in seropurulent and urinous fluid, and the peritonitis which have been found after extraction of a large stone, are due, he thinks, in most cases, primarily to inflammation leading to urinary extravasation, and not to these phenomena occurring in the reverse order. Infiltration, he argues, by no means necessarily occurs when urine passes over newly made sections of so-called cellular spaces, which really do not exist except when made by the operator; and he asks, "How, if it were a fact that these cellular interspaces could not be cut (in lithotomy) without the gravest risk to life, could forty or fifty cases have been operated on without a single casualty?" We find, however, that in his able advocacy of "a doctrine which he supposes to be opposed to the generally received notions on this subject," Sir Henry has entirely omitted to take into consideration the influence of the levator ani in obstructing the free escape of urine from the bladder to the surface of the wound, as well as that of the position in which the patient reclines after operation, in directing the flow.

If the sub-prostatic portion of the levator ani be cut at all by the incisions, and is not cut freely enough, it is evident that by its subsequent involuntary and sustained contraction under irritation for the few days after the operation, the urine will be prevented from passing freely out of the wound, and will be directed above the muscle towards the dangerous proximity of the peritoneum through the deep pelvic fascia, a direction downwards and backwards, towards which it has already a tendency from the action of gravity. It is well known that the smallest obstacle has a tendency to effect the most important changes in the course of fluids of all kinds.

If, during the section, the muscle be much relaxed (as may well happen under chloroform), it is, like all muscles, apt to yield undivided before the edge of the knife, and so it may easily happen that it is not cut to nearly the same extent as the more resisting prostate and its fascia; its subsequent contraction after the operation then leads to all the mischief. But if the capsule of the prostate is not divided at all posteriorly, its firm, close-grained, and resisting texture prevents the flow of urine in this dangerous direction, and turns it forward through the anterior opening of the levator into the wound. The deduction which obviously offers itself to us through these considerations is, that if the levator ani be cut at all it ought to be cut freely, but that an operation certainly may be done in which neither it nor the posterior part of the prostatic capsule are cut at all. The latter kind of operation would include all the median and the more limited of the lateral operations, which are combined with dilatation of the prostate, and the former the free lateral and bilateral operations. Another obvious inference seems to be that the former class of operations is adapted for the smaller stones in the adult, and the latter for the larger. And here we have again an important practical classification of these operations. On the other hand, the greater advantages of lithotrity in case of small stones will, in future, lessen very much the relative importance of that cutting operation which is in the adult proper for small stones only. With respect to the argument drawn by the author from the significant fact of the rarity of the bad consequences of urinary infiltration in children, he does not seem fully to have appreciated the bearing of the milder and less irritating character of the urine in them, and the fact that the levator ani, from the smaller size of its anterior opening, and its relative direction from the obliquity of the pelvis, is unavoidably cut freely in the lateral operation.

In the cause of death placed third in order, we have pelvic cellulitis from erysipelas, and unhealthy inflammation in a weak subject. We find, then, under another numeral division, inflammation of the mucous membrane of the bladder and kidneys, the latter ushered in by rigors, leading to suppression of urine, and terminating in death within a few days or hours. From simple cystitis the author traces the occurrence of that gradual and insidious form of peritonitis which affords the very few examples of recovery which do take place after peritonitis.

Phlebitis and pyæmia are placed low in the list of frequency after lithotomy in this country, but it is found to be in Bengal, we believe, one of the most frequent causes of death after lithotomy.

Shock, as distinguished from rapidly fatal cases of blood poisoning and hæmorrhagic exhaustion, is a rare result. Hæmorrhage (secondary) and exhaustion (as most frequently caused by bleeding) are coupled together; and tetanus is placed the eighth and last of the causes of death after lithotomy in adults.

The most frequent cause of death in children is considered by Sir Henry Thompson to be peritonitis, which he explained as the consequence of the bladder being more an abdominal than a pelvic organ, and more completely and intimately invested by the peritoneum.

The peritonitis is not usually induced, as in the adult, by extension through the cellular tissue at the neck of the bladder, but is the direct consequence of undue manipulation of instruments in the bladder itself. Crosse, and Fletcher, of Gloucester, both give instances of such fatal results by the prolonged use of a sound only searching for stone.

From this it results that lithotomy is not so successful during the first three or four years as between six and ten years of age.

Among the difficulties which are peculiar to the age of the patient treated of in Chapter VI (page 99), are those which arise from the soft, yielding, and lax tissues which form and connect the pelvic structures.

The staff is liable to be pushed out of the urethra between the bladder and rectum, if not carefully directed along the sharper upward curve which the urethra takes in children behind the triangular ligament. The only absolute safeguard against cutting for stone upon a staff which has not reached the bladder at all known to the author "is to require clear, audible, or tactile proof of contact between the stone and the staff on which the patient is to be cut." He explains that the stone may be found with a sound, and then, directly afterwards, altogether missed by the staff in consequence of the different shape of the instrument (p. 100). Another difficulty is the facility with which the membranous urethra may be separated from the prostate in children in an attempt to dilate with the finger, or in consequence of repeated cuts or notches made in the attempt to pass the knife into the groove of the staff, as pointed out by Sir W. Fergusson.

Rigidity of the neck of the bladder and enlargement of the prostate are difficulties found at the other extreme of life. To meet the former, a second cut is recommended by the author (following the practice of the late Mr. Liston) on the opposite side or lobe of the prostate. The latter difficulty he is not disposed to rank high in the scale either of difficulty or danger, since deep incisions into an enlarged prostate, or even removal of a considerable portion of its substance, are almost comparatively free from danger. He recommends the gorget as a safe and efficient instrument for extending the deep incision in such cases, because it forms a guide to the forceps, and keeps out of the way of extraction any projecting lobes or tumours.

The difficulties arising from a deep perineum, caused by the bony formation of the pelvis, or the superficial deposit of fat, and those from a narrow, deformed, and ricketty pelvis, are briefly dismissed; while those from unusual distribution of arteries, as the accessory pudix, lying close to the side of the prostate, and others, receive little more than a bare mention.

The third class of difficulties are those which depend upon peculiarities of the stone, or its relations to the bladder itself. Notwithstanding the usual acceptance of the dictum of Crosse, of Norwich, that after moderate dimensions are exceeded, danger and difficulty in extraction are in proportion to the size of the stone, *i. e.*, to the extent of the incisions and the force employed, the lateral operation in Crichton's practice showed remarkable success in such cases. Out of eleven cases of stone weighing four ounces and upwards to eight or nine ounces, all in elderly and aged men, only two died. His method was marked by freedom of incision and care in extraction. In two cases only was the stone broken up during extraction.

The author gives woodcuts of three instruments for crushing and breaking up large calculi after lithotomy, and states that he has been impressed in witnessing their use, 1st, with the liability to injure the bladder in the requisite manipulation and 2nd, in the removal of the numerous angular fragments; 3rd, with the danger of leaving a small fragment in the bladder unless carefully washed out.

He considers that in a case where there were two stones locked together, each very large, weighing together 12½ ounces, the bladder habitually contracted upon them, and the urine trickling off continually, the recto-vesical operation would have given the best chance of success, as permitting the largest opening with the smallest amount of risk.

Among the unusual examples of encysted calculi given in the book is one of a very remarkable character in a man aged fifty-one years, operated on four times by Dr. Humphry, of Cambridge, before the encysted condition of one of the stones was clearly made out. The recto-vesical operation finally enabled Dr. Humphry to reach the cyst, which was just above the entrance of the right ureter; and, by opening the sac with a hernia knife, to extract a stone about the size of a walnut. Death unfortunately occurred two days after, from peritonitis. In a second case of the same unusual character, Dr. Humphry emptied the cyst of a softish material with a scoop, and the patient recovered. One other encysted stone was successfully removed by Cadge, of Norwich, two by Crichton, and one by Lawson. One case was operated on by the author, in a gentleman aged seventy-four years, who died one month after the removal of numerous phosphatic fragments and an unsuccessful attempt to remove an encysted uric acid stone (p. 121).

In Sir Henry's opinion, the spasmodic or hour-glass constriction of the bladder, mentioned by Louth, Brodie, and Gross, is exceedingly rare. He alludes, however, to an example of this condition in the museum of the Royal College of Surgeons, in which a stone is lodged above the pubis and held there by this peculiarity in the form of the viscus, in such a way that it could not possibly have been removed by perineal lithotomy.

Three well-authenticated cases of adhesion of the stone to the vesical mucous membrane, and incorporation of some fibres of the latter in its substance, are given by him from the practice of Messrs. Nunn, Van der Byl, and Henry, of the Middlesex Hospital.

Wound of the rectum accidentally in the operation is not uncommon, and does not, even when large, in Sir Henry's opinion (coinciding with that of the best authorities), necessitate the immediate division of the sphincter ani, recommended by some. It had best be left alone, and treated afterwards, if necessary, as a recto-vesical fistula.

Removal of a portion of an enlarged prostate during lithotomy has been effected by Dr. Keith nine or ten times, as well as by Key and Civiale, and no bad results have ensued beyond a little delay in healing in some cases. On the other hand, it has often benefited the patient. In effecting it, Sir Henry recommends the use of probe-pointed scissors rather than the knife or by tearing.

In the cases in which, by an oversight of the operator, fragments or whole stones have been left in the bladder, he recommends, if they are small, the use of the lithotrite; if large, redilatation of the wound. In those in which phosphatic incrustation of the wound proves very troublesome, the use of an acidulated wash with hip bath, seconded, if necessary, by the employment of the forceps or scoop.

In dealing with the perineal fistula, which occasionally remains after lithotomy, the author considers that the plan of keeping a catheter tied in the urethra is not successful, in consequence of the urine invariably making its way by the side of the instrument, and the purulent discharge which is excited by its irritation. He prefers to pass a catheter each time the patient needs it.

Sexual impotence after lithotomy, which rarely occurs, and has been attributed to division of the seminal duct, Sir Henry considers to be rather caused by inflammatory plastic affusion in the prostate and its appendages, or from sloughing, brought on by violent extraction.

Incontinence of urine happens more frequently in cases under the age of puberty, and often no clear explanation of its cause can be given. One case, following the extraction of a large stone, was cured by the author by cauterisation of the neck of the bladder.

An unfortunate accident which occasionally occurs, viz., inability to find a stone in the bladder when the incisions are made, may result from a small stone being washed out of the wound by the gush of urine. This, the author judiciously remarks, ought not to happen, because so small a stone ought to be crushed, and not cut for. Sacculated bladder and cysts account for some of these cases.

In others, no stone has ever existed; polypoid growths, phosphatic encrustations, hardened and rugose vesical walls, and even the brim of the pelvis or spine of the ischium, have given rise, when struck by the sound, to the sensation of a foreign body or rough stone. Such cases have been recorded with praiseworthy candour by Paget, of Leicester, and Gutteridge, of Birmingham. Sir Henry Thompson is of opinion, after several experiments, that such sounds or sensations are not at all comparable with the click or note elicited upon the sound by any calculus which he himself ever encountered in the bladder. We suppose that he does not intend to include in this remark cases of real phosphatic incrustation.

Results of lithotomy.—Chapter VII. The more prevailing use of lithotrity in modern days has modified very much the statistical results of lithotomy in the more grave and severe cases to which, in adults, it is now restricted.

With much perseverance, labour, and diligence, the author has collected nearly 2000 cases, the authenticity and conditions of which rendered them, in his opinion, fair cases for argumentative deduction.

The most favorable results (with all cases taken together, young and old) are those from the records of Addenbrooke's Hospital, Cambridge, viz. 183 cases with 13 deaths, or about 1 in 14. Next comes the Leicester Infirmary, 90 cases with 8 deaths, or about 1 in 11. Then the Birmingham Hospital, 102 cases with 10 deaths, or about 1 in 10. The hospitals at Norwich and Oxford yield about 1 in $8\frac{1}{4}$ to 1 in 8, while the London and Leeds hospitals average from 1 in 7 to 1 in $7\frac{1}{2}$.

The author points out, however, that the proportion of children in the Norwich and Cambridge hospitals is rather higher than the average, implying some explanation of their better results; while, on the other hand, Guy's Hospital has also an unusually large proportion of young cases, and yet the results were 1 in 7. He then tabulates the cases which he has collected, in groups of years, according to the age of the patient operated on (p. 142), and obtains thus the result that out of 377 cases between the ages of six and eleven years (inclusive), the deaths were only16, or abont 1 in $23\frac{1}{2}$; while of 473 cases aged between one and five years, the deaths were 33, or 1 in $14\frac{1}{3}$ cases; at between the ages of seventeen and twenty (inclusive), the death rate rises to 1 in 7, indicating the increased susceptibility of the generative and urinary organs attending upon the development of puberty. The next favorable death rate is between the ages of thirty and thirty-eight, viz. 1 in $10\frac{1}{2}$; then from twelve to sixteen, 1 in $9\frac{1}{2}$; while the most fatal, as might have been expected, is in extreme old age, seventy-one to eighty-one, viz. 1 in $3\frac{1}{6}$.

The great group of infancy and boyhood up to puberty at sixteen years, comprising more than half the entire number of cases, shows a total mortality of 1 in $15\frac{1}{2}$, while that which is taken from all ages after this important vital change gives a mean proportion of one death in every five cases; a very striking difference, to the disadvantage of adult years, in this operation.

Sir Henry anticipates, in the future, even a larger proportion of fatal cases in adult lithotomy, from the selection of all the more favorable cases and smaller calculi for the less dangerous operation of crushing. Lithotomy will be performed on selected bad cases, while lithotrity will at the same time become responsible for some difficult and unhealthy cases, and will have a higher death rate per cent. than at present; but that a more favorable sum total of stone cases will be the effect of a more prevalent recourse to crushing.

From Allerton's record of cases treated by the median operation, the author calculates the mortality at 1 in 11 cases at all ages; but he considers that this operation, being reserved usually for stones known or believed not to be large, should yield a better result than lateral lithotomy.

He states that his own experience of the median method is not favorable, troublesome bleeding often occurring, and the rectum being easily wounded, either by the knife or tearing in extraction. On these grounds (which, we must observe, are scarcely sufficient) he has discarded it entirely for the lateral operation.

The death rate of the supra-pubic operation is calculated by Dr. Humphry, of Cambridge, from 104 authentic recorded cases, to be 1 in $3\frac{1}{3}$.

In the careful analyses of cases given by the author, one thing strikes the reader as being conspicuous by its absence, and that is a detailed account of the cases of lithotomy operated on by himself. This is the more remarkable when contrasted with the copious and detailed notes of 204 cases of lithotrity given to the reader's inspection at the end of the second section of the volume treating of that subject.

A critical reflection upon the reasons for this remarkable omission has given occasion for the expression of an unfavorable surmise which we have heard but will not here express; but which Sir Henry would have done well, we venture to suggest, to have avoided in the last, as we hope he will do so in future editions of his valuable work.

That portion of the volume which treats of lithotrity assumes, in Sir Henry's hands, an interest and importance which forbid an attempt to criticise it in the present pages, for want of space to accord to it attention commensurate with its value. We propose to refer to it in our next number.

X.—Lombroso on Pellagra.[1]

As long as the theory of Liebig on alimentary matters, dividing them into plastic and calorific, was held irrefutable, and before the attention of pathologists was drawn to the importance of chronic zymosis in producing poisons, our Italian brethren, who are so deeply interested in pellagra, were right, probably, in assigning the first place, among modern etiologists of the disease, to Lussana. In view of the conjectures of some speculators, his generality of attributing it to insufficient plastic alimentation seemed to the admirers of common sense to come nearest to the truth. The maize, which is the staple food of the Lombard peasantry, was pronounced even when sound, but especially when damaged, to contain too little nitrogenous matter for a labourer's dietary; and hence the inference, that the proper cure for pellagra is flesh food and a more nutritious grain-stuff than maize. The obvious imperfection in this argument is that, while insufficient nutriment is, unhappily, very common in most lands, and maize is very extensively used in warm climates, yet pellagra, on the other hand, is confined to a certain number of well defined districts in North Italy. In these its ravages are very melancholy, and when the traveller, through parts where it is most developed—say the plains of Caprino or Rivoli—sees the fields absolutely left uncultivated

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[1] *Studi Clinici ed Esperimentali sulla Pellagra.* Del Dott. CESARE LOMBROSO. Memoria Premiata dal R. Istituto Lombardo. Bologna, 1871.

Clinical and Experimental Observations on the Pellagra. By Dr. CÆSAR LOMBROSO. A Prize Essay of the Royal Institution of Lombardy. 1871.

through the incapacity of the pellagrosed inhabitants, he feels that the root of the evil has yet to be struck. And he will hail with peculiar pleasure the direction to this object of the newest means of investigation possessed by science. In answer to a prize, offered by the Royal Institution of Lombardy, Dr. Lombroso has aimed at specialising more accurately the immediate cause of the disease, as distinguished from those general causes which weaken the resisting force of poor populations, and make them more liable than the robust and well-fed to suffer from injurious influences. And we think the step he has made is an important one.

Though he has not succeeded in absolutely isolating a poison capable of producing such deleterious results, he has found evidences of its existence in damaged maize. It is associated always with the growth of *penicillum glaucum* (blue mould) on this breadstuff, and with the consequent or antecedent fermentation thereof; it is capable of solution in alcohol and water, resists the destructive agencies of cooking heat, and of panification, and is rendered inert, as far as experimented upon, only by boiling with quick lime. The *penicillum glaucum* is, however, not itself the poison; for, when grown on other substances, it seems to be practically innocuous. This poison, experimented upon in its alcoholic solution, produced, when administered to well-fed persons for a short period, eructation, pyrosis, languor, sleepiness, diarrhœa, nausea, unnatural appetite, thirst, lumbar pain, sweating, giddiness, itching of the skin. In less well-fed individuals the symptoms were more certainly produced, and were more severe. The above named were intensified, especially the cutaneous symptoms; there was discoloration, burning, and desquamation. The nervous phenomena were serious; there was dilatation of the pupil, ptosis, giddiness, irritability with loss of affection, and hysterical crying. There was also exhibited in some of them that strange desire for plunging into water (hydromania), which so often characterises cases of pellagra. These symptoms were brought on sometimes after four doses, sometimes not till after sixteen doses, but were absent in about one sixth of the subjects experimented upon. The same damaged maize, when given for still longer periods to chickens, brought on emaciation, loss of plumage, rejection of food, diarrhœa, langour, and death; iu rats, emaciation, reddening of the skin, muscular spasms, choreic movements, and death. The poison is thus shown to be of the nature of common poisons, and not to have the power of zymosis or reproduction, like morbid infections; it acts in proportion to its dose and to the length of continuance, and produces a different series of phenomena in different individuals.

Now this is just what happens in pellagra; short or partial exposure to the exciting causes (as in the case of temporary residents) does not induce it; different persons suffer in different ways; even different localities suffer in different ways; thus, in the district of Verona, anomalies of the pupil, lumbar pain, and affections of the capillaries of the skin are most frequent, while maniacal and scorbutic complications are rare. In the Milanese, on the other hand (in whose magnificent hospital strangers have commonly been introduced to pellagra), the epileptic and idiotic forms are those most usually seen. In Pavia, contractions of the limbs; at Trent, albuminuria and consumption; at Reggio, scurvy; in Tuscany, pterigium. In almost all, however, there is exhibited that singular union of a cutaneous disease with deficiency of nervous power, which is characteristic of pellagra. The cutaneous symptoms sometimes take the form of eczema or herpes, but more usually consist of a darkening of the skin of the whole body. In this variety of symptoms, with a general pathological relation to one another, the resemblance of the disease to the experimental poisoning is obvious. Exact proof of identity could only be had by using unjustifiable tests; but as evidence is gained of a poison which will produce somewhat similar phenomena, and that poison can be shown to be especially widely diffused in places where pellagra is prevalent, the argument is sufficiently complete to sanction its present acceptance and justify deductions therefrom.

Dr. Lombroso's observations and experiments both show that it is not the occasional, but the habitual, use of the mouldy maize which induces pellagra; and this is an answer to the question why, considering the liability of all breadstuffs to decay, the disease is not more widely spread. Our peasantry are, perhaps, supplied oftener than is right with musty flour, but they do not prefer it for the "nutty" flavour of *penicillum glaucum*, like some Piedmontese villagers. They are free also from the belief, which Dr. Lombroso found general, that baking would cure flour however unwholesome. Again, in most sane populations, we should expect the farmer to sell, if possible, his damaged grain, while he has the best ground for his own use; whereas it seems that the Lombard does not send it to market, partly because it is of low value, partly for fear he should be detected in having stolen it: and he does not give it to the fowls because it makes them ill—so he eats it himself! It really requires a special combination of circumstances to produce such a mental phenomenon. These circumstances are, in the first place, the climate, which is sufficiently warm to grow maize, but not sufficiently regular to ripen it with certainty as (within the tropics), so a great deal of the crop is often spoiled. Then the

Northern Italian is hereditarily a hoarder, and does not mind how hard he works and lives, especially the latter, if he can secrete a penny by it. But, above all things, the English philanthropist will probably accuse the barbarous custom of letting land on the metaire system, according to which the rent is paid by a certain share of the crop. A temptation is offered, greater than an avaricious man can resist, to cut too soon, and store the pilfered grain in ill-ventilated corners, where it soon gets musty, and, if brought out and confessed to, would convict the possessor of fraud. So he eats it, and is poisoned accordingly. Curiously enough, Dr. Lombroso does not see the unsuitability of this southern custom to an uncertain climate; he seems to consider it the normal condition of land tenure. But if, as he urges, the Government is to take any action in arresting the ravages of pellagra, we are convinced that in no direction could they promote beneficial reform so surely as in discouraging the metaire system, and facilitating the granting of leases. Properly ripened maize is such an economical food that they must think twice and again before they throw any impediments in the way of its cultivation.

The *post-mortem* appearances in those who have died pellagrose indicate an atrophic degeneration of all the soft tissues, distinguished more than usual by pigmentation. This is combined with local hyperæmia, and special affections of the brain and heart. The pia mater is thickened, and the cardiac muscles softened and discoloured brown, much more commonly than in an equal number (sixty-six) of other victims of chronic disease.

In its old haunts, pellagra would seem to have much increased during the present generation. In Lombardy the numbers affected were reckoned in 1839 as 20,282; in 1856, as 38,777. In Venetia it would seem to be checked by the better condition of the inhabitants, and their seafaring habits; in Bologna by the use of wine. But Dr. Lombroso notices its increase northward at Trent, and the occurrence southwards of as many as twenty-two cases in the Campagna di Roma—namely, at Palestrina and Capranica. He also points out that the *mal de hidago* of certain parts of Spain presents the same symptoms, apparently referable to the same cause; and that in the barracks of Paris in 1831, in the Belgian prisons in 1846, in British India in 1835 (described by Malcolmson of Madras), and again in the Crimea in 1855, there were observed peculiar congestions of the extremities, associated with nervous symptoms, which seemed traceable to damaged breadstuffs. So that every lustrum makes pellagra more and more our business—*tua res agitur, cum proximus ardet Ucalegon.*

XI.—On Neuralgia and Functional Nervous Disorders.[1]

Although two authors of the eminence of Dr. Handfield Jones and Dr. Eulenburg have written books under the title of 'Functional Nervous Diseases,' yet we are not by any means sure that the title is a good one. Nor is the matter of title only a matter of taste; on the contrary, any haziness in this respect is likely to cloud the pages which follow and any inconvenience in naming of the subject will make itself seen in the arrangement of the subject itself and in the manner of its treatment. For illustration's sake, let us apply the same kind of title to affections of other parts, for instance, of the respiratory system. Should we be satisfied with the title of functional diseases of the respiratory system, or again of the abdominal or nutritive system? A treatise on functional diseases of the respiratory system would contain a long and very interesting chapter on cough, another on expectoration, another on hæmoptysis and so forth; but should we not all perceive that neither cough, expectoration, nor hæmoptysis were diseases, but were rather symptoms, and should we not feel that to apply the name disease to a symptom was to introduce confusion at the outset? Or, to look at the matter from another side, such chapters might not contain an account of all coughs and all expectorations but only of such coughs and such expectorations as were seen in 'functional' disorders, the word 'functional' being then used in the loose sense of transient disorders, or disorders not depending upon any permanent lesions. Such an interpretation however is not likely to be acceptable to either of the distinguished authors we have mentioned. They would probably say that they wished to contemplate the various changes in living phenomena which primarily depend upon alterations in the nervous system; such a change in muscular action, for instance, as depends not upon change in muscle itself but upon alterations of its nervous supply, and so forth. But would not discourses of this kind be better named if they were entitled 'Symptoms of Nervous Diseases,' or the 'Clinical Aspects of Nervous Diseases,' which would signify that the subject was considered rather from a clinical point of view than from that of pathological anatomy. In truth, however, it is impossible to consider the symptoms of nervous diseases apart from their pathological anatomy, apart, that is, from the changes of which they are the expression.

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[1] 1. *Neuralgia, and the Diseases that resemble it.* By F. E. ANSTIE, M.D., &c. London and New York.

2. *Lehrbuch d. functionellen Nervenkrankheiten.* Von Dr. ALBERT EULENBURG. Berlin.

Where do symptoms end and where does pathological anatomy begin? or where do the phenomena of life end and those of physiological anatomy begin? Take again our illustration of diseases of the respiratory system; of these, cough is a symptom, so is expectoration, but is cavernous breathing a symptom or is fine crepitation? Whether these latter are to be called symptoms or not, they are nevertheless nothing more nor less than pathological anatomy, and the surgeon who examines the lungs or heart by the usual physical means is doing no less than making an imperfect autopsy before the time. In affections of the skin again the dermatologists who describe symptoms do in the main describe what can strictly be called their pathological anatomy. So by such arguments we seem to come to this, that symptoms are changes which we can see, and pathological variations are changes which we cannot see; a distinction which scarcely appears to be essential. Whether a certain change is to be called a symptom or not, whether, that is, Dr. Jones or Dr. Eulenburg are to deal with it or not, depends only upon the degree to which physical exploration has been carried at the time of writing; and, indeed, in no books do we find a greater effort to supplement the seen by imaginative conceptions of the unseen. This confusion always gives us some sense of discomfort in dealing with these otherwise admirable works. If any one will say that to demand a distinction between diseases and symptoms is to return to ontological conceptions and to desert the truer ways of registering sequences, we would reply that vagueness is not a necessary consequence of the demolition of symmetrical falsities; on the contrary in the midst of the bewilderment of ruin we are the more bound to know exactly what bearings we have. What then do we mean by the word disease? Without giving an exhaustive and short definition in reply, we may explain that by a disease we mean not a symptom but a group of symptoms which repeats itself with something like uniformity, both as regards the mutual relations of its component symptoms, and as regards its relation as a whole to other groups occurring under like but not identical conditions. As a matter of fact, we find that symptoms do group themselves with consistency and uniformity in most or all instances, as we find that the groups of phenomena which constitute animal life group themselves uniformly, producing new dogs and cats, new horses and snakes which we can recognise as part of our past experience. Were it otherwise we should find it almost impossible to describe human sufferings at all, and the body of English physic would only be a mass of undigested instances like the body of English law. It is clear however that we must include the pathological phenomena with the symptomatic, as a naturalist must combine description of species with their comparative anatomy; a dog is not merely an animal having certain external relations with the world but also a certain internal structure upon which this outer structure depends and with which it is in essential unity. A disease, therefore, is a collection not only of external phenomena but of internal phenomena likewise, the degree in which these latter are phenomena to our senses depending only upon our powers and opportunities of investigation for the time being,

When therefore we find a series of variations from the normal state, whether these be deep or superficial, and if these variations tend to recur in something like the same order in different persons we designate the series by a name and call the whole a disease. To take some selected members from such groups and to compare them is quite justifiable, but as we do it we thereby break up this conception of a disease as a whole. For instance, cutaneous anæsthesia is seen both in locomotor ataxy and also in myelitis, which two names represent each of them a tolerably definite and recurring group of phenomena; we may separate in our own minds the anæsthesia from each and from many other states, and consider the history and modes of this symptom taken thus separately, but we are not then describing a disease; on the contrary we have dismembered several diseases to obtain our subject, just as to obtain a study of stamens we should dismember a number of flowers. But, the reader will say, surely this is a refinement upon terms and upon mere terms, to which we answer both 'yes' and 'no.' What can, in the first place, be more absolutely necessary for accurate reasoning than a minutely precise limitation of the meaning of the words which we use. Such precision in argument is as necessary as is the accuracy of the engraver in the currency of bank notes. If no two persons attribute the same value to the tokens which they use how can any definite understanding be preserved? But again there is something more than mere wording in the matter; there is a danger to the truth of the very conceptions themselves, and it is this danger which at first led us to make these reflections. Dr. Anstie has made it quite clear to us that a great deal depends upon the term to be applied to neuralgia, whether indeed this morbid change is to be called a disease or whether it is to be called a symptom; for, independently of the accuracy of conversation, we cannot doubt that to change our term from symptom to disease in speaking of neuralgia is in some subtle but very wide-reaching and important way to change also much of our familiar thought about that affection. The thing itself is presented to our minds much as before, but our conceptions of it and the relations which it seems to bear to other things undergo a transformation. It is now become the centre of a group of subsidiary phenomena, to which group, as a whole, the name neuralgia is applied; before, it was itself but a dependent among other symptoms. The consequences of regarding neuralgia as an accidental rather than as a primary event are shown in Dr. Anstie's volume to have been unfortunate both from a nosological and from a therapeutical point of view. Nosologists have neglected to give it any adequate definition and have allowed it to remain confused with other and different disorders; while therapeutists in like manner, regarding it as a symptom, have preferred to treat it as an outlier of some more general ailment, rather than to direct their attention to it as a main elements and connoting other members of a uniform group of changes. No better example of the difference of the two points of view can be had than that which is offered by the two books before us. Dr. Eulenburg criticises Dr. Anstie's opinions somewhat adversely and Dr. Anstie complains that he is misunderstood; so indeed he is for the two writers approach neuralgia from different points of view; Dr. Eulenburg approaches it with the confusion of mind which is reflected in the title of his work, while Dr. Anstie, whether right or wrong, has taken a distinct side in the matter and proclaims that neuralgia is a disease as angina pectoris, asthma, or epilepsy are diseases. In fact, we are reminded of the position Dr. Russell Reynolds took up in his work on epilepsy, the drift of which was and is that, although convulsion like epilepsy is a symptom, yet that epilepsy like neuralgia is a disease, because it consists of convulsions occurring in definite ways in different people and having tolerably constant relations to other phenomena.

A shrewd observer once said to the writer, 'When I was a boy people used to have headaches and toothaches but now all the world has neuralgia.' From this superfine inaccuracy Dr. Anstie desires to rescue us, and he has written a very forcible, acute and practical book to prove that neuralgia is not to be taken merely as a fine word for pains and aches of whatever kind and of whatever origin, but that the term is to be restricted to a certain group of phenomena of which a pain is the chief, which occur in a uniform way in different persons and which bear in their entirety some tolerably constant relation to other groups of changes presenting themselves in the same person or in his kindred. The principal attribute, he says, of all these groups is, that they spring directly from a defect in some part or parts of the central nervous system, which latter may have suffered under some injurious influence, or which has more probably inherited some implicit weakness. Neuralgia occurs in various nerve districts, or is replaced by motor or other functional disorders, not because the central weakness differs in kind, but because the points of least resistance differ in different persons or vary with the years and circumstances of the same person. As to the state of the centres thus imperfectly resisting and of the parts which are weakest opinions differ, but Dr. Anstie expresses a strong belief on this the speculative portion of his treatise. Whether he accepts Dr. Radcliffe's theory of pain and spasm in its completeness is not quite evident, but he is strongly convinced that the central derangement which allows of the establishment of a morbid point of least resistance is due to defective nutrition, and that the appearance of pain is not due to an excited state of nerve in the sense of over activity accompanied by hyperæmia, but on the contrary is due to a state of under activity or atrophy accompanied by anæmia. He also contends, as we have said, and seems to prove that this pain is due to a defect in the nervous centre rather than in the course of the nerve springing from such centre. Our own impression is that Dr. Anstie is original in this hypothesis; we have not looked the matter up but the hypothesis struck us as original when first proposed in 'Reynolds' Medicine' and it certainly is not accepted as the usual teaching. Neuralgia has always been spoken of as an affection of the nerve in its course and, in many or most cases, as an irritation of such nerve in its length by an irritant either physical or chemical—by a morbid growth, say, by cold, or by some acid or other injurious substance circulating in the blood. Dr. Anstie, with a boldness which at first seems rather surprising, attacks this belief root and branch. He says I do not believe much in your peripheral irritants; gout is gout, rheumatism is rheumatism, syphilis is syphilis and none of them are neuralgia. Neuralgia is very rarely set up by any peripheral cause but, like insanity, depends upon a local and inherited defect in a particular centre or in several nerve centres. Toothache, he contends, is toothache and no more, unless it occur in a person predisposed to neuralgia when it may hit the latent blot and set up a true neuralgia or tic douloureux; while the pains of locomotor ataxy, again when duly considered with regard to their central causation, are in favour of his view and not against it. On the other hand consider, he says, the vast number of persons liable to peripheral irritations, such as cabmen who are exposed to all weathers, and compare this number with the few among them who suffer from any consequent neuralgia. In fact, setting aneurisms aside, which present some difficulty, Dr. Anstie's astonishment is reserved rather for the rarity of neuralgia as a consequence of any action, irritant or other, attacking a sensory nerve in its course. And in the main we are tempted to concur with Dr. Anstie. On due consideration it certainly does seem that the accidental causes of neuralgia bear but a small proportion to the inherited causes, though we think this may be pressed too far as we do undoubtedly meet with important cases of the former kind. For a good example we may, indeed, refer to a case reported by Dr. Anstie himself in the fourth volume of the 'Clinical Society's Transactions'; in which case severe trigeminal neuralgia was set up in a person, not predisposed, by overwork of the eyes. As a rule, however, the more we inquire the more we shall convince ourselves that neuralgia is a matter of hereditary predisposition rather than of individual conditions, and we find an illustration of this in a manuscript note of our own upon the freedom from neuralgia in many women in whom flooding, leucorrhœa and the like had led us too confidently to expect it. Our own view of neuralgia, which though less definite than that of Dr. Anstie is perhaps worth recording, has long been that neuralgia occurred in persons of certain constitution to whom arsenic is especially wholesome, and that it is therefore associated with some skin affections such as certain forms of eczema and psoriasis, with asthma, gastralgia and some other disorders which also acknowledge arsenic as their master. Here Dr. Anstie is with us also, but he has gone a step further in telling us the seat of the defect in such persons. Both from the point of view of classification and also from the anatomical side Dr. Anstie's remarkable book is full of light and instruction. Anatomically speaking, to place the defect in trigeminal neuralgia, for instance, in the medulla, is true in the sense of having much explaining power. The simultaneous or successive occurrence of disturbances in nerves rooted in the same parts are thus explained. Take, for example, that of a lady now under our own care who suffers at once, or within short periods, from intense trigeminal neuralgia, from cardiac disturbance, from spasmodic dyspnœa and from trophic palsy of the lung with bronchitis and pneumonic conditions of a capricious kind. Take again another case of a lady whose lungs present the same liability to trophic disturbance and who was for some years subject to occasional alarming attacks of painless stoppages of the heart with sudden faintness, the phenomena being identical with those experimentally induced by irritation of one branch of the vagus, and having now definitely retired in favour of spasmodic asthma. This lady has no trigeminal neuralgia but this affection occurs in others of her family, she herself is liable to distressing flushes of the cheek and ear, and her son has had many unaccountable attacks of swollen face without pain, coming and going in apparent caprice; he has had also several attacks of congestion and erythema, like erysipelas but non-febrile or nearly so, around the right eye. Such cases as these are readily explained on Dr. Anstie's hypothesis, for a defect in one small central district would make itself felt in these several peripheral directions. The next question which occurs to us, one upon which some conflict will probably arise, is concerning the state of the nerve or its centre during a neuralgic paroxysm. The conflict will probably lie between the advocates of hyperæmia or excessive action and those of anæmia or deficient action. We are disposed to think with the publican in Silas Marner that there is truth on both sides, and that although Dr. Radcliffe may be right in claiming anæmia or defective action as a common state of a neuralgic nerve, on the other hand a state of hyperæmic irritation may be present in other cases.[1] We have evidence in either of perturbed nerve tension and with this loss of tone we should anticipate loss of normal function. In either case, as Dr. Handfield Jones and others have clearly seen, to expect *heightened function* is absurd, and the term hyperæsthesia becomes either etymologically or scientifically abominable or both. It seems to us that distinction sufficient is not made between the local state and the general state. It is not only conceivable but likely that a hyperæmic and irritated state of nerve root may coexist with signs of general anæmia and debility, and this likelihood scarcely receives full recognition at Dr. Radcliffe's hands. On the other hand Dr. Anstie has erred, as it seems to us, in stating too roundly that neuralgic patients are, as a rule, the subjects of debility. That they have a weak spot in their organization is, of course, a truism, and that in persons having this weak spot exhausting causes will bring on an attack is likewise pretty obvious; but we daily see neuralgics who not only present the ruddiness and muscle which, as Dr. Anstie says, may be deceptive, but we see them to be men and women of full and real vigour not only of the body in general but of the nervous system likewise. An intimate friend of the present writer, who suffers from attacks of intense ophthalmic neuralgia and who is the son of an asthmatic is a fine example of a strong and enduring constitution and of a mental keenness and perseverance far beyond the average. Of course he may have, and his symptoms prove that he must have, one weak place, but who has not? However the existence of such a weak place is not made

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[1] It seems certain that trigeminal neuralgias are accompanied sometimes with vaso-motor spasm and anæmia, and in other cases with vaso-motor palsy and hyperæmia. Injection of the fundus of the eye in hemicrania we have often noted.

likely in any degree by the facts of his general health and powers. Even of abdominal neuralgia, which Dr. Anstie says (p. 83) occurs in subjects "almost invariably in a state of marked and evident debility," we know several subjects whose health and mental vigour are otherwise excellent. Many such instances rise into our memories, and many likewise in whom general debility, nervous or other, is but the consequence of the harassing pain. At the same time we acknowledge to the full the value of the common cases to which Dr. Anstie calls our attention in a most vivid and instructive way, cases in which the neuralgia is but one evidence among others of general debility and in which we find other grave faults of the nervous system likewise. A series of such cases illustrating the coexistence of neuralgia with other manifestations of itself, with angina pectoris, with gastralgia, with epilepsy and with insanity is given by the author on page 114.[1] Among these affections phthisis also holds a prominent place, and this coexistence is of especial interest to the present writer, who holds that there is a kind of phthisis which is a neurosis, and which is to be classed with several other disturbances of tissue nutrition which owe their origin to a palsy or defect of trophic nerves.[2] Another very important correlation of failure is found in the concurrence of chorea on the same family trees with neuralgia and its allies, a fact which we have long noted; this concurrence is one of the several clinical peculiarities of chorea which may be added to those mentioned by Dr. West[3] as bearing against the ingenious theory of its embolic origin, as proposed by Dr. Hughlings Jackson.

From these interesting correlations we learn once more the important lesson that no true classification of disease can be based on any other than a hereditary scheme, that it cannot attain even a provisional completeness until the affinity of diseases is ascertained from a study of their coexistence and sequence in the same person and his blood relations. There is also the curious touchstone of arsenic which has so potent an action for good in many nervous affections. We who have worked round to an investigation of the neurotic diathesis from another point of view, were led to it by following out the tracks of arsenic. We found that this drug is not a curer of skin diseases as such, but of certain kinds only, which kinds, we afterwards found, developed themselves in families and individuals who presented likewise a tendency to nervous disorders such as those above

[1] Dr. Anstie has strengthened this argument in a very interesting paper in the 'British Medical Journal' for Nov. 11th, 1871, where he deals with the connection of asthma, angina pectoris and gastralgia.

[2] *Vide* 'Med. Times,' Nov. 18th, 1871.

[3] 'The Lumleian Lectures,' "Nervous Diseases of Childhood." 1871.

enumerated and who were often to be distinguished even in health by a peculiarity of character and physical attributes. We began therefore many years ago, to extend the use of arsenic in such persons and families whether they presently suffered from skin affections, or from asthma, gastralgia, superficial neuralgia, angina pectoris, chorea or even epilepsy and phthisis. To the occasional treatment of epilepsy by arsenic we were led by the study of two very instructive cases in which epilepsy alternated with eczema and psoriasis respectively, and in which arsenic was found to have almost as great curative powers in the former as in the latter diseases.

With regard to the incidental causes which call forth attacks in the potentially neuralgic Dr. Anstie gives us much very interesting information. On two principal heads especially he expresses himself vigorously, and in a way which is full of instruction to the clinical physician. These two heads are, 1, the effect of advancing years with their corresponding tissue failure and, 2, in earlier life, the terrible harm inflicted upon the nervous system during its relatively weak and unstable youth, by artificially strained religious emotion, by peripheral sexual irritation, by false and stimulating art and literature, or by all or several of these acting together. The book is full of sayings which betray something more than the mere physician, which distinguish the author also as a cultivated and observant man of the world; such as the following: "It is a comparatively frequent thing, for example, to see an unsocial solitary life (leading to the habit of masturbation) joined with the bad influence of an unhealthy ambition prompting to premature and false work in literature or art" (p. 61). There never was a time when this could more truly be said than the present. Or again: "The truth is that the young people who make music or painting an excuse for idleness respecting other matters are invariably imposters, even in that which is their own supposed forte" (p. 215). On the other hand Dr. Anstie makes some wise reflections on the duty of imbuing the susceptible and ardent mind of youth with a serious view of art. If, he says, music or any other art be chosen and made a serious study, "a certain definite time being set apart for it, and thoroughness being insisted upon" we should have "an admirable vent for the emotional effervescence of commencing sexual life;" while the lazy and conceited manner in which young gentlemen and ladies now dabble in "accomplishments" is "intensely pernicious." No words of ours are needed to enforce these warnings; perhaps this generation will hear them when they come not from the pulpit but from the hospital, not as moral warnings against luxurious sensations and frivolous heated joys, but as interpretations of natural laws which never sleep but are surely forging sharp arrows against those who neglect them. Secondly, we are told more clearly than we have been told before, how the tissue degenerations of advancing years favour the onset of neuralgias, and in this Dr. Anstie finds, with reason, a great support to his belief that neuralgia is a diminution and not an excitement of nerve action. The neuralgias of the period of bodily decay "are of very bad prognosis."

"A neuralgia which first develops itself after the arteries and capillaries have begun to change decidedly in the direction of atheroma, is extremely likely, even if apparently cured for a time, to recur again and again with ever increasing severity and to haunt the patient for the remainder of his days."

"For this purpose I am in the habit of insisting on the great importance of sphygmographic examination for all neuralgic patients who have passed middle age. When we get the evidence which is furnished by the formation of a distinctly square-headed radial pulse curve, even though there be no palpable cord-like rigidity of superficial arteries, we are bound to be exceedingly cautious of giving a favourable prognosis' (p. 171).

Many are the attractive topics which further suggest themselves during the perusal of this volume; among others, the difference between Anstie and Eulenburg as to the true place of migraine among neuralgias proper. Our own experience bears out the statement of Anstie that migraine is but a phase of ophthalmic neuralgia. On this point we may refer also to a thesis on migraine privately published last year by Mrs. Garrett Anderson, who, while upholding the true view of migraine as a neurosis among neuroses, and not as a symptom of dyspepsia, yet would put the seat of the pain in the central encephalic ganglia. An interesting case has lately come under our care in which the appearance of severe migraine was the first indication, and, as yet, continues to be the most distressing consequence of a fibrous tumour of the uterus. Dr. Anstie concludes his description of neuralgias by an effective contrast between them and pains which are not neuralgia. Of them are myalgic pains, pains of chronic alcoholism, of rheumatism, of syphilis, of dyspeptic headache and so forth. Here the author, who regards neuralgia as a disease, shows more discrimination than Eulenburg, who, using the word neuralgia in the sense of a symptom, gathers arthritic, syphilitic, saturnine and every sort of pain into his net. We could dwell long upon the momentous chapter on treatment did space permit, for here Dr. Anstie develops in its fullest sense the important doctrine that clinical observation is only justified by its results in the alleviation of suffering. Anstie and Eulenburg both speak at length of the constant galvanic current and come to the conclusion which we ourselves support, that it is the most effectual of all remedies for the superficial neuralgias, having at least as much curative power as the hypodermic morphia and often as much power also for immediate palliation. Cases do however occur in which it fails, as in one of ophthalmic neuralgia with a gray lock of hair now under our own care, in which the current has failed to give the slightest relief. Our own experience is that if the current is to cure it will prove its intention by giving some degree of immediate relief at the first sitting. Anstie seems to speak a little too slightingly of electricity in its other forms and of the spark in particular. Both authors remark upon the success of quinine in supraorbital neuralgia which is so curious a contrast with its frequent failure in other varieties. Both authors also give its due meed of praise to arsenic. As to hypodermic morphia there is little now to be said, but we think that the treacherous tendency of morphia in some cases to keep up the neurosis it pretends to control is not recognised. Our own experience is full of such cases, for example, we remember one of most intense cervico-bracheal neuralgia in which morphia had been used with brilliant palliative effects for some time, the paroxysms however recurring as frequently as ever and with more severity if not cut short. This brave lady at our urgent request laid the morphia aside and the disease, which during the use of morphia had resisted all other treatment, then gradually disappeared. In visceral neuralgias or neurotic disorders, such as paroxysmal vomiting, we have noticed the same impotence of other remedies during the regular use of hypodermic morphia. On the other hand, of course we have numerous cases in which this means brings about an instant or rapid cure; should the operation, however, fail after a fair trial to cure, we are convinced that it should not be continued for purposes of palliation in cases where a cure is to be anticipated. In the neuralgia of decaying life, when all remedies have failed, it may of course be continued as a palliative for years, as here no cure is, perhaps, to be hoped for. Eulenburg speaks highly of the results of the hypodermic use of narcein in an inveterate case of prosopalgia as it not only relieved but cured the patient. As to omissions, we are surprised to find that Anstie scarcely mentions muriate of ammonia, a remedy which in our hands has proved of striking benefit. In a series of fifty cases of neuralgia treated by this drug, we obtained decided relief in thirty and prompt cure in twenty-three; some of these latter were very remarkable instances and the whole series is the more valuable as before the commencement of our stricter investigations we had little belief in this efficacy of the medicine. We have found it indeed at least as valuable in true neuralgia as in myalgia, or more so. The only other matter to which we can now refer is that of the use of local remedies. Of these Eulenburg speaks in terms which scarcely rise even to the level of contempt. He utterly rejects them as being always and everywhere valueless. Dr. Anstie does not go so far as this but allows that chloroform, belladonna, opium, aconite &c., have some value as helpers. Nor can we at all admit that Eulenburg is justified in his exclusiveness. We admit that it would be poor work to play about with local remedies and to forget hypodermic morphia, galvanism or arsenic, but we do certainly contend that these latter chief measures may be supplemented by the use of liniments and unguents with much advantage. We remember one case of supraorbital neuralgia which was promptly cured by aconitine ointment, to the patient's great delight, when hypodermic morphia and quinine had failed, and we think our readers will bear us out in saying that the application of opiates upon spongio piline and of belladonna, aconite, or chloroform liniments, often serve good purpose as palliatives.

As we pass on from then euralgias to other nervous disorders we turn altogether to the thoughtful and complete work of Eulenburg. This book is so full, so well condensed and so clearly written, that we would fain say more upon it than is now possible. Dr. Eulenburg handles all these questions with so much mastery and so much knowledge that we must urge all who study them to obtain his work at once, as no physician however accomplished can fail to find great profit in it, and we ourselves must confess that Dr. Eulenburg has laid us under a deep obligation. Where he is u iable to solve a difficulty he states it distinctly, and so brings us up to the limits of knowledge, that we have provided for us the exact standpoint for future work. Take, for example, his able discussion of a point which was among the first on which we sought to know his conclusions. We turned early to his chapters on Cutaneous and Muscular Anæsthesias to see how he would interpret the obscure phenomena of hysterical palsy, of tabes dorsalis and the like. The whole difficulty is admirably handled, as it appears to us, being adequately discussed yet without diffuseness. It is impossible for us to make quotations from paragraphs all of which hang so closely together, but, on the question of conduction of sensation in the cord, Eulenburg leans to the side of Schiff, who believes that this property resides both in the gray matter and in the posterior columns, but with a difference, the gray substance being a conductor only of general sensibility, while the perception of special-touch impressions is carried by the columns. He confesses however that, although this view of Schiff clears up some clinical cases, it is unequal to the explanation of others. We must, we say again, be content to do no more than indicate the kind of discourse to be found in these sections, the results being too indefinite to be shown by short extracts. The disorders of sensation, superficial, visceral and special being extensively and minutely dealt with in 340 pages, as many more are devoted in the second part of the volume to the disorders of motion. In this division all possible palsies are investigated as well as convulsions and cramps, and with the same skill as before. We are struck not only with the extensive reading of the author and his industrious comparison of cases, clinical and experimental, but also with the variety and aptness of his own experience. On subjects the most obscure he has interesting and appropriate instances of his own to bring forward, and he rarely sums up the results of therapeutics without giving the reader a sense of his own participation in these results, without putting a backbone into the mass of scattered testimony which would otherwise be too incoherent for practical purposes, and without showing that he has himself tested the methods of treatment of which he speaks and has therefore a right to give to each its due appreciation. Thus the book differs essentially from the mere compilations which may not fall short of it in industry, or in a kind of acumen, but which do not come from the hand of practised and fortunate observers. The author's remarks upon electric reaction and upon its therapeutical uses seem to us to be everywhere acute and thoroughly grounded upon investigation. Under the head of spinal palsies we turned to consult the author about the recent statements of Schiff, which have so much disturbed the current teaching concerning the transmission of motor impulses along the spinal cord. We find the difficulty adequately dealt with, and Eulenburg scarcely sees how we are to avoid accepting Schiff's assertions: 1st. That functional defect of the antero-lateral columns does not cut off motor conduction, and that isolated disease in them does so only in so far as it involves the anterior nerve-roots; and 2nd. That motor conduction is only arrested when either the anterior nerve-roots are severed, or when the gray matter is severed in its whole thickness. Eulenburg thinks Schiff's evidence seems as yet stronger than that of his opponents, and we are not to shrink from the conclusions of advancing physiologists merely because they throw our present opinions into confusion. We would draw attention in the next place to the excellent account of the mode in which palsy of the bladder occurs in spinal disorders, for we believe that the explanation given will enlighten many of our readers as it has enlightened ourselves.[1] Unlike Schiff's new views about motor conduction, Budge's new views about the innervation of the bladder throw a fresh light upon bladder spasms and palsies, and perhaps upon fæcal and seminal acts and disorders, which will not only clear up obscure cases but will compel many of us to alter our established opinions upon these disorders. Starting from the later researches of Budge upon the motor nerves of the bladder, which proved, as many of us were aware, that those nerves spring from the pedunculus cerebri, pass through the restiform body and the anterior columns of the cord, and issue with the third and fourth sacral nerves, the author goes on to say, what is certainly new to ourselves at any rate, that there is no muscle in the bladder deserving the name of a sphincter, but that every muscle-fibre in the bladder is in favour of the expulsion of urine. The only muscles, according to Budge, which prevent its flow are the constrictor urethræ and the bulbo-cavernosus. The motor nerves of these muscles spring likewise from the pedunculus cerebri, have the same course as the motor nerves of the bladder and issue from the cord with the third, fourth and fifth sacral nerves in the course of the pudic nerve. An especially important point is, that these muscles are under the influence of a reflex tone established in the lower part of the cord, the centripetal elements of which are the sensory nerves of the bladder which run in the posterior roots of the third, fourth, and fifth sacral nerves. *Section of the cord at any part above these nerves invariably sets up ischuria and decided dilatation of the bladder, but never incontinence;* and this ischuria depends not upon the division of the motor nerves of the bladder alone, but also upon the increased reflex irritability which heightens the reflex tone of the urethral muscles. Incontinence sets in as a secondary event and is due to hydrostatic pressure which then increases so far as to overcome these muscles. But, on the other hand, incontinence can be directly produced by section either of the anterior or posterior roots of the third, fourth and fifth sacral nerves, as in the former case the motor nerves, and in the latter the nerves which establish reflection upon the urethral muscles are severed. Eulenburg goes on to show how well this explanation suits, among other things, the bladder disorder of tabes dorsalis, in which we find at one time ischuria, at another incontinence, both occurring somewhat capriciously but never in a very

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[1] As we write these lines (Nov. 25th, 1871), we receive the current number of the 'British Medical Journal' and in it we see that Dr. Althaus communicates these new views of Budge.

severe degree. Like the ataxy itself the bladder disorders depend upon injury to the centripetal nerves passing from the bladder to the cord, by which both the voluntary and reflex innervation of the urethral muscles is changed in the direction either of irritation or debility. A like explanation, probably, is true concerning the constipation followed by incontinence of fæces which is so often seen in tabes, though the precise mechanism of these symptoms is as yet undiscovered, it being especially hard to disbelieve in a sphincter ani. Eulenburg has nothing more to say concerning reflex paralyses than has been said already by Brown-Séquard on the one hand and, on the other, by Jaccoud. He agrees with most physiologists that Brown-Séquard's view can no longer be upheld. For our own part, as regards the so-called urinary paraplegia, we as yet would prefer the explanation of Leyden, who believes that the paraplegia is due to a neuritis propagated from the bladder to the cord. This we believe to be the true explanation of the few cases in which the spinal disorder can be proved to be secondary to the disease of the bladder, and this state of things we were able to demonstrate in the only case of undoubted urinary paraplegia which of late years has come under our care. Very well marked inflammatory products were found matting the sacral nerves together and burying them with the bladder, peritoneum and other tissues, in a thickened mass; the cord itself was softened. On turning to the chapters on spasms and convulsions, and among these first to the sections on epilepsy, we find Eulenburg holds the opinion that epileptic convulsions may take their rise either in an anæmic brain, or in a brain in a state of venous hyperæmia. This view, according to the observations of the present writer, is supported by the evidence of the ophthalmoscope and Eulenburg agrees with us, likewise, in attaching great importance to Nothnagel's discovery of a "cramp-point" close to the fourth ventricle, a point which also corresponds, we would add, to the supposed centre of the vaso-motor nerves. Eulenburg also draws our attention to the fact that irritation of a sensitive nerve (say, the sciatic) may not only set up contraction of the arteries of the pia mater (Nothnagel) but also their dilatation by a reflex paralysis (Loven). He adds that it is tolerably clear from the experiments of Nasse (Centralblatt, 1870, No. 8) that deficiency of oxygen is insufficient to call forth convulsions, and that these depend rather upon the accumulation of injurious products, such, perhaps, as carbonic oxide, carbonic acid or other waste gases. Uræmic convulsions on the other hand do not, he thinks, depend upon the accumulation of waste, but he holds with Traube that they are the consequence of œdema of the brain with increased pressure. Like epilepsy, uræmic convulsions may occur either in a brain deficient of all blood, or in a brain overloaded with venous blood. Under the head of asthma we find that Eulenburg notices, as does Anstie, the intimate connection of this disease with trigeminal neuralgia and with angina pectoris. These clinical facts taken with others seem to point to the medulla and region of the fourth ventricle as a 'vital knot' in more senses than one; as not only is the respiratory centre here with the cardiac sensory centre, but hereabouts are the vaso-motor centre and the cramp-point;[1] here is the centre of the trigeminus, and here these are all met also by the sensory nerves from the chief organ of digestion, the stomach. In discussing the pathogeny of asthma we notice that Eulenburg fully accepts Salter's hypothesis of a bronchial spasm depending upon affection of the vagus, but he believes that spasms of the diaphragm, of the other inspiratory muscles habitual and accessory, and perhaps of the glottis, are also concerned in the attacks. If this be so it harmonises many observations which at first sight seem contradictory. We had marked many other paragraphs in this excellent and valuable treatise which we had hoped to quote or to discuss, but reluctantly we pass them over in the hope that we have said enough to direct our readers to the source itself. As regards the printing of the volume, though this is done in good type and style yet there is an abundance of mere printers' errors which are not to be excused on the plea of the author's absence during the drawing of the proofs, and which are unworthy of Hirschwald. It may be that we are too much of purists in this matter, but we certainly were a little disappointed to find that Dr. Anstie's work also in this respect falls below the standard we look for in the Clarendon Press.

T. Clifford Allbutt.

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[1] Salter was the first, or one of the first, to point out the connection between asthma and epilepsy.

Bibliographical Record.

Notes on Comparative Anatomy.[1]—The present year has been prolific in the publication of Manuals of Comparative Anatomy, which science appears to have gone through several periods of construction and of reconstruction during the past ten or twenty years. At the present time it is positive observation and not theory that is cultivated, and the older works have given place to a new series, of which the one before us is an example.

Dr. Ord's work, written in a remarkably laconic spirit, is evidently destined for purely scholastic purposes. It is about the last work in the world which a learner would dawdle over, while as a simple *aide-mémoire* it is of the highest possible utility. Its use, however, will, we fear, be chiefly confined to Dr. Ord's own class in Comparative Anatomy at St. Thomas's Hospital, for which it was especially prepared. The classification adopted by the author is naturally the first thing to which the critic turns, and we discern with pleasure that the author has with great discretion confined himself to the promulgation of a very safe and generally accepted system.

The author's initial distinctions between not living matter and living beings are highly lucid. They comprise an amplification (in part after Nicholson) of the well-known distinctions established by Professor Owen—distinctions which, in spite of the contrary arguments of Professor Reay Greene, have not been formally disproved up to the present time. In fact, it is with satisfaction that we perceive that Dr. Ord is enabled to formulate accurate and clear distinctions between animals and plants.

Some of the author's descriptions are not clear. For example, when speaking of the *Cetacea,* the following passage occurs—

"Line of centra altogether fish-like in look. Anterior extremity, scapula flattened, with small distinction of parts. Acromion and coracoid mostly imperfectly marked. No clavicle. Digits often have more than three phalanges; are enclosed in common skin or web, and have no nails. Pelvis represented by a pair of bones not attached to spinal column. They support penis or clitoris, and as is suggested, correspond to ischia."

[1] *Notes on Comparative Anatomy: a Syllabus of a Course of Lectures delivered at St. Thomas's Hospital.* By William Miller Ord, M.B., M.R.C.P. London, 1871.

The first four statements here are, we consider, rather vague, and whilst the absence of the clavicle and the "greater" number of digits than three may be taken as admitted statements, the fact that *Balæna Australis* and some *Balænopteræ* have four digits, with a rudimentary fifth one, might have been noticed. But the statement in reference to the pelvic bones rather surprises us. We were under the impression that while the ischial bones were alone present in the *Delphinidæ*, the pubis, femur, and tibia found representative ossicles[1] in some of the whalebone whales, and that Eschricht and Reinhardt had made this especially interesting discovery. An examination of the skeletons of *Balænoptera rostrata* and *Sibbaldii*, now in the Hull Philosophical Society's Museum, will further throw light on this subject. If such an error can be found to exist with respect to the large bones, we are afraid of the fate of the little ones in Dr. Ord's hands. We imagine that a hasty glance at the 429th page of Owen's book must have led Dr. Ord to take the definition of the Odontocete family for that of the Cetacean order, and thus to confuse the part with the whole.

There are few, however, of this class of errors in the work, which doubtless will be of great use as a class-book. The latest researches of Mr. W. Kitchen Parker on the development of the "shoulder girdle" are very properly introduced. We think that for students' purposes a list of the great bones of the skeleton, indicating their general, and, when necessary, their special homologies, would be convenient in a future edition. The bones which are absent in any of the great groups of animals might be indicated in italics, or in some other convenient manner.

Dr. Ord's classification of the Invertebrate group, which he considers as "very probably equal in value to the whole of the vertebrata," is remarkably lucid, and as the latest discoveries made in respect of the lowest forms of life are included by him, this portion of the work will be of the highest value to the student. In fact, the system on which Dr. Ord has proceeded, that of presenting a "chart on which a few principal centres of life are clearly indicated" is highly convenient for those who would "get up" an examination quickly. Whether the modern system of teaching Comparative Anatomy and zoology "by type," as opposed to "by definition," is the best, is another question altogether. The masters of zoological science in the last and the present generation, were not taught according to this method. They had before them the Linnean maxim. *Omnis vera cognitio cognitione specifica initiatur*, and the result has been a thorough and accurate knowledge of *species*. This of much greater importance to train and discipline the mind than the mere selection of scattered types. We never yet found two comparative anatomists

[1] Eschricht and Reinhardt, 'Om Nordhvalen,' 4to, 1861, p. 151, pl. ii; Owen, 'Anatomy of Vertebrates,' vol. ii, pp. 307 and 429; Flower, 'Osteology of Mammalia,' p. 303.

who agreed with each other what the types really are, if *e.g.* the order *Perissodactyla*; and if this be the case in well-known orders of the mammalian class, what confusion may we not expect to find amongst the less known Invertebrata?

Some of the definitions of the various theories of the origin of animal life as expressed by Dr. Ord are remarkably lucid. He says:—"In the organisation of animals at least two factors (1), the original type, (2) their habits, their history, and changes or addition of structure are therewith correlated." This is the same as the saying of Professor Huxley, that every animal is the resultant of two forces, the one teleological and the other morphologiieal. The distinction between the centripetal and the centrifugal forces which operate in the production of living things is here well pointed out. Dr. Ord is is wrong, however, in assigning to Lamarck instead of to De Maillet the origin of the theory of progressive development as usually understood; and we think that a more distinct definition of the variations of opinion between the Derivationites on the one hand and the Darwinites on the other, which Professor Owen at the end of his famous third volume has manifestly pointed out, might have been introduced. The definitions of Lamarckism and Darwinism given are not at all clear. That there is as much "innate" tendency on the part of species to vary supposed by Darwin as by Lamarck, we are bound to admit. A learned author has even proposed to term Darwinism "Subjective Transmutation," and to reserve the name "Objective Transmutation" for Lamarckism. Such expressions, while they are more correct, are infinitely more lucid than those given by Dr. Ord. Nevertheless, there is no doubt that the present work will be useful, as it is undoubtedly convenient as a pocket volume for the student during the delivery of lectures.

Jones' Comparative Anatomy and Nicholson's Zoology.[1]—The former of these two treatises has had an existence of thirty years, and been a favorite during a generation with some thousands of students. It has stood alone as a manual of comparative anatomy with just so much physiology as would interest its readers generally who were not specially occupied with that science. It has recommended itself to all by its easy, agreeable style, and particularly by its numerous and most excellent wood engravings. In these valuable features and in the general scope of information conveyed it has had no rival; and the demand for a fourth edition during the current year indicates for it a prolonged existence. How far this existence shall be lengthened out must depend on its capacity of

[1] 1. *General Outline of the Organisation of the Animal Kingdom, and Manual of Comparative Anatomy.* By THOMAS RYMER JONES, F.R.S., &c. Fourth edition, illustrated by 571 engravings. London, 1871. Pp. 886.

2. *Advanced Text-book of Zoology for the Use of Schools.* By H. ALLEYNE NICHOLSON, M.D., &c. Edinburgh and London, 1870. Pp. 340.

rejuvenescence—it must not grow old in its substance-matter. We regret, however, to state that our survey of its contents forces upon us the conclusion that it is waxing old, and that if it is yet to remain a text book of the subjects it professes to teach, it needs the hand of a young and skilful reviser.

Compared with the early editions this last contains much additional matter, but it is not up to the science of the day; and, to judge from the works referred to as authorities for its statements, the author lacks much in the knowledge of recent work done in zoology and comparative anatomy by the present best known students in those sciences. Moreover, the additions made are too much in the form of accretions to the original matter, and not assimilated therewith as they should be. These defects and deficiencies will not be so much felt by the reader seeking for general information in the subject-matter of the work, but they will seriously lessen its value to the student preparing for examinations, at which the latest scientific results are required to be known.

Dr. Nicholson's advanced text-book of zoology is intended for upper schools, and is admirably adapted for its object. We had occasion to review the "Manual of Zoology" by the same author in the number of this Review for October, 1870, and to commend it for its general excellence. The work now before us may be looked upon as an abridgment of the foregoing, retaining the same characters, giving like descriptions, but abbreviated, and illustrated by the same woodcuts.

The descriptive anatomical details are clearly given and technicality avoided; but to aid the beginner unacquainted with zoological and anatomical terms, a good glossary is appended to the book, explaining their use and giving their derivation. In all respects this treatise is an excellent introduction to zoology, sufficient for all readers not making the science a special study.

Animal Plagues.[1]—Mr. Fleming occupies a position which affords him special opportunities for the study of the class of diseases to which the term *epizoötics* has been recently applied; but his book is precisely what it professes to be, namely, a "Chronological History of Animal Plagues from B.C. 1490 to A.D. 1800.

In his "Introduction," which is by no means the least interesting portion of his book, he shows how "the commencement of what we may call veterinary medicine" may be traced to the superstitions of the primitive herdsmen, whose parallel may now be traced in the nomadic races who, with their countless flocks and herds, roam over the steppes of Central Asia. The Egyptians, Greeks, Romans, and

[1] *Animal Plagues; their History, Nature, and Prevention.* By GEORGE FLEMING, F.R.G.S., &c., President of the Central Veterinary Medical Society. London, 1871.

other nations—pastoral and agricultural—all resorted to incantations and sacrifices for the cure of the diseases of themselves and of their animals.

Passing over our author's allusions to some of the special forms of belief regarding plagues and pestilences held in the time of Homer, and subsequently in the early period of Rome, we come to his researches on the "many superstitious customs, having reference to the preservation of our domestic animals, that appear to have been derived from the early traders with Britain—the Phœnicians," in which he shows that traces of the worship of Bael, Bel, or Belus, were until recently, and we believe still are, practised in the Highlands of Scotland and in Ireland (and he might have added in Cornwall). It seems strange to the hard-headed philosophers of the present day—the followers of J. S. Mill and Herbert Spencer, Tyndall and Huxley—that it has been left to our own time to see the close connection between dirt and disease, and to recognize the fact that it is not to the special anger or displeasure of our all-wise and all-merciful Creator, but to agencies of a physical nature, commonly under our own control, that epidemic and epizoötic diseases are ours. Scarcely five years have elapsed since "processions of Greek and Turkish priests steered bare-footed through the plague-swept streets of Constantinople, the former uttering loud appeals for deliverance from the scourge, and the latter calling upon Allah to protect them, though they were opposing the most urgent sanitary measures, as contrary to the teachings of the Koran;" and about the same period (1865) a catholic archbishop in our own country maintained from the pulpit that the cattle plague, then just imported into Britain, was directly due to God's displeasure at our great love for animals! while a clergyman of the Church of England similarly asserted that the origin of the malady was to be traced to "our national and *carnal* love of beef." The great majority of our readers will, we are confident, agree with Mr. Fleming's observation that "such doctrines are unworthy of Christians, and carry us back to ages when the perpetration of the most atrocious crimes, and the cold-blooded slaughter of whole tribes of men, women and children were laid to the favour or dis-favour of the God of mercy and love."

The Introduction concludes with some valuable remarks on the bearing of the study of "animal plagues," upon human medicine, agriculture, history and political economy. "The losses" (says Mr. Fleming) "from only two exotic bovine diseases (contagious pleuro-pneumonia and foot-and-mouth disease) have been estimated to amount, during the thirty years that have elapsed since our ports were thrown open to foreign cattle, to 5,549,780 head, roughly estimated at £83,616,854; while the late invasion of "cattle plague," which was suppressed within two years of its introduction, has been calculated to have caused a money loss of from five to eight millions of pounds."

We regret that our limited space prevents us from noticing this useful and interesting volume so fully as it deserves. It contains a vast field of antiquarian matter relating to the diseases of almost all kinds of animals—epizoötics affecting bees, silkworms, fishes, birds of various kinds, and mice, cats, dogs, deer, pigs, sheep, cattle and horses.

If we had been as well acquainted as (thanks to the laborious investigations of our author) we now are with the early history of the diseases of our domestic animals, we might have derived much consolation from the reflection that the cattle plague was no new disease; that it had raged with more or less severity over many parts of Europe in the first half of the eighteenth century, and that it had, on at least two other occasions, made sad havoc amongst our own cattle, without leading to more than temporary losses and troubles.

When and where this fearfully contagious and most destructive disease originated is not clearly known. Kanold, a German historian of his own time, states that in 1709 its first beginning or appearance in Europe arose in that part of Tartary which lies on the border of Asia; "but (he adds) whether it was originally generated in this corner of Europe, or whether it was brought there from Asia, or yet whether, perhaps, it was an endemic disease, as the plague of man is in Turkey, I am unable to decide." In 1711 we find that it had spread over a great part of Russia, and from thence into Prussia, Hungary, Austria, Bavaria, Moldavia, Switzerland and Italy. How long this disease prevailed is not definitely known, but we learn that in Holland alone, from 1713 to 1723, it destroyed 200,000 cattle." The celebrated anatomist Lancisi, who studied the disease in Italy, has given an excellent description of it. He fully recognized the hopelessness of medical treatment, and "advised that every diseased animal should be instantly destroyed with the pole-axe, so that no infected blood may escape to the ground;" in fact the present stamping-out system.

In the summer of 1714 the disease was carried from Holland to England, and according to a poet of that day, John Morphewd, the "German cattle plague" came in with the accession of the German dynasty in England.

This pestilence was entirely suppressed by means of the stamping-out process within a few months of its appearance in this country. It has, however, been computed that "the number of cows and bulls lost by this disease in the counties of Middlesex, Essex and Surrey were 5,418, and of calves 439;" while "from 1711 to 1714 no fewer than 1,500,000 died of the plague in Western Europe."

In 1735 the cattle plague was again introduced, as we learn from Mr. Fleming, into Italy, and during the interval, from its original appearance in Europe to this date, it had probably never been quite extinguished in Poland or Germany. From this time up to the end of the eighteenth century, this "bovine scourge" was never absent from some part of Europe, it having been extended and perpetuated by the almost continuous wars that were then occurring. The hostile armies were accompanied by infected droves, and the contagion was thus spread far and wide.

In 1745 the cattle plague was again carried into England, and on this occasion it extended to Ireland and, as some writers think, to Scotland.

In November of that year we find that Dr. Mortimer read a paper on this disease before the Royal Society, in which he fully describes the symptoms, the appearances after death, and the treatment. After reading his prescriptions, we cannot help sympathising with the unfortunate cows which, for shortness of breath, took "whale-oil and treacle, each a pint; flower of brimstone, four ounces, in a mash of malt twice or thrice a day;" "while for running of the nose," we are told that "pouring a pint of warm vinegar, with an ounce of salt, into the nostrils, has proved successful in making the cow sneeze."

Subsequent epidemics occurred during the last century in 1769 and in 1799, and induced the legislature to pass various enactments to arrest their progress and also the introduction anew of the malady. These were of great service; but the strongest measures for stamping it out remained for adoption in the last great epidemic a few years ago.

Although the disease we have been describing is for its virulence known *par excellence* as *the* cattle plague, contagious and infectious disorders of various kinds have from the earliest times prevailed among the lower animals.

These observations on the "cattle disease" will suffice to show the great amount of study Mr. Fleming has devoted to this subject.

We have looked in vain throughout the whole of his volume for a description of any cattle disease resembling the Texas disease, to which we called attention in a recent number of this Review, and which is remarkable for its resemblance to yellow fever. We learn from an American correspondent that a full Report of the Texas Cattle Disease is now printed by order of Congress, and that it will be immediately published; and we trust that we shall have an early opportunity of reviewing it in these pages.

Buchan's Introductory Text-Book of Meteorology,[1]—The author

[1] *Introductory Text-book of Meteorology.* By ALEXANDER BUCHAN, M.A., F.R.S.E., Secretary of the Scottish Meteorological Society; President of the Botanical Society of Edinburgh; and Honorary Member of the Austrian Meteoro-

of this work is well-known even in medical circles as an indefatigable worker, and as a most careful observer in a science which is especially related to that of medicine. It was, therefore, with no ordinary degree of interest that we opened the "Text-book of Meteorology," expecting to find its contents of sterling quality; nor have we been disappointed. No branch of his subject has been neglected by Mr. Buchan, and he has treated it with all the acumen which is certain to accompany, and to reward, accurate and long-continued observation. Having devoted the first chapter of his work to an historical sketch of the science, the author at once proceeds to a consideration of the nature and distribution of atmospheric pressure, including a description of the barometer, its principle and uses. To the concluding words of this part of the subject—"the chief disturbing influences at work in the atmosphere are the forces called into play by its *aqueous vapour*"—we are inclined to take exception, remembering that this element is largely, indeed altogether, dependent on one even more powerful—namely, *caloric*. Chapters IV—VII inclusive, treat of temperature, as determined by thermometers of various kinds, as influenced by solar and terrestrial radiation, as distributed over the earth's surface, and as related to atmospheric pressure. Under the second head allusion is made to medical climatology in a brief but accurate manner. The relation of temperature to atmospheric pressure forms an appropriate introduction to the subject next taken up—the moisture of the atmosphere. This is made to include hygrometry; mist, fog and clouds; and rain, hail and snow. The matter of the chapters which treat of these important topics is particularly good. We would, however, offer one or two suggestions. Thus, the word "*evaporometer*" seems more correct than "*evapometer*" used by our author; and we think that *cirrus*, rather than *cirro-stratus*, is essentially the cloud which leads to the appearance of parhelia, paraselene, and halos. Again, Mr. Buchan attributes the relative greater amount of rainfall of the east of Europe in summer to the more westerly direction of the wind at that season. We would suggest as a more probable cause the increased frequency of heavy thunder-storms over the Continent in the hot months. In Chapter XI we pass on to the consideration of wind. This branch of meteorology is fully investigated in accordance with the principle laid down in Buys-Ballot's celebrated law of wind and pressure. As this is the case, some statements rather inaccurate in expression surprise us the more. Such are the following: "the wind blows from a region of higher to a region of lower pressure" (page 132)—there being no qualifying description of the way in which it does so; "the surface winds blow out of this space in every direction,"

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logical Society. Edinburgh and London, 1871. Octavo, pp. 218. With illustrations and tinted plates.

(page 134,) *i.e.* in anticyclonic curves. The first of these sentences is corrected at page 136, and more fully at page 140. Much valuable information as to climate in given in chapter XII, on local and other winds. The essay on "Storms" in the next chapter is all that can be desired, and is illustrated by some very pretty diagrams. We regret that atmospherical electricity has had so little space allotted to its consideration. The work concludes with some general observations on "Weather," and a brief allusion to systems of storm-warnings. Some very valuable tables for use in meteorological investigations are appended, and render the book far more complete than its modest title of an "Introductory Text-Book" would lead one to suppose it to be.

Dr. Porcher on Medical Botany of the Southern States.[1]—The copy of this treatise has come to us by favour of Prof. Henry, of the Smithsonian Institute. It is a book abounding in information and replete with suggestions of new vegetable medicines of much promise. The first part of the title is, however, calculated to mislead; it is too wide and general for the contents of the volume, and might well be omitted in favour of the second division. For the substance matter of the work is medical botany, with notes on the economical and agricultural value and management of some of the most important plants and trees.

We learn from the preface, that the book owes its origin to the late civil war in the United States, the author being charged by the then Confederate Government with the duty of collecting information respecting the vegetable productions of the Southern States, with the special view of obtaining home supplies of drugs and of materials for agricultural and economical purposes, in the absence of the accustomed imports denied them by reason of the blockade of their ports. In its preparation Dr. Porcher has also kept "in view the wants of emigrants and those abroad who wish to be acquainted with respect to the agricultural capacities of this extended section of the country."

Much diligence is shown by the number of books and of botanical collections consulted; and the large amount of matter compiled by this writer is an earnest of the immense harvest to be reaped from the study of the vegetable productions of the immense area of country comprised within the Southern States, a tract presenting wide variations of climate and of geological formation, an extended sea-coast, an interior mountain range, with vast forests and an almost boundless expanse of prairie and of alluvial country in the course of the many large rivers that flow through it.

[1] *Resources of the Southern Fields and Forests, Medical, Economical, and Agricultural; being also a Medical Botany of the Southern States, with practical information on the useful properties of the Trees, Plants, and Shrubs.* By Francis Peyre Porcher, M.D., &c. New edition. Revised and largely augmented. Charleston, 1869. Pp. 733.

In treating his subject the author has adopted the natural system of botany, as laid down by Dr Lindley.

The economical, agricultural, and commercial value of several important plants is brought under notice by lengthy dissertations, the propriety of portions of which, in some cases at least, is doubtful in a treatise of this kind. It is certainly not the sort of treatise an agriculturist would turn to for practical guidance in the details of cultivation, nor a manufacturer for learning the processes of his art; and hence, in our opinion, many farming and manufacturing expositions might well be omitted. We may cite, as examples of uncalled-for detail, the accounts of indigo planting and preparation; of grape growing and the making of wine; of oak bark and tanning; of the cultivation of the mulberry and the rearing and management of silkworms; of the cultivation and manufacture of cotton and tobacco; and of the growth and various uses of Indian corn.

To the information, indeed, conveyed in these longer articles no exception can be taken; the fault we have to find is, that the work before us is not the appropriate vehicle for its communication, and we hope that in another edition Dr Porcher will find cause for curtailing these long articles, and be enabled to occupy the space so saved by more full and definite histories of the medical plants and of their virtues. Such information will be much more appreciated, in relation to the many important medicinal plants belonging to the Flora of the Southern States; some of which have already attained celebrity in Europe, but others of which are very imperfectly known, although probably equally deserving confidence and repute.

The author, indeed, has fallen into the besetting sin of compilers, in exercising too little discrimination in the quotation of opinions. Thus, relative to the uses of plants, he appeals frequently to the notions entertained by the old physicians, some of them dating from the early part of the eighteenth century; and, again, to crude and unconfirmed statements of many unknown writers in the ephemeral literature of the day. The reference to the former revives traditional marvels of the efficacy of very common and almost inert plants, whilst that to the latter affords no reliable knowledge, and often suggests what, *primâ facie,* is most improbable. An odd example for the curious in philology occurs at page 240, where a writer in a newspaper is quoted, who, in speaking of the sumach tree, derives its name—which, it would seem, he would write shoemach—from its value in making shoemakers' wax!

Although a critical examination of this treatise may find blemishes, a debt of gratitude is due to Dr. Porcher for a volume replete in information valuable to the medical profession at large, but particularly to those of its members resident in the United States.

Silvester on the Spleen.[1]—In this able pamphlet Dr. Silvester has started an entirely new theory as to the nature of the spleen.

In a brief history of the views held concerning this organ, he he remarks that most of the theories with regard to it can be classed under one or other of the two following heads :—1. That it is the imperfectly developed left homologue of the liver. 2. That it is an originally mesial symmetrical organ, having no connection with the liver either in structure or function. With the latter view he entirely dissents. With regard to the former, he observes that one great difficulty in considering it as the left homologue of the liver, is the fact that it is ductless, and has never been shown to secrete bile.

Nevertheless, he inclines to this view, and regards the spleen as the left homologue of only a portion of the liver. We will now endeavour to give as shortly as possible an outline of his theory and the grounds upon which he bases it.

He thinks that while the alimentary canal from the mouth to the end of the œsophagus, and from the commencement of the large intestine to the anus, is, undoubtedly, mesial, and bilaterally symmetrical, as is shown from the division of the palate, the raphé of the tongue, &c., and from the fact that these portions of the digestive tract are supplied with double arteries, yet the intermediate portion with the organs attached is only in reality the right half of a bilateral symmetrical set of organs. Of the left set of these organs the cardiac portion of the stomach, the spleen, and vermiform appendix, are the only traces, the spleen being the left homologue of a portion of the liver, and the vermiform appendix that of the small intestine.

The stomach, small intestine, and liver are, therefore, not mesial and symmetrical, but lateral organs; for he considers that were the stomach fully developed according to his hypothesis, the cardiac portion of it would be furnished with a valvular apparatus similar to that of the pyloric end, and that to this would be attached a left small intestine with a left pancreas and liver.

This theory he chiefly bases on the law of symmetry, and on the fact that the stomach, liver, and small intestine, are only supplied by single not by double arteries, whereas were they mesial, bilaterally symmetrical organs, they would with this method of blood-supply form an exception to the rule observed in all other laterally symmetrical organs, which are always supplied with double arteries, one arising from each side of the aorta.

He also gives an outline of the different forms of stomach in the

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[1] *The Discovery of the Nature of the Spleen.* By HENRY SILVESTER, B.A., M.D. London.

vertebrate series, to show that this organ is merely separated into right and left portions *distinct in structure and function,* and is not merely a dilated part of a mesial canal.

He furthermore adduces several other facts in support of his theory; for instance, the situation of fœtal liver in the right hypochondriac region, rather than in the middle line; the absence of a double portal system, one for each side of the abdomen; the facts that the lacteal system of the left side of the body is smaller than that of the right side, and is not connected with the small intestine; that there is only one umbilical vein, although there are two umbilical arteries, &c.

We mentioned above that the author considered the spleen to be the left homologue of only a portion of the liver, and this statement needs explanation. Dr. Silvester shows that the liver is a complex gland, composed of at least two sets of apparatus, a biliary and a glycogenic, and he thinks that there may be a third set with functions similar to those of the spleen.

The function of the spleen is evidently that of purifying the blood by producing certain changes in its constitution—as is shown by the enormous hypertrophy of the organ in certain cases of blood poisoning,—and also that of forming colourless blood-corpuscles, as exemplified in the greatly increased number of these cells present in the blood of the splenic veins.

But the spleen, Dr. Silvester argues, both from the smallness of its size, and from the small proportion of the entire blood of the body which passes through it, is incapable of producing by itself the entire amount of these corpuscles. Now, their number is found largely increased in the hepatic vein also, and, therefore, he thinks that the liver as well as the spleen is employed in their production.

He asserts also that a fibro-cellular structure supporting blood-vessels, such as forms the substance of the spleen, may be observed in the liver, and with this portion of that organ he thinks the spleen is the left homologue. If this be the case, which, indeed, we consider not improbable, the extirpation of the spleen without ill effects to the system may be accounted for from the fact that one of two smaller glands is able to perform the functions of both, when the other is absent from disease or other cause; as, for instance, in the absence of one kidney the other can perform the amount of work proper to the two.

Dr. Silvester's opinion that the spleen is a sanguiferous gland coincides with that of most modern observers, but we think that the bilateral theory of the liver, stomach, and small intestine, requires greatly stronger evidence to support it than has been, or than, in our opinion, can be adduced. In this essay, Dr. Silvester brings forward facts in comparative anatomy to support his theory, by showing that in certain cases, one of two bilateral organs may be developed to an extent very different from the other. He cites, in illustration, the left incisor of the male narwhal, which reaches the length of six or eight feet, while the right remains of an ordinary size; the left ovary and oviduct in birds, which alone are functionally developed, the right being obstructed at an early stage; the difference in size between the right and left lung in ophidia.

But in all these cases evidence of the normal type of the organs is shown, either in certain stages of their development in the very individual in which they afterwards become abnormal, or even in other species of the class to which these abnormal individuals belong the type fully exemplified in its normal development.

But Dr. Silvester can adduce no evidence of this sort to show that the digestive tract from the end of the œsophagus to the commencement of the large intestine is formed upon a double bilaterally symmetrical type, for in no species in the whole animal kingdom is there any trace of two pyloric orifices or of two separate small intestines, and we may not rashly assume simply from a law of symmetry that such is the type of these portions of the digestive apparatus.

Moreover, we do not consider that the distribution of blood to these organs by single arteries can be considered as very weighty evidence in favour of the view that they are not originally mesial and symmetrical; for the aorta itself in the early stages of development is divided near its origin into two branches, and a right and left aorta are actually present throughout life in the lower vertebrata, although in mammalia the right branch is early obliterated, and the left is alone functionally developed.

Besides, the arguments concerning the division of the stomach into right and left portions, *structurally and functionally distinct*, may be adduced in reality in opposition to the bilateral theory; for this hypothesis would be more probable were the two portions of the stomach similar in structure and function; for how can one organ be the homologue of another from which it differs in both these points? But we do find differences of structure and functions in different portions of the length of the alimentary canal, which render it more probable that the cardiac portion of the stomach is a dilatation of the upper portion of a mesial canal, and not a lateral homologue of the pyloric portion. This view is confirmed by comparative anatomy, for in the lowest fishes the alimentary canal, including the stomach, is quite straight, and there is no appearance of a division into right and left portions, but the cardiac portion is a dilated continuation of the œsophagus. On the whole, we incline to the view that the spleen and the digestive apparatus are originally mesial organs, and this view is not shaken by the possibility of a portion of the liver being devoted to the same functions as the spleen. For it is probable that the thyroid gland and supra-renal capsules have much the same functions as the spleen, which has by some modern anatomists been classed with them; at the same time it is certain that the spleen is not homologous with them, and, therefore, it follows that the spleen need not be regarded as homologous with a portion of the liver, because they both perform similar functions.

Space forbids us to enter further into the arguments for and against the theory examined. We conclude, then, by stating our opinion that, although Dr. Silvester has come to a true conclusion with regard to the functions of the spleen, yet his theory concerning the structural relations and homology of that organ is incorrect.

We advise those interested in the subject to study the pamphlet, which certainly has the merit of being both original and ingenious.

The Year Book of Pharmacy.[1]—This volume is one of great value and interest, and will, we trust be the precursor of a series of the same excellent quality. It contains abstracts of papers relating to pharmacy, materia medica, therapeutics, and chemistry, contributed to British and foreign periodicals during the year ending June, 1870; and after these abstracts comes an account of the proceedings at the Pharmaceutical Conference held at Liverpool last year. Among the most striking features of this book we have particular pleasure in pointing out the fruit of scientific research, and the just and honorable view of the duties of the pharmaceutist which it exhibits. A very long notice would be required to do thorough justice to the merits of this volume; but we may rapidly indicate its chief contents by giving a list of those subjects which are treated with considerable fulness. Amongst these we reckon the peculiarities of foreign pharmacy (p. 27); discoveries and experiments in materia medica (pp. 67, 96—146); novelties in physiological and pathological chemistry, such as the sulpho-carbolates and preparations of chloral (p. 147); chemical analysis, apparatus, and manipulation useful in pharmacy (p. 157); and miscellaneous notes and observations (pp. 177 and 258). The bibliographical notices of chemical and pharmaceutical works are useful and well written, but the last hundred pages or so of the volume present matters of really unusual interest. "A Century of Old Books" relating to pharmacy and allied subjects engages the attention of some eight reporters, and we are bound to say that the notices of these old books are far the most singularly entertaining, and withal instructive. Though there are a few useful hints to be got from some of the old-fashioned prescriptions and formulæ, yet there are certainly some scores of humorous recipes which it is difficult to do anything with but laugh at. Take, for instance (p. 32), a German receipt of the year 1693 for the *Extractum carnis, Liebig,* of that period. To prepare *Extractum mumiæ*

[1] *Year-book of Pharmacy.* Issued by the British Pharmaceutical Conference. Pp. 596. London, 1870.

you are directed to "Take of the mummy of a healthy man, either hanged or broken on the wheel, that is, of the flesh of the thighs, arms, and other parts, q. s. This, having been once exposed to the rays of the moon and sun, cut into small pieces, and sprinkle with powdered myrrh, or a little aloes: afterwards macerate twice for some days in tincture of elder or juniper. Dry the pieces in the air, exhaust them with spirit of wine, distil off the spirit, and you have the extract remaining." Though there are many other things comic and curious culled from the one hundred old books exhibited at the Pharmaceutical Conference at Liverpool (a treatise of just 900 pages entirely devoted to nutmegs was included in the series), yet there are other matters of weightier and permanent interest to which the reader's attention is here called.

On the whole we can cordially recommend this book to all persons interested in the proper development of pharmacy, and anxious to know how scientific chemistry is helping, and may help forward a true and complete knowledge of materia medica.

Lonsdale's Life of Heysham.[1]—Many a life is written and many a name dragged from the land of forgetfulness, when the claims for a biography are most questionable, and charity towards the reading public would be better observed by leaving the inevitable oblivion undisturbed. To many the name of the subject of this memoir will, we apprehend, be unknown; but the readers of the biography will be thankful to Dr. Lonsdale for making them acquainted with the history and work of Dr. Heysham.

It seems, indeed, late in the day to resuscitate the history of a man dead some thirty-seven years, but Dr. Lonsdale may find apologies for so doing, both in the fact of his own early reminiscences of the man and of his respect for his character, and in the desire to secure to him the honour of collecting the materials for constructing the Carlisle tables, which have for many years formed the basis for estimating the value of life, in use by the principal Life Assurance Associations, although their originator may have been largely forgotten.

The rescue of Dr. Heysham's name from oblivion is, therefore, a commendable proceeding, and the readers of the 'life' will have no cause to regret its being written. It is, indeed, a most interesting biography, not laden with eulogy of its subject, but bringing him before the world as a living, busy man, determined to play his part and do his best in the sphere of life in which he found himself placed.

The reader is carried back to modes of life, to a state of society,

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[1] *The Life of John Heysham, M.D., and his Correspondence with Mr. Joshua Milne relative to the Carlisle Bills of Mortality.* Edited by Henry Lonsdale, M.D. London, 1870. Pp. 173.

and to a professional phase of existence, each and all so unlike what obtain at the present time, that it seems hard to believe so great a change to have occurred within less than a century. The picture of the times, of the men and their manners, is drawn by a well-trained scholarly hand, and cannot fail to interest and entertain.

After a brief notice of his family history we are introduced to the young Heysham as a country apprentice, roughing it in a manner that would be destructive of many of the carefully nurtured youths who now enter the profession. After five years' training in Mr. Parkinson's surgery, his next move was to Edinburgh, and, in the absence of other means of locomotion, this journey of 170 miles had to be performed on horseback. At Edinburgh he studied physic under Monro, Cullen, Black, and others of fame, and obtained his doctor's degree in 1777. After a visit to Holland he settled the following year in Carlisle, where he spent the remainder of what may be described—except so far as the works he engaged in, and of which the results remain—as an uneventful life. We find him the friend and associate of the leading people of the old city and its neighbourhood, interesting himself in the public matters of the locality, aiding various schemes of public utility, and performing very assiduously the duties of a magistrate. The sketch of the worthy doctor in this last capacity will be read with much amusement, and, we may add, with some wonderment; for it would seem that the performance of his magisterial duties was a source of considerable income, eking out very materially the comparatively small return of £400 a year derived from practice.

Following the custom of his time, he drank freely, though no drunkard. He associated on terms of equality with various fellow-citizens who held a sort of pre-eminence as "three-bottle men," and both he and they "lived to a great age, and enjoyed life to the very last."

Dr. Heysham had considerable claims as a naturalist. From his early days he was ardently given to the pursuit of natural history, and from his own researches obtained an intimate acquaintance with the fauna and flora of Cumberland. Ornithology appears to have been the favorite branch with him. Unfortunately he did not collect and publish his observations as he might have done, and excepting some fifty pages in Hutchinson's quarto 'History of Cumberland,' no record of them exists.

However, his chief claim to fame rests on his assiduity in collecting from the bills of mortality of Carlisle those data which form the basis of the celebrated Carlisle tables. His intimate knowledge of the people and of the circumstances influencing their longevity, gave precision to statistics which could not otherwise have possessed such value as fitted them for the purpose to which they were applied by Mr. Joshua Milne, the actuary of the Sun Life Assurance Society, by whose labours they were reduced to the form available for the valuation of life.

In an appendix Dr. Lonsdale gives the correspondence between Heysham and Milne relative to the information collected and the circumstances of the construction of the Carlisle tables. Such correspondence will especially commend itself to actuaries of life offices. The details of Dr. Heysham's life, and of the state of provincial gaiety prevailing, will interest not only the professional but likewise the general reader. The manner in which Dr. Lonsdale has performed his part cannot be too highly praised.

Ancient Works on Syphilis.[1]—However it may be accounted for, there are signs just now portending of a new revival or second *renaissance* in old medical lore at the hands of the syphilographers. So little is it the result of general conviction that we must fain credit them on the occasion with an earnestness all their own. No greater advance, it must be confessed, has been recently achieved in medicine than that which refers to syphilis, as its remote and natural consequence, a variety of visceral lesions. Long ignored, we acknowledge them at length; and it is, no doubt, in a very high degree satisfactory to find the view confirmed, or shall we not rather say anticipated, by those who have long ago preceded us.

But perhaps it is on too narrow grounds that we rest this novel fascination. When we find so many truths of earnest disputation in our day to have been settled long since by these authors, when we think upon these contestations, the uncertainty of each and all particulars, and find them resolved, in a sense that accords with our ultimate experience, by so many mediæval physicians, it is easy to account for admiration, even carried sometimes to excess by observers among ourselves who have most experienced the cost of inquiry, and suffered the toil of investigation. The incorrectness of these ancient authors in point of clinical remark bears no degree of comparison with what they have faithfully recorded.

Physiology in those days was so wondrous low, the Galenic doctrines so obtrusive and predominant, that little might be looked for of a sound complexion in a maze of tedious futilities; but the contrary is really the case; the clinical sense was not often betrayed, nor was their pathology so wholly wide of the mark as might have been expected beforehand. As regards these mediæval writers, after

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[1] FRACASTOR, *La Syphilis*, A.D. 1530. *Le Mal Français*, extrait du livre 'De Contagionibus,' A.D. 1546. *Traduction et Commentaires.* Par Dr. ALFRED FOURNIER, Médecin des Hôpitaux, &c. Paris, 1870.

JACQUES DE BETHENCOURT. *Nouveau Carême de Pénitence et Purgatoire d'Expiation, a l'usage des malades affectés du Mal Français ou Mal Vénérien.* Ouvrage suivi d'un dialogue où le mercure et le gaïac exposent leurs vertus et leurs pretensions rivales à la guérison de la dite maladie, A.D., 1527. Idem, Idem.

years of clinical study, and the sifting of a thousand minutiæ with all the machinery of statistics, and the microscope ever at hand, strange were it indeed if we stood not now at a level to judge them at their desert. What gives much weight to the collection before us is that the author is no mere humanist, nicely versed in classical and medical reading, but one who is firmly established in repute as a master of clinical study. Of high and surpassing scholarship there is no pretension made, the tone is of moderate pitch, and somewhat too apologetic, perhaps, for one who undertakes the advocacy of the forsaken. Let us say that the aim is practical, and directed toward skill in medicine. We shall but glance at the work of Fracastor; that is to say, his celebrated poem. Born in 1483, from mere tenderness of age he could hardly realise in fullest extent the great strong agitations of the outbreak which alarmed and surprised his contemporaries. The poem bears date A. D. 1530, but he was engaged many years, no doubt, in the performance. The prose works of Fracastor have scarcely a minor value or an inferior degree of interest. We pass on to an author far less or not at all known to our generation. The work of Béthencourt was the earliest published in France upon the same disease. Strange to say it was printed A. D. 1527, though the manuscript dates from A. D. 1525, very much in arrear of other countries. The disease had then prevailed, he says, some thirty years or so; it had spread with great rapidity, affecting a vast number of persons. If we are to believe this author, at the time he wrote it was of more frequent occurrence than all other diseases combined. The work of Jacques de Béthencourt is a small volume of 120 pages; it is written in Latin. As laid before the reader, it has been subject to wholesome retrenchment. The French, or as he prefers to call it, the Venereal disease, arises from sexual intercourse, but also, in rare instances and exceptionally, from chaste and innocent contact. Always, however, in these conditions it is initiated by the like sores as are wont to betray its origin in the common path of contagion; that is to say, primaries arise in natural course from infection by secondaries. The instance he gives of this is the sore that comes in the mouth of the infant infected by its nurse. He distinguishes consecutive symptoms; the further from its origin the more it changes its "physiognomy." He notices the Protean type of the disorder; it is composed, he says, of so many maladies. It is also at times hereditary. Many years it lies in abeyance and is occult; a man that has had it is like a fouled cask for ever, he is subject to sudden outbreaks and complications that sound men escape from. It is worse, he says, in those well advanced in age. Among the viscera the brain is the most liable to be affected, next the liver; the heart is much protected, it occurs therein but rarely. The bones suffer, though not, indeed, at first, while the cartilages, ligaments, and tendons are implicated far less often, and in an inferior degree the contrary is the case with the nerves and also the membranous parts. The muscular web is very much concerned; the pain, indeed, is chiefly found about the middle of the limb. Spleen, kidneys, lungs, and fatty tissue have more immunity. Sometimes there is œdema, pulmonary ulceration, consumption, small ulcers in the trachea, the pharynx, and also the œsophagus. There is, indeed, a world of curiosity in this small book of Béthencourt. He would seem to believe in a reiteration of the complaint. Not the least interesting part of the work is the dialogue or disputation. How can you tax me, says Mercury, with such defects? When I am given for psora, for tinea, and phthiriasis, do you then find these ulcerations of the throat, of the palate, nasal fossæ, and uvula of which you now accuse me? Do you find the same of the foulness of the breath? Béthencourt declares himself for mercury, but it must be given with much scruple and conscientious care. It is truly a sort of purgatory which the patient has to pass through.

Remarks.—It is only the external use of the drug which comes in question. Is the disease old? is the patient broken in health or aged? then your dose of mercury must be small, the treatment more prolonged. The same with the guiaicum, it is given according to the Indian method; but can the patient bear this forty days' fast, this strict and trying regimen? In many cases it will have to be refused. The collection of these handy books will comprise John de Vigo and Thierry de Héry, and we are also promised selections from Leonicenus, Torella, Villalobos, Ulrich von Hütten, Massa, Fallopius, Fernel, and others.

Poggio on Epidemic of "Red Fever" or Dengue.[1]—The red fever is that which was known to the people as *tracazo* or stroke, as it occurred in Andaluzia and other provinces of Spain during the years A.D. 1865-7. This does not appear to have been its first occurrence in the peninsular. In Cadiz and Seville, during the past century, A.D. 1764-8, there is described by Spanish writers a notable epidemic of the same type, then called *la piadosa,* the piteous disease. Elsewhere it has been termed *la pantomina;* it is the polka of Brazil, the dandy fever of Saint Thomas. In the West Indies it is more familiar to experience than elsewhere (see *Historia de una epidemia padecida en Curazao y la Habana, por el Dr. D. José García Arboleya, Cadiz,* 1854, page 141.) It is called the *colorado,* by the Spanish population in these islands, as well as on the American continent. The author last-named describes it well, but has not distinguished it from scarlatina. As occurring in Santa Fé

[1] *La calentura roja observada en sus apariciones epidémicas de los anos* 1865 *y* 1867. Por D. Ramon Hernandez Poggio. Madrid, 1871.

The Red Fever as observed in the Epidemics 1865 *and* 1867. By Don Ramon Hernandez Poggio. Madrid, 1871. Large 8vo, pp. 74.

de Bogotá, it is treated of by a French physician, Leblond, at the close of the past century (see *Observations sur la fiévre jaune et sur les maladies des tropiques, Paris,* 1805, page 44). In S. Cruz de Teneriffe and the Canary Islands, it was studied by our author, A.D. 1865. This is the *dengue, breakbone fever, rheumatic scarlatina* of the English and American writers. Cook and Stedman among the English; Dutrouleau, Barmer, Thaly, Ballot among the French, have written of it. The further descriptions are much scattered in our literature, but there is a good though brief summary in Aitken's text-book. A great variety of names have been applied to the complaint, but the French generally agree to call it the *red fever*. Nor has this been done without protest, for one of their authors, D. F. Thaly, has remarked that as found prevailing among the negroes there is no such redness to be seen, but simply an interruption of tint—a very apparent variation in the skin. Martinique, Goree in Africa, and the Isle of Bourbon have been the chief fields of observation respecting this fever for the French. It has been also described by Dr. James Mellish, in Calcutta, under the name of inflammatory epidemic fever. It is not unknown in British India and on the American continent. The circumstance that Sydenham has called scarlatina the red fever will ever hinder, we suppose, the French designation from being acceptable to us. There is small doubt that the dengue is contagious, though much favoured in its development by atmospheric conditions, the most apparent being heat and moisture. An example is given from the record of 1764, of its infection being carried by the breeze to a vessel hugging the shore from Cadiz to Cape St. Vincent, while some others further off escaped (page 36). In this distressing but rarely fatal complaint, Dr. Poggio regards the eruption as a most essential feature; it is not accidental, a mere epi-phenomenon. Its absence in certain cases, or in a certain group of cases, must be considered on the same footing as an *exanthema sine exanthemate*.

He describes the eruption as occurring from the second to the fourth day, generally after a remission obtained from the anterior febrile oppression through diaphoresis; at the same time there is felt great debility and prostration of the vital powers, which is a notable feature in the malady, as it is just the same as in influenza. The implication of the mucous tract is, however, far less than in the latter, and this suffices to distinguish them. The desquamation is extensive and considerable. In the Canary Islands the eruption assumed chiefly the form of scarlatina; in Cadiz that of roseola. The pains are severe, sometimes beginning in the joints of the toes, sometimes in the region of the spine. Early lumbar distress, great cerebral oppression, and, as well as the rheumatic pains, various hysteriform phenomena attend the disorder. More rarely, as in Teneriffe and Africa, bilious and typhoid conditions complicate it and render the prognosis less favorable. Imminent danger attends the employment of the lancet, evacuants check and prolong the evolution of the complaint. Head oppression and nervous disturbance are best combated by valerian, camphor, asafœtida, ether, ammonia, and even by Dover's powder repeated in moderate doses. A yellow condition of the skin and other signs of hepatic congestion are best dealt with by an ipecacuanha emetic. Demulcents, lemonade, various tisanes, especially that of the lime flower, *tilleul* of the French, with a warm bottle to the feet will generally carry the patient through the disorder till the stage for giving tonics arrives, and these may be begun as soon as the stomach allows of it, with generous and supporting food. The malady is sufficiently grave to endure three weeks, and relapses are not unfrequent. We regard this work as a valuable addition to the history of this strange exotic. It contains ample references, and the tone is thoughtful and philosophical throughout.

Dr. Fox on Cold in Hyperpyrexia.[1]—The profession will be very pleased to have Dr. Wilson Fox's very suggestive and practical lectures, as originally published in the 'Lancet,' reprinted in the present form as a book. Indeed, this volume is more than a reprint, for it contains additional details of the illustrative cases, "tables of the observations made on the temperature during the treatment by cold applications, and also during their later stages," two charts of temperature and one of sphygmographic tracings, the history of a case previously treated, and a tabular outline of twenty-two cases of hyperpyrexia recently published.

These lectures indicate a course to be profitably pursued in developing a system of scientific and rational therapeutics. It is true, indeed, that Dr. Currie, at the beginning of this century, advanced far on the same road, and erected a basis for the development of a system of treatment by cold water, but he lived in advance of his time, and his contemporaries were possessed of too many theoretical notions derived from the old doctrine of fluxions to give heed to his experience, and were too timid to test it for themselves.

With Dr. Fox's clinical records in hand, it will be interesting to turn to Currie's old volumes, and to compare the experience and views of these two physicians. Every reader of Dr. Fox's brochure will be persuaded that we have a potent therapeutical agent in cold water, suitable not only for desperate cases, such as he particularly refers to, but also to a wide category of diseases in which the thermometer indicates a pyrexial state.

The practitioners of hydropathy ought, indeed, to have, ere this, satisfactorily laid down physiological principles for the application of

[1] *On the Treatment of Hyperpyrexia, as illustrated in Acute Articular Rheumatism, by means of the external application of cold.* By Wilson Fox, M.D., &c. London, 1871. Pp. 78.

cold and also of hot water, but empiricism and boarding-house economics have too much beset their practice and vitiated their experience.

In the history of cold as a remedial agent we must also not forget Dr. Chapman, who has zealously advocated its application in numerous diseases. His enthusiasm, indeed, has jeopardised his cause; but at a future time, when the uses of cold have been established on a physiological and pathological basis, Dr. Chapman's name will be referred to as that of a prime mover in forcing their consideration upon the profession. The manner in which Dr. Fox has handled the subject of cold in hyperpyrexia, in these lectures, will, in our opinion, constitute a turning point in rational therapeutics.

Trousseau's Clinical Medicine.[1]—It will be gratifying to the subscribers of the Sydenham Society to have this fourth volume of these justly celebrated lectures placed in their hands; and from the date of issue hitherto maintained, to anticipate the early completion of the work. The present part is occupied by lectures on dyspepsia, gastritis, and chronic ulcer of the stomach; on diarrhœa in adults and children; on lactation and dentition; on dysentery and constipation; on fissure of the anus, and intestinal occlusions; on hepatic colic and calculus; hydatic cysts and malignant jaundice; on syphilis in infants; on gout and on nodular rheumatism; on rheumatic gout, and on acute articular rheumatism and ulcerating endocarditis.

The contents, therefore, are rather miscellaneous; but the student may be sure of a thoroughly practical consideration of the several subjects, and of great clearness in the enunciation of the pathological truths and inferences put forward. The examination of the special teaching of Trousseau must be made, as occasion offers, in the discussion of those subjects respecting which he has displayed the most originality in thought and in observation.

Senac on Hepatic Colic.[1]—Whether justly or unjustly, it is frequently, one might very well say usually, the case, that when a work emanates from some physician residing at a health-resort—be it distinguished as a sea-side, or a potable saline or mineral water resort, or simply as a place remarkable for the beauty of its scenery and the salubrityof its climate—the object of such work is supposed to be, principally, to advocate and advertise the same as a residence for invalids, with the very natural desire of promoting the prosperity of the place, and of advancing the well-doing of the local

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[1] *Lectures on Clinical Medicine.* By A. Trousseau, M.D., &c. Translated from the edition of 1868 by John Rose Cormack, M.D., &c. New Sydenham Society, 1871. Pp. 470.

[2] *Du Traitment des Coliques Hépatiques.* Par le Dr. H. Senac, Médecin à Vichy, 1870.

medical men generally, and that of the writer in particular: we say that, whether justly or unjustly, this is the usual idea with which such publications are received, and it was with some such feeling we took up Dr. Senac's volume of 260 pages—a volume that is well printed on but indifferent paper, and with the thinnest of covers.

A glance at the introduction, which is a long one—twenty-three pages—and at some parts of the body of the work, soon assured us that Dr. Senac's book merits a higher position than is usually accorded to the class of publications before mentioned.

The first paragraph of the introduction gives the ground on which the author has based his observations. He says, "Le travail que nous offrons au public médical est basé sur les résultats d'une pratique de onze années, à Vichy, station thermal où affluent, de toutes parts, les malades atteints de Coliques Hépatiques."

The last paragraph of the introduction suffices to show the motives he was influenced by when carrying on his work, and the object he aims at. "Appelons de tous nos vœux les travaux d'hommes plus autorisés et qui viennent corroborer les nôtres ou les contredire, si nous avons vu mal, ou mal apprécié. Et puissions-nous avoir été assez heureux pour contribuer, dans le mésure de nos forces, à faire avancer l'art de guérir vers le but de nos efforts: la vérité."

Beyond this, the introduction also serves as a sort of "key" to what may be expected to be found in the four chapters into which the book is divided; and this, not only as regards the writer's own views of the nature of the affections he designates "Coliques Hépatiques," but also as to the various theoretical points on which he ventures to differ from other authorities.

The first chapter, which has several divisions, is devoted to a description of symptoms: it includes the prodromatic symptoms—symptoms present at, during, and after "la crise hépatique:" then follows an appendix to the chapter with an interpretation of the symptoms, an illustrative case, and the author's conclusions.

The second chapter gives the etiology of the condition, and recounts the various predisposing and determining causes: this chapter, too, has sub-heads, and an appendix. In the latter we find a description of "la nature arthritique des Coliques Hépatiques."

The third chapter is headed "Rapports des Coliques Hépatiques, avec diverses manifestations arthritiques." In this part of the work there are given numerous cases under the somewhat obscure description of "Observations." The general divisions of the chapter are devoted to "Lithiase Urinaire," "Calculs Urinaires," "Gravelle Urinaire," "Goutte," "Rhumatisme," "Asthme," "Arthritides." Like the two preceding chapters this one has also an appendix.

The fourth and last chapter is a very long one and is devoted solely to treatment. It contains a lengthy and painstaking account of everything Dr. Senac has tried for the relief of all the various states and conditions described in the preceding portion of the book. Every chapter, it will have been observed, has had its appendix, and it is not until we come to this supplementary part of chapter number four, at page 206, that we find the treatment by Vichy baths, Vichy water and air, specially referred to and described.

The volume is intended, as expressed by the author in his introduction, to be an essentially practical one, and consequently the reader must be prepared to find, or rather *not* to find, any of those interesting accounts of experimental physiology with which such works as Frerichs, Fauconneau Dufresne, Budd, and Murchison, are interspersed. Dr. Senac confines himself to observations on certain purely functional disorders of the liver, and what he believes to be resultant conditions: his treatment of these states may be looked upon, as a whole, as being determined principally, if not entirely, by symptoms.

Undoubtedly the book may be read profitably by every practitioner; but we fail to see that it has opened up, in a satisfactory manner, any new ground in connection with that long list of dyspeptic ailments with which deranged hepatic function is so very usually associated.

Liverpool Medical Reports.[1]—We have so recently noticed the fourth volume of these reports that we have really nothing to add to, or to take away from, the observations we then made with regard to the merits and defects of this publication.

The fifth volume, like its predecessors, has about a third of its pages occupied with abstracts from the proceedings of the Liverpool Medical Institution, the Microscopical Section of the Medical Institution, and the Liverpool Northern Medical Society.

The original articles are practical in their character, but very brief, and consist of contributions by Dr. Skinner, Dr. W. Carter, Dr. Wallace, Dr. J. C. Brown, Dr. Cleaver, Dr. W. Little, Dr. Weaver, Mr. G. E. Walker, Dr. Lyster, Dr. De Zouche, Mr. Higginson, Mr. Wood, Dr. T. R. Glynn, and Mr. Abraham.

We think the articles, generally, do not compare unfavorably with those in former volumes; and the illustrations, of which there are several, deserve to be spoken of in terms of high praise. The volume, in point of paper and printing, is exceedingly well got up.

Physiological Anatomy and Physiology of Man.[2]—After a lapse of about five years Dr. Beale has at length brought out Part II of the

[1] *The Liverpool Medical and Surgical Reports.* Edited by P. M. Braidwood, M.D., and Reginald Harrison, F.R.C.S.

[2] *The Physiological Anatomy and Physiology of Man.* By R. B. Todd, W. Bowman, and Lionel S. Beale. A new edition by the last-named author. Part II of vol. i. London, 1871. Pp. 152.

first volume of his edition of "Todd and Bowman's Human Physiology and Anatomy." This part treats of the tissues, their development, nutrition, and microscopic anatomy, and contains a full description of all the more simple tissues, together with chapters on cartilage, bone and fat.

In it, with the aid of numerous and well-drawn plates, Dr. Beale exemplifies his theory on the method of development and nutrition of tissue by the germinal matter or bioplasm, but space forbids us entering into an argument on the details. Coming from the hands of such a skilful and accurate microscopical observer, the work is of course very valuable; but it is much to be regretted that so much time is allowed to intervene between the appearance of each part. At the present rate of progress, by the time the work is completed, the earlier parts will of necessity become, to a great extent, out of date, and therefore comparatively useless, owing to the rapid strides now made in our knowledge of the subjects of which it treats.

The "Medical Courier" of Lisbon.[1]—We hail with much pleasure the appearance of a review in Portugal, which seems calculated to fill up the gap made by the cessation in 1869 of the *Escholiaste Medico.* The present undertaking has for sponsors the medical gentlemen Clemente dos Santos, J. Ferraz de Macedo, J. T. da Silva Amado, J. M. Alves Branco. In the two first numbers we find a communication of singular interest concerning that curious affection the African lethargy, in which the patient appears enwrapped for weeks and even for months in one long sleep. There is, however, a degree of intermittence in the disorder. It affects principally, if not exclusively, the black race. It would appear not to be due to miasmatic influences; and some attribute it to the lymphatic constitution of the blacks. Sr. Ferreira Ribeira of the *Portuguese medical service da ultramar,* who has resided some time in the islands San Thomé and Principe, and who has raised this subject for discussion, seems inclined to attribute the affection in great degree to *helminthiasis.* We cannot say, however, that he has thrown much light upon the subject; nor do we learn much that is useful and edifying from the long debate that ensued in the Society of Medical Sciences. A certain mystery seems to hang over the affection from want of wider experience. Sr. Ribeira is of opinion that when the disease is confirmed there is little to be done for it. One case which he observed of such sleep, in the island of Principe, lasted more than a year; another treated by him in hospital, in the island of San Thomé, lasted more than six months. The same gentleman, Sr. Ribeira, addresses the Society of Medical Sciences on the conjec-

[1] *O Correio Medico de Lisboa.* Publicaçao quinzenal. 1 de Julhi, 1871. *The Medical Courier of Lisbon.* Fortnightly medical review, from 1st July, 1871.

tural enfeeblement of the therapeutical action of quinine in paludal fevers from the previous employment of the drug in the same patient as prophylactic. We observe also in the records of the meetings of the society a discussion on the waters of Penedo (Villa Pouca de Aguia). These waters contain lithium and arsenic; they gain on public favour, competing with those of Vidago; they seem of use for dyspepsia, squeamishness of stomach, chlorosis and hypertrophy of liver or spleen. We cannot but wish well to this journal, which, up to the present date, seems conducted with skill and good sense.

Transactions of Clinical Society.[1]—This fourth volume of the 'Clinical Society's Transactions," maintains the high character remarked in former volumes, as a record of well-observed and narrated cases, many of them of great pathological interest. On the whole, moreover, therapeutical memoranda abound more in the present than in some past volumes; although the value of the Society, as an organization for the advancement of therapeutics especially, remains yet to be demonstrated.

Besides the President's address, the volume contains forty-two communications, of which rather above a third refer to lesions falling more especially within the province of the surgeon.

The annual address, delivered by Dr. Gull, the President, exhibits that general readiness of perception and clearness of opinion which would be looked for from that highly skilled and popular physician. He expresses his desire to see further developed the Society's scheme of appointing committees to investigate particular clinical and therapeutical questions, rightly considering that by associated action much more can be effected than by the work of individuals; that "by putting positive questions to nature, we are more likely to find out her secrets than by waiting, however patiently, for her own revelation of them;" and that experience in medicine being fallacious, because it is limited and imperfect, the concurrent action of several trained minds affords the best available means for counteracting this defect.

After noticing the wide and unexplored fields demanding clinical research, he proceeds to remark on the advancement of therapeutics as the end to be aimed at by the Society, and advocates observation on the natural course or history of disease, interference by drugs being deferred until a healthy equilibrium be restored by physiological and mechanical rest. "It seems probable (he urges) that a large number of acute diseases may be sufficiently treated by following these indications of rest only."

In this present age, and the existing state of public opinion,

[1] *Transactions of the Clinical Society of London.* Vol. iv. London, 1871. Pp. 200.

this "sweet do-nothing" plan, if acceptable to philosophic physicians, will find little favor with the world at large, and particularly with the victims of acute disease, who, if denied tangible doses by the regular doctor, will call for the globules or drops of the homœopath, or for the decided doses of the irregular practitioner.

The various readers of the 'Transactions' will naturally value, almost as variously, the forty-two communications recorded in them; nevertheless there are some, doubtless, which will more generally commend themselves to notice by their originality or novelty, or by their connection with topics that are especially the popular theme of the day. As examples of communications, challenging attention on one or other of these grounds, we may mention the records of cases of skin-grafting, the case of recovery from tetanus, the case of vaccino-syphilis, and the cases of skin disease treated by phosphorus. Those last-named have much more than ephemeral interest, inasmuch as they exemplify a mode of scientific therapeutic research from which much may in time be gathered. They, indeed, constitute a continuation of a series of experimental inquiries by Dr. Broadbent, relative to the hypothesis that "chemical groups form therapeutical groups," the first portion of which was published in Vol. II of the Society's 'Transactions.'

In conclusion, we may congratulate the members of the Clinical Society upon the contents of the volume issued at their charge, and recommend its possession to those who are not members.

Dr. King Chambers' Harveian Oration.[1]—In this little book the reader will recognize the same independence and vigour of thought and clearness of diction which characterise the works of the distinguished author.

The brochure contains the Harveian oration, delivered at the Royal College of Physicians of London, in June, 1871, and two supplements or sequels. In the former portion the special subject is the modern theory of disease, which represents disease as "a mode of living, as an imperfect form of undeveloped vitality, as a loss of something present in health." In the sequels he opens up to discussion a much wider and more varied field of argument, dealing with many of the principal medical and medico-social topics of the day. In so doing he has adopted the form of a dialogue between several individuals of supposed different positions of life, and of diverging views respecting the matters discussed. The dialogue is well sustained, and the conclusions arrived at, if not all beyond cavil, are always both ingenious and plausible.

[1] *Restorative Medicine: an Harveian Annual Oration.* By Thomas King Chambers, M.D. With two Sequels. Philadelphia, 1871. Pp. 85.

In the oration, Dr. Chambers notices, in turn, the several theories of disease promulgated since medicine has been cultivated as a science in connection with the therapeutical doctrines dependent thereon. Coming down to the present date, he finds that the prevailing idea, as illustrated in the new articles of medicine introduced, and the mode of treatment in vogue, is that of supporting and conserving or of holding up. In short, Dr. Chambers finds consolation from the review of the present tendencies of physic, seeing therein the progressive acceptance of that "restorative medicine" with which his name has become identified.

This small book comprises, within its few pages, a collection of subjects, medical and social, which will occupy men's thoughts, and awaken discussion for many years to come. Dr. Chambers has, in regard to most of the questions raised, a decided opinion, and every reader of his arguments will respect those opinions; but this is not the place to attempt an examination of them. And with respect to his doctrine of restorative medicine, this has been developed in treatises of much greater magnitude and of more special purpose; and cannot be equally advantageously examined in connexion with a brochure like the present, written as much for the perusal of the non-medical as of the medical reader. At the same time let it be understood that, in these observations, we would in no wise depreciate the value of this smaller production of his pen, for no one can read it without awakening reflection, and furnishing suggestions worthy of note.

Hamilton on Fractures and Dislocations.[1]—Hamilton's treatise is so well known and appreciated in this country that any lengthened description of this fourth edition is unnecessary. The author has evidently taken pains to keep up with the progress of surgery in relation to the subject of his work, and has materially improved both the letterpress and illustrations of the present edition.

In the treatment of fractured clavicle two novel methods are described and illustrated, in both of which the tension of the muscular fibres of the pectoralis major is proposed to be employed for the maintenance of the fragments *in sitú*. The method devised by Dr. E. M. Moore, of Rochester, New York, is termed the "Figure-of-eight from the elbow," and is thus described:

"The end of the middle finger rests upon the ensiform cartilage while the elbow is pressed against the side of the body. A single band, two and a half yards long and eight inches wide, has its centre laid against the point of the elbow and folded around the arm; the

[1] *A Practical Treatise on Fractures and Dislocations.* By Francis Hastings Hamilton, A.M., M.D., LL.D., Professor of the Practice of Surgery with Operations in Bellevue Hospital Medical College; Surgeon to Bellevue Hospital, New York, &c. Fourth edition, revised and improved. Illustrated with 322 woodcuts.

extremity which appears in front is now carried upwards over the front of the corresponding shoulder, obliquely downward across the back to the opposite axilla, and through the axilla to the front. The other extremity, emerging behind between the elbow and the body, is carried obliquely upward to the sound shoulder, and forward over this shoulder to be tied the opposite extremity of the shawl or bandage coming from the axilla. The forearm is then placed at an acute angle and suspended by a narrow sling passing under the wrist."

Dr. Lewis Sayre (who lately visited this country) employs

"Two strips of adhesive plaster about three and a half inches wide for an adult; one long enough to encircle first the arm and then the body completely; the other of sufficient length to reach from the sound shoulder over the point of the elbow of the broken limb, and across the back obliquely to the point of starting. The first strip is looped around the arm just below the axillary margin and pinned or stitched, with the loop sufficiently open to avoid strangulation. The arm is then drawn downward and backward until the clavicular portion of the pectoralis major is put sufficiently on the stretch to overcome the sterno-cleido-mastoid, and thus draw the sternal fragment of the clavicle down to its place. The strip of plaster is then carried completely around the body and pinned or stitched to itself on the back. The second strip is then applied, commencing on the front of the shoulder of the sound side, thence it is carried over the top of the shoulder diagonally across the back, under the elbow, diagonally across the front of the chest to the point of starting, where it is secured by pins or threads. A longitudinal slit is made in the plaster to receive the point of the elbow."

Several illustrations are given of these methods, and Hamilton speaks well of Sayre's method, though he prefers his own, which consists of an axillary pad with a sling from the opposite shoulder, the forearm being enclosed in a sleeve and fastened across the body.

We notice that in the treatment of Colles' fracture of the radius Hamilton does not refer to Gordon's splints, which have come into very general use in Ireland, and are employed in many London hospitals. They are described and figured (though not very accurately) in the last edition of Druitt's 'Vade Mecum.' The chapter on the treatment of fractures of the femur was always very elaborate, and it now includes all the varieties of suspension apparatus for this injury which appear to be such favorites in America, though they have not superseded the simple "long splint" in this country. In the treatment of fractured patella we note that Hamilton does not agree with Mr. Hutchinson in dispensing with all elevation of the limb.

The chapter on dislocations of the femur has undergone considerable modification, owing to the introduction of the views of Dr. Bigelow, of whom the author remarks, "Since Sir Astley Cooper wrote, probably no man has thrown so much light upon the subject of hip-joint accidents, or contributed so much towards an accurate and systematic plan of treatment, as the distinguished Boston surgeon." Dr. Bigelow's views depend chiefly upon the importance he attaches to the ileo-femoral or Y ligament in relation to dislocation of the hip-joint. The regular dislocations are those in which the ilio-femoral ligament has not been ruptured, and the irregular dislocations where it has been torn. In order to reduce the dislocation the ligament must be relaxed, and the reduction effected by rotation of the limb; this method of reducing the dislocation upon the dorsum ilii is to flex the thigh upon the abdomen, abduct, and then rotate outwards; or to flex, then adduct and rotate a little inwards to disengage the head of the bone from behind the socket, then to abduct and pull directly upwards. The same directions apply to the dislocation in the sciatic notch. In the dislocation on the thyroid foramen Bigelow's directions are: "Flex the limb towards a perpendicular and abduct it a little to disengage the head of the bone; then rotate the thigh strongly inward, adducting, and carrying the knee to the floor," on which the patient is presumed to be lying. In addition to the ordinary dislocation of the head of the femur several anomalous dislocations are described, but Hamilton does not adopt without reserve Bigelow's statement that in all these the ilio-femoral ligament is completely ruptured.

Space will not allow of our making further extracts, but we can hardly say too much in commendation of a work which has properly become a medical classic.

Geographical Distribution of Heart Disease and Dropsy in England and Wales.[1]—It is a praiseworthy undertaking on Mr. Haviland's part to endeavour to represent the geographical distribution of disease in England and Wales, and every reader of the present essay will at once be conscious that it is also an undertaking that involves great labour and requires much skill. The portion now under notice is put forward as the first of a series of ten parts, to be produced from time to time until the work is complete, the next promised section being on the geographical distribution of cancer.

The plan as struck out by Mr. Haviland is of great magnitude, and its production a matter of great cost; we hope, therefore, the profession will afford its enterprising author the full encouragement he needs, and which also he most richly deserves. The undertaking is novel, an honour to British medicine, and calculated to promote the pursuit of a department of pathology hitherto greatly neglected.

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[1] *The Geographical Distribution of Heart Disease and Dropsy in England and Wales. Illustrated by a large Coloured Map.* By Alfred Haviland, M.R.C.S., &c. &c. Pp. 61.

To return, however, to the portion now before us. The data and conclusions will require, in our opinion, very considerable revision. The investigation is very honestly and ably entered upon, but we are not satisfied with the data relied on. These are derived from the returns of the Registrar-General for the decenniad 1851-60. Whilst recognising the general value of these returns, we are not prepared to receive them in evidence of the geographical distribution of disease. The looseness and inaccuracy of the returns of causes of death are circumstances well known to all medical men. These defects will, we trust, in course of time be progressively diminished, and we should assign a higher value to the returns for the decenniad 1861-70 than to those used by Mr. Haviland.

It seems to us, moreover, that the author has kept too prominently in his mind the meteorological conditions of localities as most influential in the production of heart disease and dropsy. He particularly insists on the salutary effects of winds, giving preference in respect to these to the west and south-west winds; and he certainly shows cause for the views he entertains, considered generally, for some curious exceptions occur respecting which no satisfactory explanation appears. Indeed, the inspection of the map suggests other causes for the prevalence of the diseases considered besides the much-prized free exposure to the wind. One such other cause occurs to the mind as probably to be found in the soil and drainage; and another one certainly obtains in the social and sanitary conditions and habits of the residents in particular localities. This latter is, in fact, one which Mr. Haviland has been compelled to fall back upon in some instances where his usual explanation is at fault.

Moreover, in reference to some localities he is not sufficiently acquainted with their geographical conditions and their geographical features. In illustration of this remark we would refer to his notice of the West Midland Counties division. Admitting rheumatism to be the most frequent cause of heart disease, a direct relation should obtain between the two with regard to prevalence; and in examining this relation no one could acquiesce in ignoring all other determining causes of rheumatism other than free ventilation by winds, knowing how largely rheumatism is due to certain occupations and to accidental circumstances quite unconnected with site and exposure to winds.

We are well acquainted with a district in which rheumatism abounds, and with it heart disease; and yet this locality appears on the map as fourth in order, having an average below that prevailing in the country at large. Indeed, we suspect that Mr. Haviland's statistics lose force as he extends his inquiries to smaller groups of population, for it is with such that accidental conditions assume the most significance, and tend to diminish the value of statistical calculations.

Some general deductions are worth noting. It is, for example, clear that heart disease kills more females than males, except in the first ten years of existence, and that the mortality from it among the population advances in each succeeding decade; "each decade has a relative mortality nearly double of the one which precedes it." Again, the mortality from heart disease is greater in places where the mortality from all causes is less; and further, it is less on the coast of the country than inland, and generally less where there is free exposure to the winds than where places are sheltered from them. Another deduction is that "the high mortality districts" have an agricultural population greatly in excess of the average for England and Wales." Lastly, in Mr. Haviland's opinion, rheumatism is endemic in many parts, and that this condition depends on a local *materies morbi.* In this opinion the hereditary character of rheumatism and of heart disease is lost sight of.

Notes on Skin Diseases.[1]—It is enough to note the appearance of this new edition of this excellent though brief history of skin diseases and their treatment. We hope that the success of the venture will not induce Dr. Liveing to make many additions, and thereby change its character from a convenient pocket companion to an ambitious volume, to occupy a place—and it may be a nearly forgotten place—on the library shelves. This undesirable transformation has not, we are happy to say, been yet made, and we are again pleased to commend the little volume to all our readers, especially to those engaged in practice whose memory requires from time to time refreshing.

Nouveau Dictionnaire de Medecine, &c.[2]—The subscribers to this very comprehensive Dictionary of Medicine and Surgery will receive with pleasure this thirteenth volume, although it advances them but a small way on the road to completion, the articles in the alphabetical order adopted reaching only from Em to Erg. Certainly some most important subjects fall within this brief alphabetical range; such are the articles Encephale, Endémie, Endocardites, Entozoaires, Epaule, Epidémie, Epilepsie, and Epithélium. There are also some very considerable essays on Epistaxis, Erectile tumours, Ergot of rye, and Ergotism.

The diseases of the encephalon are treated of by the general editor, M. Jaccoud, aided by M. Hallopeau. The long article on endocardium and endocarditis comes also from the prolific pen of M. Jaccoud. Mr. Alfred Luton is the writer of the complete essay on entozoa; M. Paul Lorain of the two articles on endemics and

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[1] *Notes on the Treatment of Skin Diseases.* By Robert Liveing, M.D., &c. Second Edition, with Additions. London, 1871. Pp. 104.

[2] *Nouveau Dictionnaire de Médecine et de Chirurgie pratiques, illustré de figures intercalées dans le texte.* Tome xiii. Em—Erg. Paris, 1870.

epidemics; M. Voisin of that on epilepsy, and M. Panos of the surgical article on the shoulder.

There is no doubt about the ability and talent of the contributions to this dictionary, but it is a question how far such colossal literary undertakings are desirable. The time taken to complete such is so great that the value of the earlier volumes must have materially deteriorated, particularly in so changing and advancing arts as medicine and surgery, before the conclusion of the series. France is especially the country of such undertakings, and much credit is due to the enterprise exhibited in their production; but, after all, should a contributor live to see the completion, and to be able to regard with patriotic satisfaction the array of thirty or more bulky tomes on his library shelves, he must still be beset with the conviction that much of the system of medicine and surgery before him possesses chiefly an historic interest, and does not represent the science and art of the day.

We heartily wish M. Jaccoud health and vigour to complete the great task he has undertaken, and can congratulate him on the general excellence of the many articles that have been contributed, whether under his supervision or by his own hand.

Handy-Book of the Treatment of Women's and Children's Diseases.[1] —The translation of this little book has been well made, and the subject-matter of the book will have interest to those practitioners who desire to know the peculiarities of German therapeutics. Moreover, the notice of remedies and the formulæ will prove suggestive to the reader in search of additions to his armamentarium. Numerous and much varied, indeed, are the prescriptions set forth, some of them placebos and herb preparations such as are to be seen in the dispensatories of the last century; whilst others again are as potent, derived from the chemical laboratory. Descriptions are also given of various operations for surgical lesions of females, as, for instance, of ovariotomy; but we regard these as rather encumbrances of the treatise, being far from explicit enough to be a guide to a would-be operator. They, moreover, do not represent the present improved methods of operation, particularly those pursued in England and the United States, where this department of surgery has been most advanced.

Dr. Nicol deserves credit for his part of the work, but we cannot comprehend why he selected this manual for appearance in an English dress. It is not the work of a representative man in German medicine, but of a compiler of "Handy-Books," previously known among students by the preparation of similar treatises on

[1] *Handy-Book of the Treatment of Women's and Children's Diseases, according to the Vienna Medical School. With Prescriptions.* By Dr. Emil Dillinberger. Translated by Patrick Nicol, M.D. London, 1871. Pp. 208.

medical and surgical diseases. The success of those former productions may have been a sufficient inducement to Dr. Dillinberger to produce one also on diseases of women and children for the medical tyros of Vienna, but this gives no explanation for Dr. Nicol taking up such a production for translation for English students and medical men, in whose hands so many superior treatises on the same subjects are to be found.

Manual of Practical Therapeutics.[1]—This new edition of Dr. Waring's Manual of Therapeutics will be welcomed by practitioners generally; particularly when it is known that the work has, to a great extent, been re-written, in order to make it convey adequately, without useless augmentation of size, the results of modern inquiries into the use and operations of medicines. Chloral, bromide of mercury, iodide of methyl, bichloride of methylene, protoxide of hydrogen, sandal-wood oil, apomorphia, and other new remedies, are noticed in this new edition; and, likewise, a more complete account is given of several drugs that have of late advanced in public estimation. Space, also, has been found, even with a decreased number of pages, for articles on the hypodermic and endermic methods of treatment, and to additions to that on antidotes, setting forth the most approved methods of treatment in cases of poisoning. There is, in short, an amount of information on therapeutical matters that may well stagger the practitioner who first opens the pages of this treatise; but at the same time the inquiring reader who looks to it for reference will value it accordingly, as putting before him, in a condensed form, the opinions and the results of observations of a multitude of medical men respecting the many drugs which have established a claim to recognition as aids in treatment; however, the practitioner will especially appreciate this work as presenting to him therapeutical information without that mass of natural history, chemical and manufacturing details, which usurp in books on materia medica so large an amount of space, but possess so little interest to him after having duly ridded his mind of them at his examinations.

The composition of the book is a marvel of industry and of patient research after information in all quarters.

Dr. Ringer's Therapeutics.[2]—This treatise on therapeutics at once assumed the first position among kindred works on the subject in the English language. Medical men could at a glance learn the reputed properties of drugs, their action on the human frame, the indications for their use, and the mode of using them, without

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[1] *A Manual of Practical Therapeutics, considered chiefly with reference to Articles of the Materia Medica.* By EDWARD JOHN WARING, M.D., &c. Third Edition. London, 1871. Pp. 875.

[2] *A Handbook of Therapeutics.* By SYDNEY RINGER, M.D. Second Edition. London, 1871. Pp. 483.

stumbling over much irrelevant matter, as met with in the mongrel contents of an ordinary treatise on Materia Medica. Another good feature in Dr. Ringer's volume is, that it is not made up of excerpta of opinions and of results of experience collected from a host of books and periodicals—of opinions and results, by the way, ofttimes of no weight by reason of deficient critical power on the part of their recorders—but, on the contrary, presents a careful and critical digest of the views and experimental inferences of observers selected as most competent to put them forward.

In our review of this work, in the number for April, 1870, we took occasion to advert to the imperfection of the arrangement followed with reference to many articles treated of. We find the same defect in the present edition, and it is one probably inseparable from the mode of dealing with the several subjects pursued by the author. He appears to form therapeutical groups of medicines, where possible; but where this plan fails, the arrangement adopted is not clear. The reader who would refer to the properties and action of any particular medicine must, therefore, depend on the index appended at the end of the book.

In the review alluded to, the general features and merits of the treatise were enlarged upon, and it is, therefore, unnecessary, in noticing this second edition, to revert again to them. At the same time, it will be satisfactory to the reader to know that the "present edition has been carefully revised, and much additional matter has been incorporated with it;" yet, we observe, without increasing the number of its pages.

It is right also to note that a copious posological table is appended, and also a dietary for invalids, giving numerous recipes for articles of nourishment required in the sick room.

In conclusion, we have great pleasure in recommending this treatise on therapeutics to our readers as a useful practical production, and one creditable to British medicine.

Transactions of the Pathological Society of London.[1]—These two copious volumes of 'Transactions' bear witness to the vitality of the Pathological Society of London, and to the activity and industry of its members. They likewise sustain the reputation gained by the transactions of past years, and afford a large repertory of facts that may be turned to the best account by pathological students.

The form and character of the contents of these records of the Society's work are so well known as to render description unnecessary, whilst at the same time the nature of the contents precludes critical analysis.

The reports of the Committee on some of the morbid growths

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[1] *Transactions of the Pathological Society of London, comprising the Report of the Proceedings for the Sessions* 1869-70 *and* 1870-71. Forming vols. xxi and xxii.

exhibited will always be referred to with interest, as at times clearing up obscure points in morbid anatomy; or, if they do not succeed in this, as opening up fresh questions, and indicating new lines for research.

Ever and anon there is a tendency on the part of members to indulge in the luxury of a pathological essay, instead of making the exhibition and history of a morbid specimen the goal of their ambition.

There can be no objection to such papers, considered as contributions to medicine, from careful and intelligent observers and thinkers; at the same time, the multiplication of such papers would change the character of the work of the Society, and deprive it of its special claim to recognition as an independent Society with an avowed but limited object.

The Pathological Society has from its institution been a popular association; its list of members shows that it still retains its popularity, and its volumes of transactions give proof that it deserves it.

Dr. Hartshorne's Essentials of Medicine.[1]—It is a year since we noticed the second edition of this handbook of medicine; the appearance of another edition within so short a time may be taken as evidence that the treatise is much appreciated. Although we had to remark on some deficiencies and a few errors, our opinion of the former edition was very favorable. Of this third issue the same good opinion may be expressed, and we are pleased to find the author has taken note of our remarks, and made the corrections and additions we ventured to suggest.

Journal of Anatomy and Physiology.[2]—We have before us the last published number of this most valuable scientific Journal of Anatomy and Physiology, which, with reference to those subjects, brings English scientific journalism to a level, at the least, with the prolific periodical productions of the German press. Much honour and praise are due to Professors Humphry and Turner for their enterprise in starting, and for maintaining, their journal in its high scientific character. They deserve the fullest encouragement from the anatomists and physiologists of this country. Indeed, the number of contributors to its pages shows that its value as a vehicle for the more advanced researches in the sciences it is devoted to is largely appreciated.

The present number sustains the high character recognised for the

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[1] *Essentials of the Principles and Practice of Medicine. A Handbook for Students.* By Henry Hartshorne, M.D., &c. Third Edition, thoroughly Revised. Philadelphia, 1871. Pp. 487.

[2] *The Journal of Anatomy and Physiology.* Conducted by Professors Humphrey and Turner. November, 1871.

Journal by those that have preceded it. It contains seventeen communications and a "Report on the Progress of Physiology." The papers deal with questions on both human and comparative anatomy and physiology, and some among them address themselves to medical men who would not arrogate to themselves a special scientific position, but who make it their business to learn the advances of physiology in their bearings upon practice.

Omitting articles that are chiefly of interest to comparative anatomists, we may note the presence of papers on "The Brain of an Idiot;" on "The Action of Inorganic Substances when introduced directly into the Blood;" on "The Relation of the Temperature of the Air to that of the Body;" on "Uterine Contractions during Pregnancy;" and on "A new Schema of the Circulation."

Original Communications.

I.—On the Proper Management of Tedious Labours. By Dr. G. Hamilton, Falkirk.

(*Continued from No.* 96.)

In resuming the consideration of this subject, I would remark that I have always considered it of great importance, when a labour threatens to become lingering, to ascertain with precision the position of the head, and, if possible, also the form of the pelvis. Both these are important guides if the forceps should be required. When labours have been easy, I confess to having often not paid much attention to the positions; and this has, no doubt, led me, in the following rough and ready arrangement of vertex presentations, to differ somewhat from that usually followed by authors. Mine, in fact, has relation to the use of the forceps, theirs to the whole labour, as well as to both easy and difficult labours. Setting aside cases where the head has already turned into the hollow of the sacrum, or where the face is to the pubes, I would divide vertex presentations into six kinds, regulated by the position of the ear placed anteriorly at the time when the use of the forceps is required. Though more numerous than usual, these positions are very simple. They are—1st, face to right side, with ear to the right of symphysis; at, or very near, symphysis; and on the left of it. 2nd. With the face to the left side, and the ear the reverse of these. We have here, therefore, six positions, of which the most favourable is the first, and the least favourable the sixth. Generally speaking, also, those with the face to the left are less favourable than when it is to the right, evidently because the rectum occupies a certain space on the left side. Although the first position of authors (with ear and face to right side) is by far the most common, taking all kinds of labours, I do not find that it is so when I have to apply the forceps. By far the most common situation in which I have then found an ear has been at or very near the symphysis, with the face sometimes to the one side, sometimes to the other (my 2nd and 4th positions); and this, and the other positions, I shall immediately have to show, become almost indispensable guides in the proper use of the instrument. It is easy to see why an ear near the symphysis is more common than the other positions, when a certain amount of arrest has taken place, because the ear may have been placed originally at the symphysis in my 1st, 2nd, 4th, and 5th positions, or it may have passed into this from my 3rd and 6th. When I require it, therefore, it is there that I generally seek for an ear, which in the great majority of cases is my principal guide in forceps cases; for I agree with Smellie and Burns in thinking that, "when the forceps are applied along the ears and sides of the head, the blades are nearer to one another and have a better hold" than when applied over the occipital and frontal bones,[1] or, indeed, anywhere else. A knowledge of these positions also shows what cases are likely to be most retarded, because, if a revolution of the head have to be made, the further it has to travel the greater, probably, will be the detention. Hence, my 3rd and 6th (on the *wrong* side of the symphysis, as I have called it) are on each side generally the most lingering, and most frequently require interference and rectification.

Continuing my narrative, I have now to state that the second class of cases in which I have had to apply the forceps is one in which the head is higher up than in the first, the ear near the symphysis, with the face mostly to the right side, but where the head, although the pelvis is roomy enough, though flat, obstinately refuses, from the *vis à tergo* alone, to take the turn into the sacrum. This kind of case I have mentioned in my paper in this Journal in 1853 as having particularly attracted my attention, especially from the arrest having occurred repeatedly in the same female—in one case as many as ten times out of twelve labours. The forceps here are generally pretty easily applied; the head yields to their joint rotation and traction, often with a jerk, and, after the face gets into the hollow of the sacrum, the delivery is quickly completed. The case will frequently have been so remarkably intractable before this rotation has been applied, and so easy of management afterwards, as to astonish one not prepared for it. Indeed, it is so simple as to have been effected by Dr. Montgomery with his hand alone. This I have almost never been able to do, but with the straight forceps rotation is often easy and most effectual, nature unaided, if allowed, going through the rest of the labour without impediment. Short, squat females are peculiarly liable to this kind of detention. Soon after I settled in practice I lost twins, where the patient, a primipara, had this make.

My opinion, at the time I published the paper referred to, was, that this description of labour constituted a larger proportion of forceps cases than I am now inclined to hold. I think the third class, which I have now to refer to, is more common and more important, and, as I shall attempt to show, it admits of a special treatment which requires a modification in our ideas, both theoretical and practical, of the utmost moment.

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[1] 'Smellie's Obstetrical Plates,' p. 13.

This third class consists of cases where an ear is found to be more or less easily within reach of the finger, at or near the symphysis pubis, on either side, and with the face either to the right or the left side, which do not naturally take the turn, and cannot, without the application of more force than it is advisable to apply, be made to take the turn, into the hollow of the sacrum.

As a proper view of these most important and often very serious cases involves a consideration of, 1st, the powers and operation of the forceps as an instrument; 2nd, of the mechanism of labour in relation to this instrument; and 3rd, of the kind and form of forceps best fitted to effect our objects, I shall make a few remarks on each of these points. After this has been done, my observations on this subject will be greatly simplified, and made much more intelligible.

And first, as to the powers and operation of the forceps as an instrument. On this department of the subject the observations of Dr. Barnes[1] are the most recent and minute that I have met with, and on these, therefore, I would beg to make a few remarks; and in doing so, though I shall have to say that I differ from some of the conclusions at which Dr. Barnes has arrived, I yet must express the interest with which I have read, and the instruction I have derived from, many of his papers on obstetrical subjects. Dr. Barnes says,

"Three distinct powers can be developed in the forceps. First, by simply grasping the head and drawing upon the handles, it is a 'tractor.' Secondly, the forceps consisting of two blades, having a common fulcrum at the joint or lock, we can, by a certain manipulation, use it as a *double lever*. Thirdly, if the blades are long enough and strong enough, and otherwise duly shaped, the forceps becomes a compressive power capable of diminishing certain diameters of the child's head."

And again he says:

"First, as to its tractile powers. In order to draw, the instrument must take hold. How does it take hold? You may, at first sight, suppose that this is accomplished by grasping the handles; but in the case of the ordinary forceps, especially the short-handed forceps, there is little or no compressive power, so that the hold cannot be due to the handles. The hold is really due to the curvature of the blades, which fit more or less accurately upon the globular head, and the compression of the bows of the blades against the soft parts of the mother, supported by the bony ring of the pelvis. This may be made clear by a simple experiment. Take an india-rubber ball, slightly larger in diameter than a solid ring; place the ball upon the ring, then seize the ball through the ring by the forceps; the blades

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[1] See 'Med. Times and Gazette,' July 27th, Sept. 7th and 21st, 1867, pp. 85, 249, 313, quoted in 'Braithwaite's Retrospect,' vol. lvi.

will be opened out by the ball; then drawing upon the handles, even without squeezing them together, you will see the blades pressed firmly upon the ball by gradual wedging, as the greatest diameter or equator of the ball comes down into the ring. Just so is it with the child's head and the pelvic brim and canal. The blades are held in close apposition to the head by the soft parts and pelvis of the mother. In many cases this outward pressure upon the bows of the blades is enough to account for traction."

And again:

"Thus, just as the pressure of the soft parts and the pelvis is a main agent in fixing the forceps upon the head, so is it in moulding the head to allow of its passing. Indeed, I think this pressure almost entirely accounts for the alteration of form the head undergoes when the English forceps is applied."

With many of these propositions and conclusions I confess myself unable to agree, for, first, excluding the pressure of the mother's parts altogether, if I take a croquet ball ten inches in circumference, put it in a cotton bag, and attach this to a spring balance, I can easily, using my forceps, figs. 1, 2, p. 179, with both hands, make the index of the balance stand at 62 lbs. before they slip. Again, instead of finding the compressive power "inconsiderable in almost all the English forceps," I find, when I drive two strong nails into a board, say eighteen inches asunder, and attach the spring balance to one nail, and pass a loop of strong cord over the other, so that the two blades of the forceps can be made to act, the one on the balance and the other on the loop of cord, that I can with my two hands make the index stand at 70 lbs. with my forceps, figs. 1, 2; at 62 lbs. with the instrument I have used until lately for thirty years; and at 64 lbs. with the short forceps, fig. 3. As the index gives the power represented by only one of the blades, and its opposite must be the same, we have here respectively 140 lbs., 124 lbs., and 128 lbs., for the powers of these three instruments. Now, let any one try to lift a weight of 140 lbs. with both his hands, and he will realise the compressive power of the forceps. Exerting nearly all my strength, I am barely able to lift it. If anything like this force were not diffused over a considerable surface, and restrained within certain limits by the width between the blades, the head of the child would inevitably be crushed, and its life sacrificed instead of saved, by the use of the instrument.

And here it may be noted that the proper use of the forceps forms not quite such a "gentle" operation as it has been represented by some authorities. "Force," says Dr. Ramsbotham, "is a word which should be expunged from the vocabulary of obstetrical phrases."[1] This, I think, is a mistake; only it requires, in applying the force, that we should know well what we are doing. In general, I

[1] 'Obstetric Medicine and Surgery,' p. 338.

would say that Dr. Ramsbotham is correct as to the *introduction* of the forceps; but in extraction nature uses, and we often require to use, very considerable force, and this, I am satisfied, can be applied, *if not too much prolonged,* with perfect safety to mother and child. It is *prolonged* pressure that is especially hurtful to both, and this whether it be applied by means of the forceps or by the resistance to the *vis à tergo* by the bony unyielding maternal canal. When the hand has to be introduced into the uterus in turning, we get, during a pain, an idea of the power nature brings into operation. In my early practice I have seen force used in the introduction of the forceps which was altogether unwarranted, and, I now believe, unnecessary. Knowledge and skill are here the chief desiderata, and especially a correct diagnosis; but force, and very considerable force, must occasionally be used in extraction. This need not alarm us much, when Dr. Haughton tells us that the expulsive powers of nature in parturition amount to something like a quarter of a ton,[1] even though this estimate is held by Dr. Mathews Duncan to be much exaggerated.

Let us now further inquire what are really the powers brought into operation by us when we use the forceps. Dr. Barnes says, "we can, by a certain manipulation, use it as a *double lever;*" but it again seems to me that we never, properly speaking, use the forceps as a double lever, except when we grasp the head, and in the sense that all pincers are double levers, having their fulcra at the joint. But this is not the real power exerted in the common mode of using the forceps, for the instrument is then used more as a double hook or loop than as a lever. The leverage power is exerted, in such a case, simply in laying hold of the head, not in extracting it. This will become evident if we imagine an unyielding band passed round the blades of the forceps when placed on the head. In such a case no leverage power might be used at all, and yet the child might thus be supposed to be extracted by the forceps, acting as two hooks, or combined as a loop, passed round the head or round the croquet ball. The leverage exerted by the hands on the handles of the instrument, therefore, simply represents the power conferred on us by this imaginary band when we are exerting traction. When we employ, in delivery, leverage power, we act, properly speaking, not with a double but with a single lever, and this is effected by converting, as it were, the forceps and head of the child into a single bar, the one end of the bar thus being the head stopped in its passage. By working on the other end the head may gradually be dislodged, but the power employed is mostly traction, for the leverage power usually exerted in extraction is remarkably small. It consists, indeed, nearly altogether, as Dr. Barnes says, in using "slight movements

[1] 'On the Muscular Forces employed in Parturition.'

of laterality or oscillation." I shall presently have to show that in my practice I have used the leverage power of the forceps in a much more efficient way; and this, in fact, is by far the highest power possessed by the instrument.

As this point is important in relation to my practice, I may illustrate it by the following example:—If we take a large malleable iron headless nail, which will bend somewhat, and drive it partially but firmly into a tree, this may be supposed to represent the head of the child. In extracting this wedge we can employ three methods:—1st, laying hold of the wedge with a pair of sharp-pointed pincers or double lever, we can extract it by simple traction, and in this way we can also deliver the child, but the power employed is not great; 2nd, we can extract it or the child by uniting with the traction a motion from side to side, which is traction and leverage combined. This is more powerful than the first, but the leverage power obtained is not great. 3rd, We can extract by laying hold of the wedge, and forcing the pincers and it to move in part of a circle *round the tree.* The force employed here is in great part leverage, and is much greater than in either of the other two, and, as I shall immediately have to show, it is that which I now mainly employ in cases embraced under my third class. If, after extraction, we look at the wedge and the hole from which it has been drawn, what do we find? This certainly, that the edge of the hole in the tree, on the side on which we made the revolution, is depressed or indented, because it has served as our fulcrum, and the part of the wedge which was in the tree has been somewhat bent, because it had not strength to resist the leverage of the bar (consisting of pincers and part of the nail) brought to bear on it. Precisely the same thing takes place in extracting the child in my mode of operating. It must be noticed, however, that the forceps do not press upon, or make a fulcrum of, the parts of the mother, exactly in the same way as the pincers never come in contact with the wood of the tree. We see something like the same process brought into operation when a smith extracts with his convex-shaped pincers a *projecting* nail from the shoe of a horse, only that he makes, at a certain stage of the extraction, the shoe his fulcrum. At first, however, the cases are identical, and every one knows what immense advantage the leverage he employs gives him in the extraction. This will give an idea of the mode in which I now frequently use my forceps; or it may be likened to the power we see acquired when a smith uses a bar in turning a vice (known as the "rack-and-pinion" modification of leverage); only, in my case, the forceps *embrace the head,* whereas in the case referred to the bar used for procuring leverage is *inserted in a hole.*

In the second place, in trying to clear up this part of the subject, I shall make a few remarks on what I have observed to be the mechanism of labour in the class of cases I am now treating of. The generally received opinions on this subject are well stated by Dr. Barnes,[1] in speaking of the application of his long double-curved forceps. He says:

"The pelvis has been compared to a screw. I think a better idea may be formed of its mechanical properties by comparing it to a rifled gun, and the child's head to a conical bullet. But even then the comparison is not complete, for the pelvis, unlike a gun, is a curved tube. Now, just as the head must traverse the pelvis in a helicine course, determined by the relation of form between pelvis and head, so is it natural that an instrument designed to grasp the head should be so modelled as to be fitted to follow this helicine course during introduction and extraction. This indication a well-modelled long forceps fulfils. No single-curved forceps can fulfil it."

Now, I contend that the pelvis in labour is not *necessarily* a screw or rifled curved tube. I maintain that, in many cases, it is a plain curved tube without any rifling; and I prove this assertion by stating that I am constantly in the habit of placing a blade of the forceps on an ear near, at, or even above the symphysis pubis, and that *neither it nor the opposite blade ever rotates or materially alters its central position till the head of the child is just being born.* The one blade and ear move steadily under and round the symphysis pubis, while the other blade and ear sweep along or near to the central line of the sacrum, until the child's head has passed, or is just on the point of passing, the *os externum*—often the former, but sometimes the latter. It is principally by using the forceps as a *lever* that this can be effected, and the appositeness of the illustration I have given, in which the nail is supposed to be extracted from the tree as we walk round it, will become apparent. In point of fact, I often make the handles of the forceps go through even more than a semicircle in this mode of extraction. I have constantly practised this, to me, entirely novel operation for twelve or fifteen years; and it appears to me, from its simplicity, from its safety, and from the otherwise difficult class of cases to which it can be applied, to be one of the most important in obstetrics.[2]

Of course, double-curved forceps could not be employed as I have used mine; they could not perform the curve I have described

[1] Loc. cit., and 'Braithwaite's Retrospect,' vol. lvi, p. 312.

[2] My son, who practises in Ashton-under-Lyne, writing me in September last as to some forceps cases he has had, says, "In one the head occupied your No. 5 position, that is, with the ear to the symphysis and the occiput to the right. The woman was of a squat make, and had previously been delivered with forceps with great difficulty. After the blades had been applied over the ears, I tried, as an experiment, the effect of a straight pull, with no result. I then gave the head a slight turn, when it was easily brought down into the hollow of the sacrum and delivered, making the sweep of a large segment of a circle, and never changing the position of the forceps until the head was born."

without injuring the mother. In other words, they necessarily lose the advantage of the leverage power I have referred to. We cannot, so to speak, go *round the tree* with them.

On the other hand, if the pelvis, in these instances, is not the screw spoken of, where can be the necessity of the curve in the forceps to fit into it? On this point I shall have something more to say immediately. But, in point of fact, I cannot "*go round the tree*" with *my* forceps, unless when they are applied and maintained, as it will be observed they are in this class of cases, with the *flat part of the blades* to the pubes and sacrum. The advantage thus gained as to space will be at once seen by looking at the measurements of forceps and parts. For example, I find, in an ordinary female pelvis, the width of the arch formed by the rami of the pubes to be, at its lower, middle, and upper portions, respectively $3\frac{5}{8}$, $2\frac{1}{2}$, and $1\frac{3}{8}$ inches; while my forceps measure, from blade to blade, at the widest part, $2\frac{5}{3}$ inches, and *across* each blade $1\frac{1}{2}$ inch. It will thus be seen that even their range (and the blades are $\frac{3}{8}$ths of an inch closer than most others) is extremely limited when the flat of the blades is towards the ischia; while when the flat of the blades is towards the sacrum and pubes, the bones admit of their going to nearly the top of the arch.

I come now, therefore, naturally to speak of the forceps I have employed, or which can be employed, in delivering in this third class of cases, as I propose should be done. As I have already mentioned, I have used for upwards of thirty years only the late Dr. Ziegler's straight forceps, and until lately only one pair of these. Shortly after Dr. Ziegler invented his instrument I called upon him in Edinburgh, and he recommended me to get the forceps which were then being made by Young, of Edinburgh. These consisted of the two blades which are now so well known, and also of a shorter blade, which Dr. Ziegler thought could occasionally be advantageously substituted for one of these.[1] This forceps is thirteen inches long, and is a most admirable instrument. Its ingenuity, simplicity, and great utility, have hardly yet, I think, been justly appreciated by the profession, arising, perhaps, a good deal, in Scotland at least, from the circumstance that the brilliant reputation of Sir James Simpson, who shortly afterwards designed, and of course recommended, an instrument of his own, overshadowed and kept in the background this, in my opinion, the best of all forms of the forceps I have seen. My original instrument, after

[1] I have hitherto managed to dispense with this addition, but I think it may be found useful, for example, where one blade has been fixed near the symphysis, and a projecting promontory of sacrum prevents the other from locking with it. In such cases I have managed by applying the blade next the sacrum first, throwing it well back. In one case of this kind I got hold of the head without the blades locking, and drew it down sufficiently to allow them to do so. The blades, applied over the cheek and ear, and opposite parietal bone, hold very well.

long use, I found to have certain defects, and on that account I have now laid it aside; but although I have done so, I look upon it with wonderful respect, as a servant that has been most useful to me in many trying situations.

The defects of this instrument I have found to be, that it was barely long enough in certain cases that have occurred to me, that it is not strong enough to be used as a lever in my mode of delivery or even with powerful traction, that the hinge has not sufficient "play" to allow the blades easily to lock,[1] and that the open fenestra in blade No. 1 is apt, when much leverage is used, slightly to injure the cheek or side of the head of the child next the pubes. Mr. Young has constructed me two instruments to remove these defects, and the one which I consider the better of the two is represented in figs. 1, 2, the figures being one fourth their proper size.

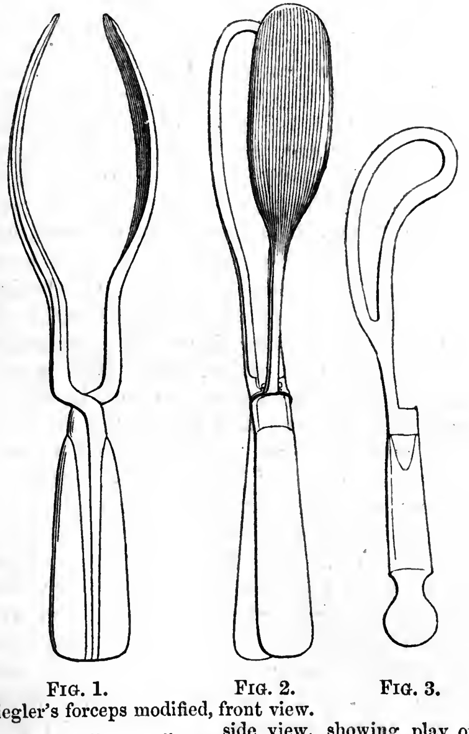

FIG. 1. FIG. 2. FIG. 3.

Fig. 1. Ziegler's forceps modified, front view.
„ 2. „ „ „ side view, showing play of hinge, closed fenestra in blade No. 1, and prolonged fenestra in No. 2. (One fourth proper size.)
„ 3. Short double curved forceps, in use forty years since.

[1] Mr. Young, who introduced this improvement into Ziegler's instrument, tells

The following are the dimensions of different forceps:

	Over all.	From middle of hinge to end of blades.	Greatest inside width of blades.	Between tips of blades.
	Inches.	Inches.	Inches.	Inches.
Sir J. Simpson's double-curved forceps	$13\frac{1}{2}$	$8\frac{1}{4}$	3	1
Dr. M. Duncan's ditto ditto . .	$12\frac{1}{2}$	$8\frac{1}{4}$	$2\frac{7}{8}$	1
Dr. Ramsbotham's ditto ditto . .	$12\frac{3}{4}$	...	$2\frac{7}{8}$	1
Dr. Beaty's straight ditto . . .	$12\frac{1}{2}$	...	3	$1\frac{1}{8}$
Dr. Ziegler's ditto ditto (figs. 1, 2, page 179)	$13\frac{3}{4}$	$8\frac{1}{2}$	$2\frac{5}{8}$	1
Short forceps (fig. 3, page 179) . .	$11\frac{1}{2}$	$6\frac{3}{4}$	$2\frac{6}{8}$	$1\frac{1}{4}$

Comparing the above forceps, I would remark, 1st, that Ziegler's (figs. 1, 2) are longer than any of the others, and, I believe, also stronger. 2nd, That Sir J. Simpson's, which come nearest to them in length, have two projecting horns, nine and a half inches from the extremities of the blades, which must, I should think, limit their introduction to this length, whereas those wanting these horns may be easily locked even within the parts of the patient, if necessary. These appendages were added, no doubt, to give additional traction power, but this is amply secured by the great strength of those I am using, 3rd, Mine, inside the blades, measure two inches and five eighths, and the others one eighth, two eighths, and three eighths of an inch more. Now this, again, may become important in difficult cases, for, as Burns properly remarks ('Midwifery,' p. 503), "forceps whose blades could come considerably within three inches of each other may, in a particular degree of contraction, act better and require less exertion" than others. 4th, With a width of three inches between the blades the instrument may be simply a double hook or tractor, while with two and five eighth inches it may lay hold of the head, and enable us to use it and the head as a lever. 5th, Dr. Beaty well observes ('Braithwaite's Retrospect,' vol. li, p. 361), that the "elongate curve" (which is given by the lesser width between the blades of Ziegler's forceps) renders the introduction of forceps more easy. 6th, The solid blade in figs. 1, 2, will, I think, be found a great improvement, not only by preventing, as I have said, injury to the skin of the child, but also by facilitating their introduction; and, when smeared with india-rubber paste, making the large friction-surface that lays hold of the scalp really of considerable importance. In fact, the forceps when thus assisted in laying hold of the skin, become almost, as it were, an instrument combining the properties of the forceps with what was aimed at by Sir James Simpson in his air-tractor. In fine, I think my instrument,

me that he has done the same with Sir James Simpson's, from whom I think he said he got this "wrinkle."

from wanting projecting parts, and thus being less liable than double-curved ones to injure the mother; from the ease with which it can be introduced and locked; from its great length, which allows us to reach all kinds of cases; and from its great strength, which enables us firmly to lay hold of, move, and mould the head, is superior to any other I have seen.

I have remarked that straight forceps can alone be used so as to gain the leverage power which I have indicated, and also that they must have their blades placed antero-posteriorly in relation to the pelvis. If we inspect the bony pelvis, I think we see the reason of this, as well as the advantages which are thus gained. The outlet of the pelvis has been described as an irregular oval space, with its long diameter placed from before backwards. I would rather incline to describe it as *two* irregular spaces, the posterior always oval and the anterior often so, running into each other at the point formed by a line drawn between the two spinous processes of the ischia ($c\ c'$), as seen at A and B, figs. 4 and 5. It is, I think, in the anterior space that the head in my second and third classes mostly gets fixed, and we may, I think, perceive that mere traction must often have very great difficulty, or that it may be found impossible,

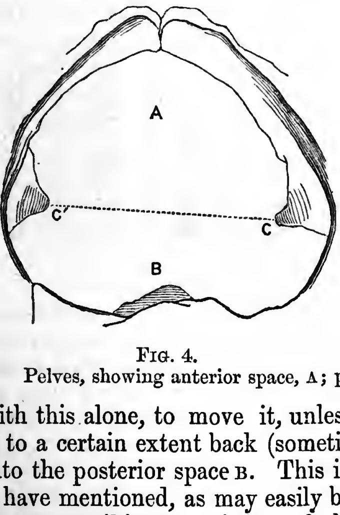

FIG. 4.

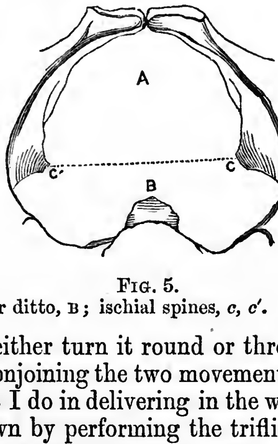

FIG. 5.

Pelves, showing anterior space, A; posterior ditto, B; ischial spines, c, c'.

with this alone, to move it, unless we either turn it round or throw it to a certain extent back (sometimes conjoining the two movements) into the posterior space B. This is what I do in delivering in the way I have mentioned, as may easily be shown by performing the trifling but very striking experiment of placing the forceps in the bony pelvis, and making them describe under the symphysis pubis the semicircle, or more, I have alluded to. I have been called in by brother practitioners, using, as I did, Ziegler's forceps, who have yet been unable to deliver under these circumstances after hours of traction, where I have succeeded at once by altering the position and direction of the instrument—applying leverage, and thus throwing the head back into the posterior space. My friends, I dare say, have been apt to think that I possessed a superior instrument to what they used, but the true secret of my success consisted in the different mode in which I used it.

Although the anatomical peculiarity I have referred to is at once seen by glancing at the photographs, figs. 4 and 5, yet, as this is a point of great practical importance, it may be useful to illustrate it still further by some measurements of pelves I have got taken, without any particular selection. In the following table the peculiarity adverted to is seen by looking at the difference in the measurements in the first and second columns; and some other measurements have been added, to show the general capacity of each pelvis, as well as to illustrate some other points. Nine of the pelves are normal, and two are somewhat distorted.

Nos.		Between the ischial spines.	Between the tubera ischii.	Between the promontory of the sacrum and pubes.	Transverse diameter of brim.	From hollow of sacrum to pubes.	Lower part of pubic arch.	Upper part of pubic arch.
		Inches.	Inches.	Inches.	Inches.	Inches.	Inches.	Inches.
1.	Natural.	$3\frac{6}{8}$	$3\frac{7}{8}$	$4\frac{3}{8}$	$5\frac{2}{8}$	$5\frac{1}{2}$	$2\frac{6}{8}$	1
2.	Natural.	$3\frac{6}{8}$	$3\frac{7}{8}$	$3\frac{5}{8}$	$4\frac{6}{8}$	$4\frac{5}{8}$	3	$1\frac{2}{8}$
3.	Natural.	4	$4\frac{2}{8}$	$4\frac{3}{8}$	5	$5\frac{1}{2}$	$3\frac{2}{8}$	$1\frac{2}{8}$
4.	Natural.	$4\frac{3}{8}$	$4\frac{5}{8}$	$3\frac{2}{8}$	$5\frac{3}{8}$	$4\frac{1}{2}$	$3\frac{1}{2}$	1
5.	Natural.	$5\frac{2}{8}$	$5\frac{1}{2}$	$4\frac{2}{8}$	$5\frac{5}{8}$	$4\frac{7}{8}$	$3\frac{3}{8}$	$1\frac{3}{8}$
6.	Natural. (fig. 4)	$4\frac{2}{8}$	$4\frac{6}{8}$	$4\frac{7}{8}$	$5\frac{5}{8}$	$5\frac{1}{2}$	$3\frac{5}{8}$	$1\frac{3}{8}$
7.	Natural.	$3\frac{5}{8}$	$4\frac{5}{8}$	$4\frac{6}{8}$	$5\frac{1}{2}$	$5\frac{1}{2}$	$3\frac{2}{8}$	$1\frac{5}{8}$
8.	Natural. (fig. 5)	4	$3\frac{1}{2}$	$3\frac{5}{8}$	...	$5\frac{3}{8}$		
9.	Natural.	4	$3\frac{2}{8}$	4	$5\frac{1}{8}$	$5\frac{1}{2}$	$2\frac{6}{8}$	$1\frac{1}{8}$
10.	Distorted	$4\frac{5}{8}$	5	$2\frac{6}{8}$	5	$4\frac{6}{8}$	$3\frac{2}{8}$	1
11.	Distorted	$2\frac{5}{8}$	$3\frac{5}{8}$	$2\frac{5}{8}$	$4\frac{2}{8}$	$4\frac{1}{2}$	3	$1\frac{2}{8}$

In the numbers from 1 to 7 in this table we see that the difference of the measurements between the ischial spines and between the tubera ischii goes on gradually increasing from one eighth of an inch to an inch. In No. 6 (fig. 4), the difference is only half an inch, and yet the oval form of both spaces is seen to be very decided. In such a pelvis as No. 7, where the difference is an inch, the anterior space may almost be spoken of as a trap, from which, if the fœtal head got in, it would have great difficulty in escaping. In Nos. 8 (fig. 5) and 9 the proportions, it will be observed, are reversed, and the head would therefore in these pass out of the anterior space with the greatest ease.

The third column, giving the measurements of the conjugate diameters, shows at once in what cases the uterus would be likely to be caught by the head and pelvis, and where even the head alone would have difficulty in passing.

The fifth, sixth, and seventh columns show the capacity of the pelves for allowing the use of the forceps with "leverage," as I have described the operation.

Having given these explanations of what seem to me the mechanism of labour, and of the powers and proper form of the forceps, I can now, I think, with greater profit, say a few words as to how I came to use the instrument in the way I have described. But in truth I can hardly do this in a systematic manner, for I may say it grew up rather than was invented by me; and I confess I did not at first see, as I afterwards have done, the novelty and importance of the changes implied by it. Perhaps the first thing that particularly attracted my attention was the different position in these deliveries required to be occupied, in the bed, by the attendant. Formerly (the patient lying on her left side) she used to sit, say a little higher than the middle of the bed, gently elevating the patient's right knee. Now, I required that she should sit on the pillow, on *a line with the patient's head*, and I usually found that I had to make great exertions to get her to keep the patient's right knee and leg out of my way, I myself going into the bed and following the course taken by the forceps. I found myself always crying "Up, up, still further," till the nurse sometimes lost patience, saying she could do so no more. This, of course, arose from the much greater curve which I was now making the handles of the forceps take. This, in point of fact, is so great, that I often, as has been stated, find the handles of the instrument, at the termination of delivery, almost parallel with the abdominal parietes of the patient. In November, 1861, I delivered with the forceps a lady (primipara) lying in a bed which stood in the midst of the room, and I noticed that I introduced my forceps while sitting on the right hand side of the bed, that as the delivery progressed I went round the foot of the bed, that when the delivery was completed I was standing at about the middle of the *left*-hand side of the bed, and that in the last stage of the delivery I had changed the position of my hands in grasping the handles, and was then *drawing* the instrument forwards and upwards. I did this, as I mentioned, with Ziegler's forceps; but had I been using such an instrument as I commenced practice with (fig. 3), it is manifest that the discovery of such an operation being possible never could have been made.

Let me now give a single case illustrative of these remarks, in which I used one of the new instruments made for me by Mr. Young.

December 30th, 1870.—Was called about 1 p.m. to Mrs. G—, who has a midwife with her, and states that she has been in labour since 7 a.m. Is of the squat figure I have referred to as having tedious or very difficult labours. States that she has had thirteen children (the first twins), of whom five were stillborn, three of these being delivered with the forceps. The head was at the brim of the pelvis, the uterus easily dilatable, and the os uteri about twice the size of a crown-piece. Pains regular, but rather feeble. On examination the pubes and promontory of the sacrum were found to project inwards, and between these and the head the uterus was caught. My first care was to get rid of this obstacle, which I succeded in doing, first, by using two fingers in pushing up the uterus in front; and secondly, as this failed, by introducing my hand and extricating the engaged part of the uterus posteriorly. After a few endeavours of this kind the uterus slipped over the head, the latter immediately descending a little into the pelvis. The head was still, however, so high up that it was with difficulty the tip of one ear could be felt a little to the right of the symphysis pubis. The face, I thought, was to the right (which I afterwards ascertained to be the case), but of this I was not at the time quite certain. After the head had come a little further down into the pelvis I tried to apply my new forceps, and, by directing the handle of the blade No. 1 [1] well backwards, I succeeded in placing it over the ear next the pubes. The other blade easily slipped into its place on the opposite side, and these gave me a remarkably firm hold of the head. I used simply traction at first, assisting each pain, until the perinæum began to be pressed upon, when I turned the instrument forwards and upwards (supporting the perinæum with my left hand), until the handles had revolved round the lower part of the pubes. When the head was born the instrument remained, as when applied, nearly in the mesial line of the woman's body. One blade was over the right ear and cheek, and the other over the opposite ear and parietal bone. There was a very slight excoriation over the left parietal bone; the anterior solid blade had merely compressed the skin and made it look slightly grayish. The umbilical cord was round the neck, and the child for some time failed to breathe. By at once, however, applying my mouth to that of the child and inflating the lungs, and compressing the sides of the chest in conjunction with this (as I have recommended and described in the 'Edin. Med. Jour.' for May, 1855), the child soon began to breathe and cry, and both it and the mother did well.

In my notes I find the following remarks on this case:—Here the length and strength of my new instrument gave me great advantage, for it must be remembered that in such labours strength is equivalent to length. I think my old instrument would have been constantly slipping until the head advanced, and hence losing most valuable time. And time here, as far as the child was concerned, was of immense importance, for, had the labour been prolonged even a very little more, the child must have been lost. From the time I saw this woman till her delivery something more than an hour elapsed. I should say that probably another half hour, and certainly another hour, in the passages would have killed the child. As it was, not an instant of time had to be lost in order to save it; and here I would

[1] I call the blade with closed fenestra No. 1.

remark that slapping the buttocks, dipping in cold or warm water, &c., would have been bad practice. In such extreme cases inflation, as the major remedy, should instantly be used, for then even seconds of time are precious. In delivery, the force applied was at first almost pure traction, and at last it became almost pure leverage. At first I tried to turn the head into the hollow of the sacrum; but finding this not to succeed, I let the instrument take its own course, the head passing the *os externum* with the blades nearly antero-posteriorly. Looking at it as a whole, I think we have here a key to the success which has attended my mode of practice, for it is easy to see how enormously a want of decision and knowledge must in these trying cases increase the mortality to both mother and child. Even though the ear was at the brim of the pelvis, and so high up that it could barely be touched, the simplicity of the whole operation, and the ease with which it was performed, seemed to me very remarkable. Once the anterior blade had been placed over the ear, I seemed to take almost no heed as to all the rest. The posterior blade at once took its proper place, and locked without giving me either trouble or thought (for this is the rare quality in Ziegler's forceps, that they lock of themselves); as the head advanced I did not know, and I did not care, what turn it would take, or whether it would take any, for my forceps were adapted to all contingencies; so, as it were, I almost let them take their own way, only watching the handles, which at once told me what was being done. If they should keep antero-posterior, then leverage was the power I had mostly to trust to; if they became transverse, the curve they could go through, or the lateral oscillation that could be employed, I knew was very limited, and traction then would mostly be the power available. With such simple rules as our guides, it seemed to me almost impossible for a person of the most ordinary competency to go wrong.

But if, from timidity or other cause, I had allowed the uterus to be caught at the brim for an hour or two more, and, as a consequence, the head to be arrested and the parts to become swollen, and had then introduced a curved instrument within the uterus, and had set out with a conviction that it must, and a determination that it should, describe a screw-like course in delivery, how very different might have been the results to mother and child. Looking at the difficulties of such cases, so managed, I think there can be little wonder that most writers on obstetrics have expressed a salutary dread of "long-forceps" deliveries, or that some should even have preferred to them opening the head.[1]

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[1] The following extract from Dr. Barnes's paper already referred to, containing instructions for the use of this instrument, is so suggestive that, though long, I think I shall be excused quoting it:

"*Introduction of the first blade.*

"*First stage.*—One or two fingers of the left hand are passed inside, in at the

The double curve appears to have been first given to the forceps by Levret, so as that it might fit into the shape of the sacrum, and, when the face has already got into the hollow of the sacrum, a short instrument of this kind suits very well. The curve has been retained on the longer instruments used since then on different grounds—as far as I can understand, because it gives a larger surface for laying hold of the head; because the head at the brim can be better reached with it; because, as Dr. Barnes says, it fits into the natural screw-like form of the pelvis. These and other reasons I have heard given for retaining the double curve, but I have never yet seen a case in which I would have preferred this to the straight instrument.[1] In this I think the Dublin school, where Dr. Beaty's straight forceps are mostly used, is right. Dr. Barnes says he formerly used straight forceps, but has latterly laid them aside, in consequence of finding that they injured the perinæum. On this point I can only repeat what I have already stated, that I have never had in my *whole* prac-

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perinæum and between the cervix uteri and the head. Then, bearing in mind the relative forms of the instrument, the head, and the pelvic canal, the point of the blade is passed along the palmar aspect of the fingers, at first nearly directly backwards towards the hollow of the sacrum.

" *Second stage.*—The handle is now raised so as to throw the point downwards upon the left side of the head. As the point of the blade must describe a double or compound curve—a segment of a helix—in order to travel round the head-globe, and at the same time to ascend forwards in the direction of Carus's curve, so as to reach the brim of the pelvis, the handle rises, goes backwards, and partly rotates on its axis.

" *Third stage.*—The handle is now carried backwards and downwards to complete the course of the point around the head-globe and into the left ilium. Slight pressure on the handle ought to suffice. This will suggest *movement* to the blade; the *right direction* will be given by the relation of the sacrum and head. The blade is now *in sitû;* the shank is to be pressed against the coccyx by the back of the operator's left hand whilst he is introducing the second blade.

"*Introduction of the second blade.*

" *First stage.*—Two fingers of the left hand, the back of which is supporting the first blade against the perinæum, are passed into the pelvis between the os uteri and the side of the head which lies nearest the right ilium. The instrument held in the right hand lies nearly parallel with the mother's left thigh, or crossing it with only a slight angle. The point of the blade is slipped along the palmar aspect of the fingers in the vagina, across the hauk of the first blade *in sitû*, inside the perinæum towards the hollow of the sacrum.

" *Second stage.*—As the point has to describe a helicine curve to get round the head-globe, and forwards in the direction of Carus's curve, the handle is now depressed and carried backwards until the blade lies in the right ilium. When it has reached this position the handle will be found near the coccyx, nearly in opposition to the first blade."—" Locking," &c. &c., 'Braithwaite's Ret.,' 1867, vol. lvi, p. 353.

[1] The fallacy and viciousness of Dr. Barnes's argument and illustration, that a screw must require another screw to fit into it, must be apparent when we notice the ease with which the straight forceps follow the natural rotations of the head when it does turn, as well as from observing their superiority, as to simplicity and safety, when extensive rotations are required. In the latter I would lay it down as a rule, from my own experience, that a double-curved forceps never should be used.

tice rupture of the perinæum; and that, when using the forceps, if I find any unpleasant distension of it, I take them off, and support the parts with the palm of my *right* hand (not the *left*, as recommended by some authors), pushing the head strongly forwards and somewhat upwards, as recommended by the late Professor Hamilton, or, in other words, "shelling it out," as this manœuvre has been appropriately named by Dr. Barnes. As far, therefore, as I can judge, I would say that the instrument I now use (figs. 1, 2) is fit for the management of any case I have ever met with, and certainly it can be used in many where the double-curved forceps are wholly inadmissible.

My 4th class consists of such cases as cannot be reduced to the 1st, 2nd, or 3rd classes, which may arise from a variety of causes. To some of these I shall now advert; and here I must repeat the opinion I have already expressed, that, with increased knowledge and experience, and the use of better instruments than we formerly had, we may reasonably hope the number of such will in future be considerably diminished. Let us take, for example, such a case as I have recorded in the 'Ed. Med. Jour.,' Oct., 1861, p. 320, where the ear was at the symphysis and the face to the left (my 5th position). In this case I could not get the face to rotate to the left, and was therefore obliged to push up the head, rotate the face to the right, and then deliver. If I met with such a case now I would, of course, at once endeavour to use the forceps as a lever in the manner described, and my impression is, that with my stronger instrument I should succeed. Supposing we fail, however, the principle of rotation, as I have pointed out in the article referred to, can be resorted to with great advantage, and to an extent (with straight forceps) that would astonish many practitioners. Sir James Simpson used to be much interested in this subject, and I recollect relating to him a still more interesting example of this kind. The case had been a long time under the care of an ignorant, and, what was even worse, an obstinate midwife, who persisted in the vain endeavour to make unaided nature effect the delivery. When I saw the woman I found the parts dreadfully swollen, and the case fast assuming all the characters of "impaction." With considerable exertion I found that the ear and face were to the left of the symphysis (my 4th position), and succeeded in fixing the forceps over this ear and the head, but could neither turn the face into the hollow of the sacrum nor make it advance downwards. I then, as in the last case referred to, pushed the head above the brim, and rotated it so as to place an ear and the face to the right of the symphysis (my 1st position), and still I could not deliver. I then resorted to podalic version, and by that means saved the child. The mother, however, succumbed to peritonitis[1] in

[1] Curiously enough, the husband of this patient again married a woman who has

three or four days afterwards. Besides showing the extent to which rotation can easily be carried, this case is one among many others confirming the doctrine that the base of the child's head, when brought down first, will sometimes pass easier through a narrow pelvis than if the vertex had presented. With my later ideas as to the mechanism of labour, and of the powers of the forceps, I would give my adhesion to this general principle with reservation; and I would say, as the result of my own experience, that podalic version should never be resorted to until the forceps (with rotation to the right, should the face be to the left) have failed.[1]

When I am absolutely compelled to apply the forceps within the uterus, the rule I follow is still, if I can, to place them over the ears, which mostly requires that the anterior blade (No. 1) should be thrown well back on the perinæum; or, if I cannot do this, or cannot exactly ascertain where the ear lies, I then lay hold of the head wherever I can (sometimes placing blade No. 1 posteriorly before introducing No. 2),[2] and advance it until, by examination, I find its

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always to be delivered with forceps, for though the pelvis is broad, the pubes project very much inwards. I therefore, contrary to my general practice, have to apply the instrument diagonally. The last time I delivered her I noticed there was so much room at the sides that when I placed the first blade at the right obturator opening it fell backwards, and had, therefore, to be held in position till the other blade was fixed. Usually, my first blade is held quite firm between pubes and head.

[1] I therefore could not agree with Sir James when, in the Obstetrical Society, he said ('Edin. Med. Journ.,' Oct., 1862, p. 378)—"When it became necessary to shorten the labour he believed that it did not matter very much whether the operation had recourse to were extraction by the forceps or version; that it was the speedy delivery of the patient from all her sufferings and struggles, and not the special kind of operation, that was her source of safety." This, however, relates merely to the safety of the mother, while it is the duty of the accoucheur, if possible, to save both mother and child. Dr. Figg, late of Bo'ness, Stirlingshire, who used to contend that all vertex presentations ought to be converted into footling ones, was present, and spoke on this occasion; and Sir James, in the above extract, seems to give countenance to the doctrine that in a case of tedious labour it is immaterial whether podalic version be resorted to or the forceps applied. I can only express on this point my very decided opinion, that an ordinary vertex presentation gives the easiest of all labours, and ought not, therefore, as a general rule, to be interfered with; and that when the labour has become tedious, any practitioner would be guilty of very bad practice indeed if he converted it into a footling case before he had exhausted the powers of the forceps. Sir James's obstetric practice seems at this time to have been in an unsettled or a transition state, for whereas formerly, at the Edinburgh Maternity Hospital, which was chiefly under his direction, the forceps deliveries had been 1 in 472, he (November, 1861) stated during this discussion that he then was using the forceps once in perhaps every 15 or 20 cases. He also stated that "he had never doubted that the chief danger from delay began when the first stage was completed, and an arrest of the labour took place when the fœtal head had begun to be compressed in the maternal canals."

[2] My reason for generally introducing blade No. 1 first anteriorly is, that after it has been placed in position the other usually at once slips into its place. A projecting promontory will sometimes, however, prevent this. The projecting pubes will sometimes also, as I have said, cause the blades to be diagonal. I very rarely

exact position, when I then know precisely how to proceed. At the same time that the forceps draw the head through the os uteri, I find it most useful, with the fingers of the left hand, to push the uterus up over the head. In these high operations the length and strength of my present instrument give me, I think, very great advantages.

Cases with the face to the pubes, occurring in primiparæ, have given me some trouble. My 732nd case, as I have stated, was of this kind, and I lost the child, and in an earlier part of my practice I recollect the same thing happening in another instance. It was interesting to me, therefore, since I have been using my new instrument, to meet with a case exactly similar. It was under the care of a midwife, who called me to assist her. I found the head jammed in the parts very tightly, and, as I could not at first ascertain with precision the position, I placed the blades antero-posteriorly, and used as much leverage as the case would allow. In doing so I suddenly felt the handles jerk round and become transverse. Taking the instrument off and examining, I now found that an ear was slightly to the left of the symphysis, with the face to that side also.[1] Reapplying the instrument I again used leverage, when the head made another jerk round, placing the face in the hollow of the sacrum, and delivery took place immediately. Here, again, I was much impressed with the great power my new instrument and mode of delivery gave me, and I was much pleased with the firm hold the india-rubber on the inside of the blades (which I had lately been using) seemed to give. The simplicity of the operation especially struck me, for it was nature, and not I, that took the turns. I found, on examination, that the solid blade (No. 1) had at first been place over the left eyelid and cheek, which were a little red and swollen, but not in the least degree excoriated. This disappeared in a few days, and mother and child did excellently.

After all, of course it will occur that in some instances the disproportion between head and pelvis is so great that delivery cannot be effected with the forceps, though I am now inclined to think, if the case has been well managed, and absolute deformity of pelvis or hydrocephalus be absent, this is much rarer than is generally imagined. In such cases, if the mother has not previously been too much exhausted, podalic version naturally presents itself to us as

indeed find myself placing them transverse at first if the head is high up. But at first, in such cases, I am content with any good hold.

[1] In the 'Edin. Med. Journ.,' for October, 1861, in mentioning my 732nd case, with face to pubes, I state that I found I could not move the face to the right, and was equally surprised to find that it comparatively easily took the turn to the left, in this differing from what usually occurs in vertex presentations. The present case, it will be observed, did the same, which, on consideration, seems to me the *natural* course, for the rectum prevents the *occiput* from revolving to the left, while the spaces on the right posteriorly and the left anteriorly are free. I think it is worth while keeping this, as a rule, in mind.

another resource before perforation. From the line of practice which I have followed, I have had so much less experience in podalic version than most hospital accoucheurs,[1] that I cannot venture to speak practically on the subject with great confidence. I would remark, however, that in such cases, where the head is very large and refuses after version to enter the pelvis, or where it is caught high up and does not advance after prolonged traction, it is perhaps the best practice, if the want of beat in the funis indicates the child's death, at once to use the perforator. In the case immediately preceding my series of 731 cases I did this, the child being almost a monstrosity in point of size. In another case, where I had to assist two brother practitioners, I did the same, the cervical vertebræ having given way before it was resorted to. In the first case the mother died also, from peritonitis, while, in another case still, I was called by two practitioners to assist them, and found the head alone in utero, and the mother moribund. Such examples, conjoined with the mortality to the children in cases under my own care, already referred to, have, I confess, given me a great dread of turning and high footling cases.

Even when the face has come into the hollow of the sacrum, in muscular primiparæ, the difficulty in delivery must, I think, often be so great that the pressure on the cord will kill the child. Here I have found the two dangers to be injury to the spine from traction, and death from compression of the cord. Although speaking only theoretically, I would hope that both of these may be in most instances overcome. The statement of Professor Busch, of Berlin,[2] that he had applied the forceps in such cases and saved most of the children, is certainly most encouraging for the use of this instrument, though I have never myself so used it. Dr. Meigs also advocates the use of the forceps in the same kind of cases. The statement, also, that the introduction of a catheter or tube into the child's mouth, or even expanding the parts with the fingers, has enabled it to breathe until delivery could be effected, would seem to point out that the joint admission of air to the child and traction *applied to the head*, are the desiderata required. It has struck me that this might be effected in two ways. On this same subject I said, in the 'Edin. Med. Jour.,' May, 1855, p. 404, that possibly a vectis, more curved than usual, might be found useful in such cases. I got one made and tried it, but the curve prevented its introduction.

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[1] In note in No. 96, page 202, we find in the 'Glasgow Maternity Report' 12 versions to 882 deliveries.

[2] "*Turning with the feet foremost* was performed in forty-four cases, with the highly favorable result that only three children lost their lives from the effects of the operation. Professor Busch ascribes this unusually small mortality among the children thus delivered to the circumstance of his instantly applying the forceps the moment the head experienced any hindrance when passing through the pelvis." —'Brit. and For. Med.-Chir. Rev.,' 1838, p. 579.

Latterly I have reverted to the same idea, lessening the curve, and getting a deep groove made in the instrument, so that the child could easily breathe by it, at the same time that traction was being

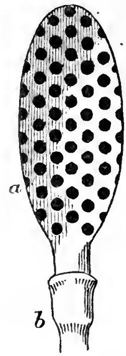

Fig. 6 (one fourth size). A kind of spoon, with double floor of perforated zinc (*a*), and india-rubber tube (*b*); to be introduced into the hollow of the sacrum, so as to enable the child to breathe while the forceps are being used in footling cases.

applied. The great objection to the vectis (properly, of course, a tractor), however, is its small power; and therefore I have also got a little instrument made (fig. 6), which perhaps may serve better than a catheter to allow of respiration while the forceps are being used. I also got made a hook to fit on the forefinger of the left hand when introduced into the child's mouth, so that traction and breathing might go on at the same time. The "spoon," fig. 6, and the forceps, however, seem likely to be the most useful. A vectis without fenestra, to give it increased surface, made upon the principle referred to, and smeared with india-rubber paste, however, may probably be found a tolerably efficient instrument, from the hold which it will have on the scalp. These suggestions, however, are theoretical, but they seem to me worthy of trial.

This paper has extended so much beyond the limits I originally assigned to it, that I have left myself almost no space to notice objections I have heard urged to the practice I have been advocating. This is the less necessary, however, first, because the good sense of almost all later practitioners seems to have set them aside as in a great measure baseless, Sir James Simpson setting the good example by telling us how he had altered his practice. Who now, of British accoucheurs, will venture to say that the forceps require to be used only once in 500 or 700 labours? or what general practitioner would think the use of them excessive even once in every twentieth or thirtieth labour? This alone shows how great has been the alteration of opinion on this subject during the last twenty years. Secondly, because all these objections I have heard of, I think, resolve themselves into the fallacy which I had formerly to expose. I then showed that it was not the forceps, but the delay and tedious labour, that killed the child; and so now, if any minor injuries are likely to accrue to mother or child, I say again, it is not the forceps, but the tedious labour, that should be blamed for these; and just, I believe, in so far as the forceps are promptly and judiciously used will they be found less frequent. I can most faithfully say, however, and without the slightest reservation, that I have observed in my own practice no drawbacks to their application of the very slightest consequence. This includes an alledged one to which my attention has only lately been directed by a friend, as he has found it stated by Drs. Barnes, Pettigrew, and Murphy, viz. that the use of the forceps encourages flooding. Now, after my long and constant practice with them, I should surely be able to speak pretty confidently on this point, and since the subject has been brought under my notice I have been trying to remember any of my forceps cases in which flooding had occurred (for most assuredly this never had attracted my special attention), and I can recollect only three that were severe. In the first the forceps were used, no doubt, but the flooding recurred, in the same patient, in five out of other seven labours, all of which were particularly easy. In the second and third chloroform was also used, and both patients had flooding more than once in their labours. In no patient whatever delivered by me with the forceps for the last twenty-five years have I failed (and generally with great ease) to restrain the flooding by the method I have recommended, viz.[1] introducing the one hand *under* the uterus, and compressing it between this and the other applied externally. In the instance formerly referred to, where the chloroform excited the patient, it was the chloroform alone that prevented the application of the necessary measures, and hence made the case formidable.

I must now take leave of this subject, hoping that the details my practice has furnished to me, and the observations it has enabled me to make, may be of use to my professional brethren. I must warn them, however, that it is not alone by a more frequent use of the forceps that success will be attained, if the other rules which have guided me be neglected. The first stage of labour improperly interfered with, by giving ergot or otherwise, may exhaust and kill the child, or the same may be done by allowing the second to be too much prolonged; so that a frequent use of the forceps and a high infantile mortality, although at first sight apparently contradictory of my practice, might not necessarily be so.[2]

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[1] See 'Edin. Med. Journ.,' October, 1850, and October, 1861, p. 315.

[2] In the note regarding the Glasgow Maternity, as given in No. 96, page 202, it will be noticed that among the 882 children born, version or the forceps were used 29 times, or about 1 in 30, and yet the stillborn children amounted to 91, or more than 1 in 10.

II.—On the Recent Progress of Uroscopy. By Dr. Karl Berthold Hofmann, Lecturer in the University of Vienna.

The design of the following paper is to present an account of the progress that has been made on uroscopy, especially in Germany. I shall therefore notice principally the works of the German physiologists, taking it for granted that most of the English works are known to our readers, while most of the German works are inaccessible to the English medical public.

On the formation and elimination of urea under different conditions.—By far the most important constituent of the urine is the urea; its synthesis on the one hand, and the explanation of its derivation through the retrogressive metamorphosis of the tissues on the other hand, have for a long time occupied the attention of the chemist and physiologist.

During the past year in this direction two series of researches have been made. Basaroff succeeded in forming urea from ammonia (NH_4O), and carbonic acid (CO_2); and Béchamps, for the third time, asserts the possibility of obtaining urea by the oxidation of albumen.

In addition to the old method of artificial production of this body from the cyanate of ammonia, Nathansen has discovered two others which support the hypothesis that urea is to be considered as the amide of carbonic acid. He obtained urea by treating dry ammoniacal gas with phosgene ($COCl_2$), or by heating carbonic ether with ammonia in excess.

The first process is $COCl_2 + 2NH_3 = CON_2H_4 + 2HCl$. The second, $CO_3(C_2H_5)_2 + 2NH_3 = CON_2H_4 + 2C_2H_2O$.[1]

In intimate connection with those two methods stands the following method of Basaroff.[2] He passes pure dry ammoniacal gas and carbonic acid through absolute alcohol. By this is formed carbonate of ammonia (formula: $CO_2 + 2NH_3 = (NH_2)CO_2NH_4$). This formula, compared with that of urea (CON_2H_4), contains one molecule of water (H_2O) more, to remove which he heats this combination in an hermetically sealed tube at the temperature of 130°—150° centigrade.

Daily experience teaches us that when the urine is in process of putrefaction the carbonate of ammonia which gives the urine its disagreeable odour is got from the reception of two molecules of water by one molecule of urea. Basaroff, by inverting this process,

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[1] In the formulæ given above the equivalents of C and O are taken twice as large as in less recent chemical works.

[2] A. Basaroff, "Directe Darstellung des Harnstoffs aus Kohlensäure und Amoniak," 'Journal für Pract. Chemie,' N. F., Bd. i, p. 283.

succeeded in splitting up carbonate of ammonia into urea and carbonic acid, by heating the same in an hermetically sealed glass tube at a temperature of 130° centigrade.

Less fortunate was Béchamps.[1] He proposed the following method:—Ten grammes of albumen (free from fat and sugar) are mixed with sixty to seventy-five grammes of permanganate of potash and one hundred to three hundred c.c. water; this is heated in a water bath until the blue colour disappears; then the fluid is filtered, and acetate of lead added (excess of the latter is to be avoided). Having filtered again, the filtrate is to be treated with sulphuret of hydrogen. The precipitate of sulphuret of lead is to be filtered, and to the clear filtrate caustic baryta and subnitrate of mercury added as long as a precipitate is formed. The precipitate must be separated, washed, and afterwards decomposed by the addition of sulphuret of hydrogen. The acid fluid is filtered once more, and then neutralized with carbonate of baryta and evaporated. Béchamps states that he found in this residuum crystals of urea.

I am not able to confirm this statement of the French experimenter. Although I have closely followed his directions, I have not succeeded in obtaining a single grain of urea. My own experiments have been made in Prof. Fr. Schneider's laboratory. Some time afterwards Löwe, of New York, proved the crystals which Béchamps took for nitrate of urea to be only the crystals of nitrate of baryta. These two negative results apparently prove that Béchamps' latest assertion is just as incorrect as his two former ones. The superficiality of his work is proved by the mere fact that he did not even deem it worth the trouble to subject his supposed urea to the elementary analysis.

The generally conjectured derivation of urea from the albuminates is therefore not yet proved.

Next to the formation of the urea physiologists are most interested in the *place* of its formation. Is the urea first made in the blood, and have the kidneys only to eliminate it? Or are the kidneys the place where it takes its origin? To solve this contested question N. Gréhant[2] repeated in C. Bernard's laboratory the following decisive experiments. He extirpated both kidneys of a dog, and found, after this operation, that the blood contained more urea than before. The same results followed the placing of ligatures on the ureters. Both experiments proved the accumulation of urea in the blood.

If the blood of the renal vein and artery of a perfectly healthy

[1] M. A. Béchamps, "Sur la formation de l'urée par l'action de l'hypermanganate de potasse sur les matières albuminoides," 'Comptes rend.,' lxx, 866-69.

[2] N. Gréhant, "Recherches physiologiques sur l'excrétion de l'urée par les reins," 'Bibliotheque de l'école des hautes études,' Sect. des sciences natur., i, 265.

kidney be examined, it will be found that the venous blood contains less urea than the arterial, which shows us that some urea is lost during the passage of the blood through the kidneys. If the two ureters are tied so that no urea can be eliminated, the blood in the veins and arteries contains the same per-centage of urea. This is an indisputable proof that the urea is not formed in the kidneys, otherwise the venous blood would contain more urea than the arterial. The great mass of urea in the blood after the extirpation of the kidneys proves the same, for if this were not true the production of urea would cease after the kidneys have been extirpated. To determine the quantity of urea Gréhant used Millon's fluid.

Induced by the observations of Gréhant, Cyon attempted to prove that urea is produced in the liver. To this end blood was taken from the carotid of a dog which had not been narcotized, quickly defibrinized, and a part of it placed in an apparatus by means of which it was to be forced through the liver. These tubes were introduced respectively into the *vena cava inferior, arteria hepatica,* and *vena portæ,* then the liver cut out of the body and placed in a vessel heated to the temperature of the blood. The first tube was connected with an aspirator, the second and third with the above-mentioned apparatus, in which the blood was contained. The circulation of the blood through the liver could be kept up by means of a connecting tube, which returned the blood on its arrival in the aspirator into the first vessel, from which it could be again forced through the liver. After the blood had been several times passed through the liver it showed a greater per-centage of urea than a like quantity of the active blood of the same animal. Cyon draws, therefore, the conclusion that the liver is the place of formation of the urea. He promises, at the same time, to follow these short communications with a more elaborate article. (Hr. E. Cyon, "Ueber Harnstoffbildung in der Leber," 'Centralblatt für die medicinischen Wissenschaften,' 1870, No. 37.) p. 580.

Dr. Perls,[1] lecturer at Königsberg, proposed a new method for determining the quantity of urea in the blood. First prepare a watery solution of sulphate of copper of the concentration one to five. Then the blood which is to be analysed is to be diluted with an equal quantity of water. For every twenty c.c. of this diluted blood, which contains ten c.c. pure blood, there is to be used one c.c. of the sulphate of copper. The result of this reaction will be a yellowish-green fluid, which, after standing a few hours, forms a precipitate. This fluid is to be filtered, and the filtrate evaporated; then an alcoholic extract to be made from the residuum, and this extract treated with nitric acid, whereby nitrate of urea is formed.

[1] "Neue Methode zur Bestimmung des Harnstoffes im Blute und in den Geweben," 'Med. Centralb.,' 1870, p. 49.

Meissner asserts that by this method he found, in ten to twenty c.c. blood of rabbits, whose kidneys had been extirpated, four centigrammes of nitrate of urea. This also proves the accumulation of urea in the blood after nephrotomy.

Elimination of urea in starvation and in fever.—As the elimination of urea affords the best, indeed the only means, of explaining the atomic changes in the human body, so the attention of observers has been called to the investigation of its quantity under different physiological and pathological conditions. The chief aim of these observers has been to make themselves better acquainted, by means of the examination of the urine and its constituents (especially by the quantitative determination of the urea), with the atomic changes which accompany starvation and fever.

A new investigation of Dr. Nicholson's, "On the Body-weight and Urea in a Case of Starvation," published in the 'Brit Med. Journ.,' 1870, No. 470, confirms the previous experience, that with abstinence the elimination of urea does not cease, and that the accompanying diminution of body-weight is in proportion to the same.

The observations of the urine in fever, and especially of the quantity of its nitrogenous constituents, have been made partly on patients with febrile diseases and partly on dogs.

Naunyn[1] determined the quantity of urea eliminated by a starving dog. The quantity was 0·28 gramme per hour. If in addition he produced fever by the injection of ichor, the dog excreted 0·42 grammes in the same time. Another dog, during the starving period, excreted 0·26 gramme per hour, during the fever 0·48 gramme. The increase in the total quantity of the urine during fever (from 180 c.c. to 280 c.c.) leads Naunyn to believe that in this condition not only the nitrogenous constituents of the organism, but also the non-nitrogenous, are subject to a greater oxidation than in health.

Naunyn wished to become certain whether or no the increase of urea during the febrile state is the effect of the raised temperature of the body. To decide this point he shut a dog in an Obernier's chest, the air of which was perfectly saturated with steam of the temperature of 35°—40° C. During the six hours the dog remained under the influence of this heat the temperature of the dog rose to 42·5 C., and he excreted 110 c.c. urine, containing 9·716 grammes urea. Outside of the chest, in the same time, with the same food, the dog passed 120 c.c., containing 6·3—7·5 grammes urea. This seems to indicate that it is the raised temperature which causes the increased production of urea.

Unruh's investigations on fever patients would go to disprove

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[1] Naunyn, "Ueber das Verhalten des Harnstoffausscheidung beim Fieber," 'Berliner Clinische Wochenschrift,' 1869, No. 4.

this hypothesis; on the other hand, Schröder's observations on typhus patients confirm it.

Unruh[1] makes the comparison between the urine of patients with fever and those without, the same food having been given to both. The patients without fever, one of whom had a cancer of the stomach, the other syphilis, excreted, on an average, 17·466 grammes urea; those with fever (relapsing fever, pneumonia, typhus fever, acute synovitis, rheumatismus articulorum febrilis, trichinosis, erysipelas, typhoid fever, wound fever) eliminated more urea. The increase reached 3·06 — 3·07 times the average quantity which those without fever eliminated. These results agree with those obtained by former observations. The quantity of uric acid and of kreatinine was also increased, the increase of uric acid being 1·6 times the normal quantity, and that of kreatinine nearly double. This result confirms my experiments on kreatinine.[2] The quantity of urine, with few exceptions, was decreased. Unruh thinks that the retention of water in the system is the cause of the diminution of urine. Senator and Naunyn consider the decrease to be only apparent; for fever patients taking less food, less water is introduced into the system, and, therefore, according to the opinion of the last two investigators, there cannot be so much urine eliminated. This last objection is worthless, for Naunyn explicitly states that the patients had the same food as those without fever. According to my own observations, thare are many cases where the patients, tormented with excessive thirst, drink a much greater quantity of water than the food and beverage together amount to which they consume in a healthy state, and hardly excrete half so much urine as they are accustomed to in a healthy condition. In these cases it may be that the excess excreted through the lungs and skin, rather than a retention of water in the syetem, is the cause of the decreased excretion of urine. This explanation is confirmed by the everyday experience that in a state of health the quantity of urine diminishes if a person perspire excessively, because the skin acts vicariously for the kidneys.

Unruh's investigation of the connection between the increase of urea and the height of the body-temperature does not perfectly agree with the observations of others; he found, in the beginning of a very high fever, that the quantity of urea is not as great as in the following days, though the temperature does actually increase. In the stadium decrementi, in spite of a lower temperature, a greater quantity of urea is sometimes eliminated.

In diseases with light fever Unruh found more urea than in severe fever, *e.g.* in rheumatism more than in pneumonia.

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[1] Unruh, 'Virchow's Archiv,' xlviii, 227 and f.

[2] Hofmann, "Ueber Kreatinin," 'Virchow's Arch.,' xlviii, p. 390.

Unruh explains the increased elimination of urea which continues during the epicritical stadium of the fever, partly by the rapid disintegration of the fever, partly by the accumulation in the organism during the stadium incrementi of products which are only entirely oxidized at the crisis of the fever. The observations of Naunyn (at least in some cases) go to disprove the hypothesis that the increased diminution of urea is consequent on the greater height of the temperature of the body.

According to Unruh's observations, the proportion between urea and uric acid remained the same in the febrile and in the normal state of the organism.

Chinine seems to have neither a constant influence on the elimination of urea, as the urea was now increased, now diminished; nor, in opposition to the former opinions of Ranke, can any constant influence on the uric acid be said to have been observed.

L. Schröder's[1] observations on patients with typhus fever seem (as already said) to confirm the theory that the increase in the urea *is* dependent on the increase of temperature. He examined the urine of two typhus patients, that of the one during eight days, of the other during nine days, the food given being every day of the same quality and quantity. Both patients were treated on some days with cold baths and on others without; one of the patients eliminated on the days when he received the bath, on the average, 33·9 grammes urea, on the days when he was not bathed 41·7 grammes; the other, in the first case 19·9 grammes, in the second case 29·6 grammes urea. The decrease of urea on the days with bath is, therefore, like the decrease in the temperature, very considerable.

According to a short communicaton in the 'Centralblatt für die medicinischen Wissenschaften,' 1870, p. 194, Professor Leyden found the increase of urea in fever = 1 : 1·45.

O. Schultzen's[2] observations in cases of febris recurrens confirm the results obtained by Unruh and those previously obtained by Riesenfeld and Huppert, *i. e.* that the increased elimination of urea continues some time after the temperature has begun to fall. This observer attempts to explain the continuation of the increased elimination of urea in a manner differing from that of his predecessors. He rejects the supposition that substances could be retained in the body, which, being afterwards oxidized, could increase the quantity of urea eliminated in the epicritical period, and affirmed that no trace of any such substance is to be found in the urine, and that such substances would have to be present in very great quantities. He holds that this process has its analogon in the healthy

[1] "Ueber Harnstoffausscheidung," 'Deutsches Archiv f. Klin. Medizin,' vi, 4.

[2] O. Schultzen, "Ueber d. Stickstoffumsatz bei Febris recurrens," 'Annalen d. Charité-Krankenh.,' xv, 153 and ff.

organism where, after a long-continued consumption of albumen, the increased elimination of urea continues, even after the quantity of albumen in the diet has been greatly diminished, until the body reaches a new state of equilibrium. The difference between these two cases is this, that the albumen of the circulation (Circulations-Eiweiss) which is to undergo the process of oxidation is derived, in fever, from the increased quantity of albumen of the tissues (Organeiweiss) in process of disintegration, in health from the increased quantity of the albumen of the food (Nahrung-Eiweiss). This increase in the urea eliminated (the chlorides being at the same time diminished) is also confirmed, in seven cases of febris recurrens, by Meymott Tidy.[1]

As can easily be seen from what has been brought forward, the point in discussion remained open, *i. e.* whether a causal connection exists between the raised temperature of the body in fever and the increased elimination of the nitrogenous products of disintegration; and if this is the case, whether the increased temperature is the cause of the increased disintegration of the tissues or *vice versâ.* M. Naunyn[2] undertook to answer this question by means of experiment.

It cannot be doubted, at least, the great majority of observers confirm the theory, that fever is accompanied by an increased elimination of nitrogenous substances. It is probable also that the nitrogeunus substances are subjected to an increased oxidation and disintegration. The above-mentioned experiment on the dog enclosed in the steam-chest of Obernier confirms Bartel's experience on men who have remained in a steam bath from ten minutes to an hour and a half. An increased elimination of urea was observed, which probably resulted from a retention of heat and a consequent oxidation. As a result of these observations it can reasonably be supposed that the raised temperature was the antecedent and the cause of the increased metamorphosis of the tissues. He (Naunyn) repeated the subcutaneous injection of ichor on animals. He reasoned thus: if the retention of heat be the cause of the more rapid disintegration, no increased elimination of urea should take place during the period between the injection and the beginning of the noticeable rise in temperature, the (so-called) period of the latent fever. But numerous observations taught him just the contrary—that already in the period of the latent fever, when no rise in temperature could be observed, the increase in the production of urea had already begun, being a proof that this was not consequent on the increased temperature but on that enigmatical process which

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[1] "On the Urine in Relapsing Fever," 'Lancet,' 1870, i, No. xxvi.

[2] "Beitrage zur Fieberlehre," 'Reichert's und Du Bois-Reymond's Arch.,' 1870, Heft ii, p. 159.

we call fever, and this increase did not keep in the same proportion to the increased temperature.

This seems to be in harmony with the above-mentioned observations of Unruh on man. Naunyn supposed the increased elimination of urea to be a result of its accumulation during the crisis and subsequent less rapid evacuation after the crisis.

I will here just mention that Burral,[1] like Fuller,[2] found an increase in the urea in nervous diseases accompanied by hypochondria. It is a pity that Burral made his conclusions from the mere formation of a precipitate consequent on the addition of nitric acid, which precipitate could easily have been confounded with one composed only of urates, or, if this confusion did not take place, may only prove the concentration of the urine.

On elimination of uric acid.—B. Naunyn[3] and L. Riess made experiments on the elimination of the uric acid, the most important constituent of the urine after the urea. They found on dogs fed with flesh the same proportion between urea and uric acid which Ranke states as existing in man, the similarity in these two cases ceasing, however, to exist when the food of the dogs consisted chiefly of non-nitrogenous substances. These two observers found, when the food contained proteine in only small quantities, no uric acid (and also no kynurenic acid), while Ranke and myself saw under the same conditions in the urine of man the uric acid decrease in quantity, but not entirely disappear. In a state of complete starvation we are told that the presence of uric acid was again observed in the urine, while we found a continued diminution. The two observers attempted to advance a proof that the increase in the elimination of the uric acid was consequent on the decreased oxidizing power of the blood.

Induced by the supposition, which does not agree with my analysis, of which mention will be made below, that in leucocythæmia the decrease of the red blood-corpuscles is the cause of the diminished oxidating power of the blood, and, therefore, of the increase of uric acid, they tried to bring dogs under similar conditions. They made the animals anæmic by means of venesection. Although by these means the oxidizing power of the blood must have been lessened, still they could not find any sensible increase in the quantity of uric acid eliminated. They performed another series of experiments for the determination of the quantity of uric acid in diabetes.

Everybody has experienced that, in analysing diabetic urine, it is often impossible to obtain uric acid by treatment with hydrochloric acid. Naunyn and Riess showed us a new method, which gives,

[1] F. A. Burral, 'Amer. Journ. of the Med. Soc.,' July, 53.

[2] 'Medico-Chirurgical Transactions' for 1858.

[3] "Ueber Harnsaureausscheidung," 'Reichert's und Du Bois-Reymond's Arch.,' 1869

even in such cases, satisfactory results. The uric acid is to be precipitated with acetate of mercury, the precipitate to be filtered, then suspended in water, and then to be decomposed by the action of sulphuretted hydrogen. The sulphuret of mercury formed in this manner is to be filtered. The uric acid contained in the filtrate can now be determined. They found by this method the proportion between uric acid and urea in diabetes = 1·99. After the administration of large doses of opium the quantity of uric acid slightly decreased in proportion to the urca.

On elimination of urea and uric acid as influenced by muscular exertion.—In the 'New York Medical Journal' of October a short article was published on the influence of muscular exertion upon the excretion of urea and uric acid, by Dr. Flint, but was in no respect trustworthy. Mr. Weston is said to have walked one hundred miles in less than twenty-two hours. Given that this is true, which is open to strong suspicion, the figures for urea and uric acid are, nevertheless, of doubtful value. During the period of comparative rest we find the quantity of urea discharged 191·4 grains. This quantity is evidently erroneous for such a brawny man as Mr. Weston must be, judging by the pretended unequalled muscular exertion. I obtained from subjects of mean strength the average quantity of 540 grains from subjects of weak, delicate constitution, the average quantity of 460 grains. As to the figures obtained in the period of the great exertion, the quantity of 463·368 grains, compared with the nitrogenous food (consisting of sixteen eggs, one to two bottles of beef essence, and two bottles of oatmeal gruel), is not to be esteemed an increase of urea, being not higher than the average quantity found by me in weak persons living on ordinary diet.

A comparison cannot be made between the first and second figures, unless we suppose that Mr. Weston is as great an abnormity in the voiding of urine as in walking.

The quantity of uric acid during the period of exertion recorded by Dr. Flint (forty grains) deserves no earnest criticism. Researches like this are not fit to assist in bringing this delicate question to a definite solution.

Urine in acute atrophy of liver and in intoxication with phosphorus.—A number of analyses of the urine have been made by O. Schultzen, L. Riess, and A. Valenta,[1] in another interesting group of diseases, namely, on the molecular changes in acute poisoning with phosphorus, and in acute atrophy of the liver. Schultzen and Riess[2] observed ten cases of poisoning with phosphorus and fever of

[1] "Ueber Acute Phosphorvergiftung und Acute Leberatrophie," 'Annalen d. Charité Krankenhauses,' 1869, 183 a. f.

[2] 'Wiener Med. Jahrb.,' xviii.

acute atrophy of the liver in Professor Frerich's Klinik. The characteristics of the urine in the cases of poisoning were as follows:—The urea was greatly diminished, especially as death approached. The place was taken by other nitrogenous substances, the composition of which is not perfectly known, and which, as they are also precipitated by nitrate of mercury, can be easily mistaken for the urea. If the patient begins to recover, the urea returns, and these abnormal substances vanish.

The uric acid does not disappear, even in the most severe cases of poisoning. The urates precipitated by alcohol are said to give crystals similar to those of tyrosine. In one case, ending with recovery, 2·326 grammes uric acid were eliminated (in twenty-four hours) in 3000 c.c. urine (sp. gr. 1021)—a very considerable quantity. In the same urine three grammes kreatinine were found. I pass by the abnormal substances whose nature is not perfectly known, referring the reader to the original.

Those of the known substances which were present were bile-pigment and acids of the bile. Albumen was not constant, and when it appeared was always present in only small quantities. Sugar was never present. The result of these analyses seems to indicate that the protein substances are split up into nitrogenous and non-nitrogenous combinations, but that the combustion is not carried far enough to produce the final results of a normal oxidation in the healthy organism. The crystalloid substances resulting from this decomposition—for instance, the lactic acid, the peptone-like matters, and the uric acid—are eliminated with the urine; the colloid substance—for instance, the fats—cannot pass through the kidneys, and collect in the organism, inducing thus its fatty degeneration. The phosphorus resembles in this respect the ferments, that even small quantities of it produce this derangement of the organism, and it possesses also the power of transforming great quantities of oxygen into ozone. Schultzen and Riess believe that the phosphorus so changes the organs in which the oxidation takes place that they lose their power, and that thus the oxidation is not completed.

The urine in acute atrophy of the liver differs from the above-mentioned only in containing large quantities of leucine and tyrosin, which, in the poisoning with phosphorus, either fail entirely or are present only in small quantities, and that the albumen appears as a constant constituent of the urine. It contains, moreover, all the substances which we have mentioned as occurring in the urine after the poisoning—the peptone-like substances and an organic acid, $C_8H_8O_4$, which the authors call oxymandelsäure. The characteristics of the urine in both cases (acute poisoning with phosphorus and liver atrophy) seem to indicate that they do not essentially differ, but that the poisoning with phosphorus takes a too rapid course to show us the same final stage which we observe in the acute atrophy of the liver. Dr. Ossikowsky, who, in chronic poisoning with phosphorus, found leucine and tyrosin, supposes also, in a lately published paper, an identity between the two diseases.

Urine in Tetanus.—Senator[1] had the opportunity of examining the urine in two cases of tetanus traumaticus. Passing over details, the result of his analysis was as follows:—The quantity of the urine was much diminished (in the first case 500—600 c.c., in the second 485—870 c.c.); likewise the urea (on an average 1 grain to 3 grains, and 21·8 grains). This proves the assertion of the physiologists that the increased action of the muscles does not increase the quantity of urea eliminated to be true.

It was then observed that, on the days when the convulsions were most violent and numerous, consequently on the days of the most violent muscular action, that the quantity of urea eliminated was not the greatest. The small quantity eliminated is so much the more remarkable inasmuch as the convulsions were accompanied by a high fever temperature (39·4°—41·0° and 37·0°—38·2°). Creatinine as well as uric acid was present in small quantities. The result of this analysis accords with the observations of Nawrocky and myself that the creatinine is not increased after violent muscular exertion, and therefore is only partly derived from the disintegration of the tissues, and partly from the food.

On Chyluria.—Hr. Eggel[2] had the opportunity of observing a case of chyluria, a disease seldom to be studied in Europe, and which is alike enigmatical in its nature and in its ætiology. The case was one of a woman, fifty-seven of age, from Rio Grande, in Brazil, who had left her native country thirteen years before. We take from the amanuensis that her health had always been good, with the exception of headaches, which appeared alternately in the region of the occiput, the temples, and the top of the head, and attacks of dizziness, having no connection in time with the headaches (accompanied at the same time by constipation and colics). She bore three children; the birth and period of confinement were in all cases normal. The disease first attracted her notice eight years ago. Six months after a violent concussion, received in a collision of railway trains, she observed for the first time that her urine was milky. This first attack lasted uninterruptedly during eight months. One year later it was followed by a second, lasting, with interruptions, for some months. Two years later by a third, which continued for two years, with free spaces of weeks in length; and finally, fifteen months later, by a fourth attack, which Eggel observed. The urine was in quantity normal; specific gravity 1·10—1·14, slightly acid or alkaline;

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[1] "Ueber die Beschaffenheit des Harnes in Tetanus," 'Virch. Arch.,' xlviii, 295.

[2] 'Deutsches Archiv f. Clin. Med.,' vi, 421.

decomposing easily, with a weak, stale odour of ammonium sulphhydrate, the existence of which could not be chemically proved. It was white, with a reddish tinge; milky, forming a one mm. thick cream-like surface layer, and an equally thick sediment. The normal constituents (unfortunately only given in percentages) were urea, 2·10—2·20; uric acid, 0·03; salt, 0·35 per cent. The abnormal constituents were albumen, 0·32—0·627 per cent.; fat, 0·687—0·20 per cent. Some cholestearin, lecitine, and traces of a fibrinoplastic substance, but no sugar. The urine contained no coagula. The sediment consisted of pus and blood-corpuscles, and darkly lined bodies of a high refracting power, and enclosing a few glistening molecules, which bodies were smaller than large white blood-corpuscles. Neither epithelium, casts, nor milk-globules were observed. The opacity of this urine was caused by the finest molecules. Beside this constitution of the urine, only backache, great weakness (at the beginning of the disease) and dysuria, were observed.

The author, whose paper is remarkable for the careful use he makes of former cases, calls our attention to the fact that his case also was a chronic one, and that the disease was not developed until five years after the patient had left her native country, where chyluria is endemic, and also that the patient was a lady of a nervous temperament. The conclusions arrived at by the author were as follows:—The abnormal constitution of the urine cannot be simulated on account of the exceedingly fine division and equal distribution of the molecules, and has, as has often been supposed, nothing whatever to do with the secretion of the breasts. The fat in the chylous urine is not derived from the molecular changes in the tissues, but from incomplete assimilation of the food, especially of the fat contained in it, because in the tropics the high temperature of the atmosphere reduces the assimilating power.

The chyluria is caused by the atrophy of the capillaries in the kidneys, not by a disease of the kidney parenchyma, nor rupture of the lymphatics, the lymph containing less fat than the urine, and the urine but few lymph-corpuscles.

On Leucocythemia.—Analyses of the urine have been made by Reichart, Salkowsky, and myself, to obtain a more thorough knowledge of the molecular changes in leucæmia.

Reichart[1] analysed the blood and the urine of a forty-three year old man, who, formerly addicted to drinking, had suffered for three years from dyspnœa, and finally, on account of ascites, weakness, œdema of the lower extremities, and diarrhœa, came into the hands of the physician. The spleen extended twelve centimetres over the

[1] 'Jenaische Zeitschr. Medic.,' v, 389.

median line towards the right side; its greatest breadth = twenty-four cent. The proportion between the white and red blood-corpuscles (1—2 and 1—3) indicated a high degree of leucæmia. In an average quantity of urine (1100 c.c.) in twenty-four hours, he found 28·30 grains urea, and 0·71 grains uric acid. It seems that the urea was diminished, though neither the quantity nor the quality of the food is mentioned. The uric acid was increased absolutely as well as relatively to the amount of urea (uric acid, urea = 1·31—70, while in normal urine the proportion stands 1·60—80). The mean quantity of urea equalled 30—40, and of uric acid 0·50 grains. This increase of uric acid, confirmed by Virchow and all other observers, seems to indicate that connection which Ranke was the first to mention between the state of the spleen and the excretion of uric acid. Neither albumen nor hypoxanthine could be detected.

In the case under my examination[1] (the patient being a man of thirty years of age) a series of analyses gave the following results. The quantity of colouring matter of the urine was increased, indicating neither a greater disintegration of the red blood-corpuscles nor the production of an abnormal pigment of greater colouring power derived from hæmatine. The whole quantity of phosphoric acid was greatly diminished (66·66 per cent.), this diminution being greater on the side of the alkaline phosphates than on the side of the earthy phosphates. The quantity of chlorine, sulphuric acid, and creatinine was normal. The urea also was in this case not diminished, in contradiction to a greater number of cases where the diminution was great. The uric acid was absolutely, relatively, and, in proportion to the urea, increased. Ranke's supposition that the decrease of the urea is not dependent on the increase of the uric acid appears to be wholly confirmed by this case. The increase of uric acid was not in consequence of a febrile state of the organism. The normal quantity of urea conflicts with the supposition that decreased oxydation is the cause of an incomplete combustion of uric acid, and therefore of its increased elimination. No hypoxanthine could be detected, but slight quantities of lactic acid and albumen were present.

Salkowsky[2] made a careful analysis of the urine in a third case of leucæmia. A series of observation during thirty days showed that here, as in other cases, the uric acid was considerably increased. These analyses are followed by an excellent criticism of Mosler's method for the detection of hypoxanthine; he shows the fallacy in this old way of examination, and advances at the same time a more accurate method as follows. The urine is made strongly alkaline,

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[1] 'Wiener Med. Wochenschrift,' 1870, No. 42.
[2] 'Virchow's Archiv,' 1870, l, 174.

and after some hours the precipitated phosphates filtered off. To the filtrate is added an ammoniacal solution of nitrate of silver as long as any precipitate is formed. This greyish-white precipitate is collected in a filter, washed till all the chlorine has been removed; then suspended in water and decomposed with sulphuretted hydrogen; then heated to a boiling point, filtered while hot, and the filtrate evaporated to dryness in a water bath. The residuum consists of uric acid, xanthine, and hypoxanthine. To separate the uric acid from the xanthine and hypoxanthine the residuum is dissolved under the action of heat with diluted sulphuric acid (1·30). The hot liquid is filtered off, and ammonia added in excess. After cooling, the liquid is again filtered, and a precipitate formed by the addition of an ammoniacal solution of nitrate of silver. The precipitate, treated according to the method of Neubauer, gives the compound *nitrate of hypoxanthine and silver*.

Salkowsky gives us also the method of distinguishing hypoxanthine from xanthine and xanthine-like substances (on which point reference can be made to the original). By this more accurate method it was impossible to detect any trace of hypoxanthine in the urine in this case of leucæmia. Lactic, formic, and acetic acid as well as oxalic acid were found, but as the same are normal constituents of the spleen, their appearance, as well as the increase of zinc acid, may be caused by the hypertrophy of this organ. We thus do away with the necessity of supposing a diminished oxidising power of the blood.

On Albuminuria.—The practical importance which the presence of even small quantities of albumen in the urine has for physicians, makes it desirable that we should have an exact method for its detection. Such a method, resembling that of Mehn, has been given us by Meymott Tidy.[1] Mehn precipitates the albumen with a mixture consisting of one part carbolic acid, one part acetic acid, and two parts alcohol of 86 per cent. The carbolic acid alone makes the albumen insoluble. The acetic acid prevents the precipitation of other salts, if such should be present. Tidy's method differs from this only, in that he first adds one cubic centimetre alcohol to a small sample of urine, and then adds a solution of like parts of carbolic acid and acetic acid. The accuracy of this method surpasses that of the method with nitric acid. While the nitric acid method detects the presence of one part albumen in 8000 parts water, this latter method detects one part albumen in 15,000 water.

E. Hefsen[2] gives us his observations on the nature of the albuminates in the urine. In thirty-one cases of albuminuria he seems to have found with certainty globuline. If the urine, namely, be strongly

[1] 'Lancet,' 1870, May, p. 691.
[2] 'Virchow's Archives,' vol. lix, p. 437.

diluted with water, it becomes opaque, which opacity is increased by the influence of carbonic acid, but which disappears on the addition of some drops of hydrochloric or acetic acid, of some drops of ammonia, or of a concentrated solution of common salt. He could neither detect "kalialbuminat" nor "paralbumin."

Hegar and Kaltenbach[1] observed the appearance of albumen in the urine after a prolonged narcosis from chloroform. The albumen was detected by addition of nitric acid and heating; besides this epithelial casts were found. In thirteen cases albumen was found in six, and failed in the other seven. In several cases of chloroform narcosis I did not succeed in proving the presence of albumen. The patients were all men, while the positive observations of these two observers were for the most part on pregnant women. As the number of my observations was but small, it seems desirable to carry the observation on this interesting point further.

M. G. Calmettes[2] made experiments on the appearance of albumen in the urine after the injection of different albuminates. After the injection of a large quantity of a solution of casein or milk (20—26 gr.) into the veins of a rabbit, he found that several times the urine turned opaque from the action of heat alone; in other cases this did not take place until nitric or acetic acid was added. The milky appearance in the first case he attributed to the presence of albumen, that in the second to casein. The injection of white of egg and gelatine seems to me to give unreliable results. When white of egg was injected it sometimes itself appeared again in the urine and sometimes albumen of the serum.

On the determination of Sugar.—On glycosuria (*diabetes*).—The property which Fehling's solution has of being easily decomposed by the influence of light and fermentation, induced several observers to seek other methods of determining sugar.

K. Knapp,[1] induced by Liebig, advances a method of determining the sugar by means of cyanide of mercury. Ten grammes of pure cyanide of mercury are dissolved in water, ten c.c. of a solution of caustic soda added of the sp. gr. 1·145, and this liquid diluted to a litre.

The determination is made in a way similar to that in Fehling's method. Forty c.c. of the above-mentioned solution of quicksilver are put in a porcelain basin and heated to boiling. The diluted urine is dropped in from a burette till all the mercury is precipitated. The test solution, at first muddy and opaque, becomes clear and somewhat yellowish. The reaction is finished when a drop of the liquid on

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[1] 'Deutsches Archiv für Klinische Medicin,' 1870, p. 67.

[2] 'Arch. de physiol. norm. and path.,' iii, 26.

[3] "Ueber eine neue Methode zur Bestimmung des Traubenzuckers," 'Annalen der Chem. und Pharm.,' vol. lxxviii, p. 252.

filter paper does not take a brown tinge when exposed to the action of the fumes of strong sulphuret of ammonia (Schwefel-ammonium). The end of this reaction can be detected with the greatest accuracy when the paper is held before the window. In the beginning of the reaction the whole drop becomes brown; at the end only a narrow ring of colour is formed at its periphery. Löwy proposes a modification of Fehling's standard fluid for the volumetric analysis of sugar. Fehling's fluid, as is well known, moulds easily, and is decomposed by the action of light. Löwy proposes glycerine instead of the Rochelle salts. He gives the following directions for his standard fluid:—Sixteen grammes pure sulphate of copper are dissolved in sixty-four grammes water, and eighty c.c. of a solution of caustic soda of the sp. gr. 1·34 (nearly 112 grammes), are added by degrees to this solution; finally, six to eight grammes pure glycerine are added. A clear deep-blue liquid is the result. This solution is said to remain perfectly unchanged.

Schubert finally proposed a method which is based on the reduction of sugar by caustic potash in excess, even at a low temperature.[1]

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[1] 'Verhandlungen der Physik. Med. Gesellschaft in Würzburg.'

III. — The Administration of Medicines in comparatively small and frequent doses. By John Kent Spender, M.D. Lond., Surgeon to the Mineral Water Hospital, Bath.

I propose in the present essay to exhibit the advantages of administering certain medicines, when given by the mouth, in comparatively small and frequent doses.

The inquiry I am about to enter upon is in one sense old, in another new. It is quite an old practice to administer a medicine at frequent and definite intervals during the emergency of disease or pain; the natural impulse is to try and cure extraordinary suffering by extraordinary and rapid means. But it is a new proposal to establish this method of practice as a defined and regular thing under defined and regular conditions: not as a remedial method merely for urgent distress, but as a trustworthy way of dealing with particular phases of slow disease. I shall attempt to show, too, that there are certain deviations from health, neither very quick nor very chronic, which can be alleviated or removed by the same method; and that this method has some positive and unquestionable advantages. The difficulties and hindrances which attend the practice of it will be enumerated and examined; and the circumstances will be pointed out which appear to render this mode of administering medicines unadvisable, or of inferior utility to the common plan.

The title of this essay requires one or more postulates to start from. What is a small dose? What is a frequent dose? Both adjectives are of relative significance, but the former represents a quantity varying necessarily with the medicinal substance to which it is annexed. Further, "doses" are things determined by the variables of time, place, and circumstance; what is specified in the 'British Pharmacopœia' as a "dose," should, I imagine, be regarded as that moderate equivalent of a drug which can do no harm, even if it fails to achieve the least appreciable good. Accepting this conventional interpretation of the word "dose," a *small* dose would mean any quantity *below* the standard, and a *large* dose any quantity *above* it. Even this definition does not exhaustively express all we wish to convey; for a medicine may be used in different doses with the avowed purpose of fulfilling widely different ends, a small and a large dose of it respectively having no specific or even generic relation with each other. So that we must qualify our statement by saying that a *small* dose, or a *large* dose, is one relatively small or large for the purpose which we have in view.

Next, as to the *frequency* with which a medicine is given. This also must be measured by a standard, real or imaginary; and the customary professional standard of administering a medicine is formularised by "bis terve die," now and then by "quartâ quâque horâ." More often than this constitutes a relative frequency which requires me to draw an arbitrary line somewhere; and I fix *two hours or less* as the interval at which the doses of a medicine must be given in order to bring it within the scope of this essay. My two postulates, then, are these: a *small dose* signifies a dose of medicine which, when given to an adult person, is smaller than the standard dose specified in the 'British Pharmacopœia;' and a *frequent dose* signifies a dose of medicine given (say) four or more times at intervals which do not exceed two hours, and which may be less. And I shall try to demonstrate the advantages which, under particular conditions, appear to follow this plan of treatment.

Every medicine, properly so called, represents a composite power, physiologic and therapeutic. Its physiology can be determined sometimes deductively either from its composition, or from its manifest relation to the blood and tissues. Its therapæia can be predicated sometimes deductively too; more often it is discovered by experiment or by pure accident. But whichever element predominates, the physiologic or empiric, the fact remains that a new compound, of greater or less complexity, is added to the blood; and, permeating every part of the body, it modifies every nutritional act performed within certain limits of time, *i.e.* until it is wholly excreted from the body by the excreting glands. It cannot be said that the "syphilised plasma" of a syphilitic subject can be more than evanescently affected by a single dose of mercury or of iodide of potassium; a "dose" of benefit is received, but the constitutional vice of the "syphilised plasma" is so little modified by that single and separate quantity of medicine, that no perceptible alteration follows, and the *vis morbifica* is practically unchecked. Why must so many "doses" of medicine be given before the syphilised plasma becomes *un*syphilised?

If that number can be counted (as it may) and calculated (as it ought to be if all the data are forthcoming), why may we not shorten the period of our healing process by embracing that number within a less time?

A whole series of problems is opened up.

The *number* just spoken of is probably determined by a profound reference to the cyclical changes of the human body. Probably so many changes have to be transacted—so many currents to flow. Finally and gradually the tissues are renovated, and the tracks of dynamic error are effaced. Again, a quantity of a particular medicine is labelled *poisonous* or *dangerous, i.e.* it is poisonous or dangerous if taken by a person all at once. Now, suppose that we break up this quantity into equal parts—say five or more; how near in time may these parts be administered before any dangerous or poisonous results are developed? The one large intoxication is disintegrated into five little intoxications; they crowd upon one another, but each has very nearly gone before a fresh one comes. The excretory organs unceasingly operate to expel what is a foreign body—an alien body no less than the disease which it cures, though by its grateful aid that disease may be brought to an end.

The key to the principle which this essay is intended to illustrate is, therefore, as follows:—Increase the frequency of a medicine, not its amount. Given a specific disease, to saturate the system with its antidote. Do not, if possible, administer a medicine so as to add its own toxical error to the morbific error which that medicine is designed to remove or relieve. And if this evil be occasionally unavoidable, shorten its duration within the smallest practicable limits. Withdraw the medicine by degrees, and, before it is withdrawn altogether, see that the counteracting forces are being steadily controlled, and at length entirely conquered or eliminated. Examine all the surrounding elements of hygiene, and neglect no auxiliary means which may help to restore and maintain the health.

The dynamic effects of small frequent doses of a medicine keep pace with the molecular changes of nutritional force. The renovating momenta of the two forces go on together, and even assist each other. The new element is introduced into the system *guttatim* (so to speak), and carries with it the many qualities which proverbially belong to quiet perseverance. But the notorious fact that nearly all substances so administered can be detected in the urine within a few seconds afterwards, proves that there is an opposition between those substances and the principle of organic life, even when they are introduced with the intention of putting it right by driving out or by neutralising something wrong.

A definite residue is appropriated, dynamically incorporated with blood and tissue. This is really all that is required; but what is excreted is by no means mere surplusage. It is pushed out of the system partly because it cannot help being excreted with the normal excreta, but partly because it represents work already transacted in the system and duty done. Its quittance is a declaration of irreconcileableness of essence between itself and animal life; its mystery is accomplished, and so it is cast out.

Take belladonna as an illustration of this doctrine. Belladonna has a potent influenee on spasm; it quells and relaxes it; a rhythmic convulsion like asthma, which comes on at special times or on special days, is held in check. But no curative operation is performed. The spasm is prevented only while the belladonna circulates in the blood. Omit the belladonna for a single night, and back the asthma comes. But the convulsion is not so severe as if the belladonna had never been given; the neurosis is favorably modified; we may legitimately infer that some of the belladonna has been appropriated by nerve-tissue, and that the morbid habit of that tissue has been *pro tanto* controlled.

Belladonna poisoning is an awkward phenomenon to look at; but it is devoid of real danger unless the quantity taken has been mortally large. Leave the poisoned person alone; do nothing for him in the way of antidotes, and gradually will all the functions return without a trace of damage. There will be no ill afterwards, even of a temporary kind, except what may come from the interruption to the healthy ingestion of food.

Tartrate of antimony often abolishes inflammation in a remarkable way when administered in the method which will be illustrated presently. Select a case of typical phlegmon—acute and quick—and administer one sixteenth of a grain of tartrate of antimony in solution every hour for twelve hours, then every two honrs for twenty-four hours more; by this time the inflammation will be clean gone. Usually the patient will be ignorant that he has been taking medicine at all, so far as any collateral effects are concerned.

Now, when we consider the action of this method of subduing inflammation, and the contrast it offers to the "*vi et armis*" plan by which it was formerly deemed necessary to knock down and expel the inflammatory demon, the candid inquirer must acknowledge the radical difference. In the old way certainly the disease was sometimes knocked down, but the whole body was knocked down too. A fourth of a grain of tartrate of antimony given every four hours caused serious nausea and depression—possibly syncopal disturbance of the heart. This was the costly price at which disease was got rid of, and occasionally there was no riddance at all. The clear and accurate pathology of our own day proves inflammation to be (without metaphor) an error of nutrition—a neurose perversion of a natural process; and how consonant it is with philosophy and common sense to try and correct this error by quiet coaxing ways, by the persuasive influence of "*sæpè cadendo,*" the correcting agent being thus not a new disturber and disorder producer, but a new beneficence and guard.

As a rule, when a medicine has been administered with unfailing regularity every hour or every two hours (except in the night) during from one to three days, it should be *discontinued by degrees.* This principle is exceedingly important, and much disappointment has come when it has been neglected by the patient or by me. I shall be able to show its necessity when I relate the safeguards and cautions which demand observance while tartrate of antimony is being given for the cure of inflammation, and iron for the cure of erysipelas. Inattention to it may entail the labour of beginning our work again, to say nothing of lost time, and the occasional impossibility of doing afterwards what was done once only with difficulty. Nor does the subsequent administration of the medicine at longer intervals take it away from the scope of the principle enforced. The whole scheme is logically bound together, and the end is no less an integral part of it than the beginning. And in practice it will be found to be one of the most delicate touches of art to hold one's hand, and to withdraw in a timely and gradual manner the material instrument of healing.

Some practical difficulties are alleged to lie in the way. Trouble is imposed on the attendants of a sick person; there may be a dangerous accumulation of a medicine in the system; and a medical man has no means of testing regularity of administration. Now, it is obvious that every earnest and true scheme of therapæia must involve trouble. I do not urge that a medicine be given during the night (11 p.m. to 7 a.m.), except under the pressure of a grave and almost desperate exigency. I do not say that refreshing sleep is to be disturbed during the day in order to give a specified dose: none but a pedant would do this, and no pedant ought to undertake a nurse's duties. Similarly no one ought to minister to a sick child who has not much judgment; for there are certain medicines which, when administered to children in any sort of cumulative fashion, may unexpectedly induce dangerous or alarming symptoms, calling unmistakably for a suspension of the treatment. Other casualties may occur, requiring the opinion of the medical adviser; or a manifest benefit from the medicine may be so suddenly established as to justify its discontinuance, or its administration at longer intervals.

Just as different quantities (or "doses") of a medicine may signify so many different ideas, or even represent so many theories; the diverse frequency in which that medicine can be administered may be symbolical of diverse methods, each constituting a separate canon. In the former case, we think primarily of the element of *quantity*, the arithmetical measure of how much or how little; in the latter case, the element of *time* predominates, and a rule of stern punctuality has to be observed. Whatever may be the determining principle which guides the practitioner in prescribing a medicine, it is essential that he have a competent knowledge of the several therapeutic forces which that medicine embodies, and which ordinarily it is capable of exercising. Then, having clearly discerned and identified his mark, he aims at it with that instinctive precision which governs the marksman in shooting his arrow or his bullet, the cricketer in throwing his ball, the player in hurling his quoit. That there is often a waste or error of force in the execution of all these actions, is simply equivalent to asserting that human imperfection exists everywhere. But all honest endeavours to reduce that imperfection to its lowest amount form the basis of sound knowledge.

I proceed now to the illustrations of my paper. I begin with the relation of cases in which opium, or its derivative morphia, was administered in "comparatively small and frequent doses;" some of the cases are drawn from private, others from dispensary, practice.[1] All occurred in my own practice, and were under my own professional care through the whole course; and of nearly all notes were taken at the time.

(1.) P., March, 1858.—Married female in the middle rank of life, æt. 60. Acute cerebral rheumatism, preceded by diarrhœa, profuse sweating and collapse. The first thing I did when called to the case was to darken the patient's room, order plain milk diet, and give one grain of powdered opium in a pill every hour for six hours; then every three hours for twenty-four hours; then less frequently during two or three days. No other medicine was prescribed until the fever began to pass away; quinine then helped the natural progress towards cure.

The neurological theory of rheumatism is reviving; it has been ably advocated by Dr. H. Day in his recent 'Commentaries,' and by Dr. Ridge in late numbers of the 'Medical Times and Gazette.' I have always inclined to it as the only one adequate to explain all the clinical phenomena, and the treatment by opium—long ago strenuously urged by Sir D. Corrigan and Dr. Sibson—affords a good plea for the soundness of the theory. If excessive alvine flux precede the rheumatic affection, there is really no other treatment available; and, in all other cases, opium can be administered hand in hand with alkalies even when pushed to the extent recommended by Dr. Fuller;[2] and a further merit of opium is that it favours the escape of the serous membranes from rheumatic inflammation. The case just related is typical of other cases which have been under my care.[3]

(2.) D., May, 1867.—Boy æt. eleven; child of very poor parents; spare and ill-nourished. Acute tubercular peritonitis; sudden access of excruciating pain, suggesting perforation of bowel or fœcal abscess; sickness and constipation; pinched features; great prostration. A grain of opium gum was administered every hour for several days, and he was allowed to drink iced milk and barley water. A thin hot linseed poultice over the abdomen. The case

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[1] Those from private practice are marked P.; those from dispensary practice are marked D.

[2] 'The Practitioner,' March, 1869.

[3] Those who are interested in the history of the theories of disease should read a chapter (Chapter VI) in Dr. Pring's 'Exposition of the Principles of Pathology, and of the Treatment of Diseases,' London, 1823, p. 321. The chapter in question is entitled "The Origin of Disease in the Nerves." Dr. Pring became more of a humoralist as he advanced in life, and at the end of this chapter it will be seen that he distinctly abandoned his early views.

seemed very unpromising, but eventually the boy pulled through well, and was heard of last May as fat and strong. It deserves note that he was treated in his own wretched home, and had none of the benefits of hospital nursing.

Inflammation of serous membranes would, in the judgment of many practitioners, always clamour for the administration of mercury. In strong, healthy adults, the union of equal parts of *hydrargyrum cum cretâ* with opium may be a good point of practice, especially in inflammation of the cæcum; but the opium is always foremost in its virtue, and in almost every other case should be given alone. In the so-called inflammations of the tubercular diathesis, the bare mention of mercury is a heresy. It is not only absolutely unnecessary, but pernicious in all conceivable ways; a long period of bad health may be engendered by it, possibly the development of acute tuberculosis. The opinion seems growing that all serous inflammations can be safely treated without mercury.

(3.) D., June, 1869.—Married woman, æt. 40; tall and thin; bore her fifth child in March, 1869. Incessant vomiting from pyloric cancer; diagnosis easy, but had been overlooked by medical man who attended the labour, as he attributed the vomiting to the pregnancy. Of course the prognosis was as hopeless as it invariably is in malignant disease, but the vomiting nearly ceased for more than a month, when powdered opium was continuously given in half grain doses every two hours. Only cold milk was allowed as food. The vomiting returned badly at intervals, but it was always relieved by opium, which was sometimes combined with the extract of belladonna. Death from ascites and exhaustion on October 12th. This patient was treated at her own home all the time, but she was supplied with sundry comforts by benevolent friends.

I plead earnestly for the liberal administration of opium in all instances of malignant disease in which pain is an urgent symptom. The penurious dole of a grain of opium every night, or of half a grain now and then, is a mock at suffering. I ask that opium may be administered freely, and yet not without rule and careful measure; I ask the practitioner not to wait till pain clamours for medicine, but to offer it readily as a preventive and a comfort. In most cases opium is assimilated with remarkable success, and with a total absence of those inconveniences proverbially associated with this drug. It is sometimes better still to give morphia, and now and then to order a hypodermic injection of morphia once or twice a day (as occasion demands), auxiliary to smaller doses of opium or morphia by the mouth at shorter intervals.

I have found the opium gum of signal advantage when it has to be given frequently. Prescribe a grain (or even more) every two hours, and the nervous system is kept in happy quiescence; and if we can ward off the acuter paroxysms of suffering, the temptation is removed to take opium in large and irregular quantities, or to inhale chloroform perhaps to a dangerous amount. The painful spasms which accompany the passage of an urinary or renal calculus down their respective ducts are well known to be susceptible of relief by opium; but the best mode of administering it is not in shock-like quantities of heavy narcosis, but by the gradually tranquillising influence of "small and frequent doses." The phenomena of the puerperal state shall supply my last illustration of the therapæia of opium.

(4.) P., August, 1862.—A lady, æt. 40, was delivered of her fifth child; severe post-partum hæmorrhage, as on nearly all previous occasions. This (says Dr. Barnes) should always make us watchful for any event of the pyæmic kind. On the fourth day after labour, without warning, severe pain set in about the uterine region, with apparent collapse. I was summoned almost immediately, and ordered a grain of powdered opium every hour for six hours. The benefit was quick and positive; pain went away, and the skin became warm. The same dose was then given every two hours for a few times, and afterwards at gradually lengthening intervals. The recovery was uninterrupted and complete.

(II.) Morphia, or the therapeutic essence of opium, identical with opium in most of its properties, has yet some special virtues of its own.

I propose to relate, first, all very illustrative cases, and then to comment on those points in the histories which deserve special note.

(5.) D., May and June, 1869.—An artisan, æt. 40; tolerably healthy; no history of rheumatism or of syphilis. Severe sclerotitis, which, in point of duration, almost deserved the name of "chronic;" probably slight iritis. Only one (the right) eye affected. One twelfth of a grain of acetate of morphia in solution was administered every hour for three days; every two hours for ten days; and then gradually at wider intervals. Two grains of blue pill were given every night for the first ten days. No aperient medicine was needed. The recovery of vision was absolute.

(6.) P., September, 1869.—Lady's maid, æt. 23; healthy, but complexion somewhat anæmic. Acute conjunctival catarrh. One sixteenth of a grain of morphia in solution was administered every hour on the first day, and one twelfth of a grain every hour on the second day; the same quantity every two hours on the third day; and every four hours on the fourth and fifth days. She was then quite well.

A number of cases like this have been treated by me with perfect success during the last seven years, both in private and public practice.

Other neuroses may be favorably modified by morphia.

(7.) P., May, 1863.—A young gentleman, æt. 10, suffering from the troublesome sequel of whooping cough; cough incessant, but the peculiar spasm of the specific disease had passed away. One twenty-fourth of a grain of acetate of morphia in solution was prescribed every two hours for the first two days; afterwards at longer intervals of time. The cough was notably alleviated almost immediately, and the patient enjoyed refreshing sleep during the first night. Scarcely any drowsiness was produced by the morphia, nor was any other inconvenience felt; not even constipation. Within five days the cough had nearly gone.

(8.) D., May, 1869.—A female child, æt. 5; parents not very poor; house clean and healthy. Whooping cough, rather severe; no complications. One thirty-second part of a grain of morphia, with three grains of bromide of potassium, in solution every two hours; mother was instructed to suspend the medicine for four hours at any time if unusual drowsiness came on. No medicine was given during the night. The usual auxiliary treatment as regards diet and clothing. The paroxysms of whooping cough soon became less severe, and the duration of the disease seemed shortened.

There are many mysteries about the therapeutics of morphia which we have yet to fathom. I incline to the opinion that the extraordinary and unexpected benefits which have been reaped from the subcutaneous injection of morphia have partially overshadowed the good which morphia is capable of affording when administered in the usual way. Its power over neuralgias and neuroses, after they have become chronic habitudes, is notably shown when prescribed in the small recurrent quantities which Dr. Anstie calls "stimulant," rather than in the larger and much less frequent quantities which produce phenomena approximating "narcosis." These heal and soothe for the vicissitude which evoked them; those are less hasty to avert suffering, but their ultimate effects are equally sure, and almost always more lasting. Adopting Dr. Inman's opinion, the drug thus given probably exerts a modifying influence on the organic condition of the affected part; it may prevent a threatened destruction of tissue, and consequently relieve pain.[1] But I cannot agree with Dr. Anstie's axiom that "when a particular symptom, *e.g.* pain, can be relieved only by narcotic doses of any drug, the medicine is probably altogether an improper one for the case."[2] A single dose of morphia may relieve (it is remotely possible that it may even cure) an obtrusive neuralgia; it does so distinctly through the medium of narcosis; but there is nothing intrinsically improper in our eliciting this narcosis. The narcosis passes away;

[1] Quoted by Dr. Anstie, 'Stimulants and Narcotics,' p. 118.
[2] Ibid., p. 280.

any possible evil arising of the narcosis passes away too; and the pain is gone, at least for the time. The probable (almost certain) return of the pain proves, not that the remedy is wrong, but that it is *administered in a wrong way*. Administer it in the right way, and it is proved to be a right remedy; and it is further proved to be *the* right remedy by the fact that none other can supply its place.

Accepting Dr. Anstie's terminology as sound and authentic, I base upon it my proposed method of working. I break up a "narcotic" dose of morphia into fractional "stimulant" doses; I order these to be given to a patient with punctilious regularity at definite short intervals of time. I never allow (if it can be avoided) the accumulative influence of "stimulant" doses to cause "narcosis." I have the power of controlling this (under ordinary circumstances) by *lengthening the intervals* or by *diminishing the quantities*; and I prefer the latter plan as more truly in accordance with the physiological operations of nature: and thus a typical narcotic, like morphia, may become a rational stimulant medicine, rectifying, by its direct action, "some deficient or too redundant natural action or tendency."[1]

It deserves note that long ago Drs. Bardsley and Stokes recommended morphia as a remedy for chronic gastritis to be administered in an order inversely to that which I have spoken of; that is to say, one twelfth of a grain is to be given twice on the first day, three times on the second day, and so on, increasing the quantity until the patient consumes one to one and a half grains in twenty-four hours.[2] It is not said whether the medicine is to be left off in the same gradual manner in which it is begun.

Digressing for a little while to the subject of the general action of morphia, there are plenty of facts which attest the value of this drug in promoting nutrition. A man, æt. fifty, is lying in his bed crippled by chronic rheumatoid arthritis; he is racked by much pain; he is sleepless and emaciated. I persuade him to take a small dose of morphia five or six times a day for several months; and, besides the ordinary relief which might *à priori* have been expected, the skin becomes warm, the appetite returns, the muscles are better nourished, and at the end of a year he scrambles down to my out-patient room. Again, whenever a neuralgia, apparently pure and simple, is the one pressing symptom of distress, it is right to administer morphia in very frequent stimulating doses, while we are ripening other more eclectic methods of treatment. A delicate woman asks to be cured of an acute uncomplicated pleurodynia, or neuralgia of the intercostal nerves. What are the therapeutic ideas which her case suggests? One physician would prescribe imme-

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[1] Dr. Anstie, op. cit, p. 161.
[2] Dr. Waring's 'Manual of Practical Therapeutics,' p. 469 (second edition).

diately the dynamic tonic, quinine; a second, the chemical blood tonic, iron; a third, the nutritional tonic, cod oil; and a fourth might be contented with the moderate instalment of an opiate embrocation. But her pressing need will not be removed by any of these means except in terms of weeks or days, and she craves to be relieved as quickly as possible. Now give her minute and frequent doses of morphia, just sufficient to cause the most trifling drowsiness, and yet not enough to interfere with the performance of life's daily occupations; observe that the pain is almost to a certainty gradually and surely palliated, and its urgency blunted.[1] Withdraw the medicine little by little; while it is being withdrawn, begin to introduce the nutritional and blood tonics which are the pabula of nerve tissue, and which are the antidotes to that pain which signifies the deterioration of this tissue. And thus the philosophical order of our therapœia is sometimes to attack first the effect, and then the cause; and for the plain reason that the effect is peremptory and injurious, clamouring for alleviation because of its annoyance and damage.

I proceed to comment on my cases. Nearly ten years ago the late Mr. Z. Lawrence illustrated the successful treatment of acute sclerotitis and iritis by the "antiphlogistic" power of morphia.[2] His cases are remarkable, and are not disposed of by recent criticism;[3] he suggests that, in a certain class of cases, pain may be the cause of repletion of blood-vessels, and he submits that the action of morphia may be to reduce that nervous irritability which is the primary cause of the inflammation. Since the publication of Mr. Z. Lawrence's paper I have pursued the same line of treatment in cases of conjunctivitis and sclerotitis; and also in some cases of iritis, combined with the local use of atropia. I eliminate the latter cases because the use of atropia vitiates them for purposes of evidence, and I relate two examples which are similar to a host of others that have come under my care during the last nine years.

My own practice differs from that advised by Mr. Lawrence in one specific point, though generically identical. The doses enumerated by him are administered never with greater frequency than every third hour, sometimes every fourth hour, and the amount of each dose is comparatively large (a quarter to half a grain). I strongly recommend a great reduction of dosage, and a proportionate increase of frequency in its administration. To a child of five years old I give one fortieth to one thirty-second of a grain of morphia every second hour, with the rigorous instruction that the effect be

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[1] It is not inconsistent with my plan to give a double or a treble dose *horâ somni*, and this I usually do. In the text I have ventured to draw a model figure, with whose head and stomach morphia is assumed to "agree."

[2] 'Med. Times and Gazette,' December 31, 1859.

[3] Mr. Carter in the 'Practitioner,' July, 1869.

carefully watched. For an otherwise healthy adult I often prescribe one twelfth of a grain of morphia every hour for the first twenty-four hours (omitting, as usual, the eight hours of the night), and one tenth or one eighth of a grain every two hours for the subsequent two or three days. The only local application necessary is a compress of lint dipped in warm water (or cold water, if the patient prefer it); and the affected eye is never allowed to be opened during the early stage of treatment, except for necessary inspections by the medical man. The effect of this simple scheme of therapeutics is usually very happy. The patient may be scarcely aware that any drug is being taken, as only the slightest approach to narcosis is permitted; he is prudently confined to the house, and he is prudently restricted in his diet; an occasional purgative also may be advisable. But note how quietly the vascular congestion of the conjunctiva and sclerotic disappears; how the pain and photophobia diminish, and may have gone entirely on the third or fourth day of the treatment; and sometimes the cure may be completed in less than a week without any other help, though it may be expedient now and then to finish with quinine as a vaso-motor tonic, in order to gain a perfect victory.

Justice to Mr. Lawrence's plan is hardly done by distinguished ophthalmologists. It is necessary to be clear about the presence or absence of syphilitic taint; but if there be the slightest uncertainty, the safe course is to order a small dose of blue pill once or twice daily, as in the history of my own case (5).

The troublesome and importunate neurosis, a cough, affords an excellent field for the exercise of my therapeutic principle. When a cough is a mere factor (though a principal one) in some specific disease, that disease requires to be attacked with clinical precision if we are to acquire a mastery over its several factors. In the most recent exposition of hooping-cough[1] the writer politely dismisses the drugs which have been propounded for its cure during late years; and he emphasises the view long maintained by him, that morphia is the best remedy, because it is more certain and uniform in its action than all others. I am weary of treating hooping-cough with any medicine but morphia, but I think that bromide of potassium may be usefully combined with it. Dr. E. Smith's proposal is as follows:—"With children under one year of age the dose of the hydrochlorate or acetate of morphia should be one sixty-fourth of a grain, repeated every four hours; with children between one and three years of age, one forty-eighth to one thirty-second of a grain; and with those yet older, one thirty-second to one twentieth of a grain. The dose selected should be repeated three or four times,

[1] Dr. Reynolds's 'System of Medicine,' vol. i, pp. 271-86. The author of the article is Dr. E. Smith.

and if no perceptible drowsiness be induced it should be increased a step and repeated in like manner, and again increased, if necessary, until the dose has been found which produces the slightest oppression of the sensorium; the aim must then be to maintain this effect by repeating the same dose, or by further increasing it from time to time. The cases of slight hooping-cough are extremely few in which *slight* drowsiness has been produced and uniformly maintained for three or days without the spasm having subsided, and the cough nearly reduced to that of a common cough."[1]

There is nothing so successful as success, says the proverb, and this is equally true when we cannot explain the success. But Dr. E. Smith's success is based upon reason, not upon empiricism. As before, I have an amendment to make. It may be wise in many cases to administer morphia to an infant with extreme tentative care, and a dose may be appropriately given at first every four hours. But to almost any child from six months to a year old it is absolutely safe to give a single minim[2] of the Liq. Morphiæ Hydrochloratis, vel Acetatis, every hour for several hours, in order to pacify a convulsive cough. After five or six doses the medicine may be left off for a little while, to be resumed either in the same way or (perhaps better still) in doses of a minim and a half every two hours. It is presumed that the infant is under the incessant guardianship of an intelligent nurse, that the taking of food is interfered with as little as possible, and that the general precautions expressed by Dr. E. Smith are punctually adhered to.

Other medicines may cluster around morphia when used according to the directions now given, but strictly subordinated to it.

Opium and morphia are, therefore, grand remedies for inflammation, pain, convulsion. Dying struggles may be alleviated, sometimes removed, by them, especially the final agonies of laryngeal phthisis, and of many forms of malignant disease. They are particularly useful in certain rapid and painful phases of rheumatoid arthritis. When to give opium rather than morphia, or morphia rather than opium, is not requisite here to define; it is a problem which rarely puzzles the experienced therapeutist.[3]

(III) *Tartrate of antimony* may challenge all other medicines for certainty of action; and when administered in the mode about to be recorded, its conquest over disease is not only the most effective, but the inconvenience to the system the very smallest.

(9) P., September, 1866.—A lady, æt. 38, was delivered of her eighth child; she suffered severely from flooding. On the afternoon

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[1] Op. cit., pp. 282–3.
[2] Equivalent to $\frac{1}{120}$th of a grain of the salt.
[3] Among eccentric neuroses I may mention "hay fever" as often capable of very great relief by a drop dose of tincture of opium given in water every hour.

of September 5th, fifty-eight hours after delivery, signs of inflammation of the left breast were noticed. Six hours afterwards, late at night, I was called to see her on account of the violence with which the inflammation had set in. Fifteen drops of antimonial wine (equal to one sixteenth of a grain of 'tartar emetic') were given every hour through the night, and until I saw the patient again at eleven o'clock next morning. The inflammatory hyperæmia was then entirely gone, the breast was only a little more swollen than the other, and there was scarcely any pain. There had been not only no vomiting, but no appreciable nausea. The medicine was continued in the same dose every two hours until the next day, then every four hours for another day, and in less than four days from the beginning of the treatment all signs of inflammation of the breast had disappeared.[1]

(10) P., September, 1866.—The wife of a tradesman had a rapid favorable labour on September 25th. On the fourth day after delivery inflammation of the left breast was suddenly developed. On account of general nervous excitability and a tendency to diarrhœa, one drop of tincture of opium was combined with fifteen drops of antimonial wine, and administered in water every hour for fourteen hours. After five doses had been given profuse diaphoresis occurred; coincidently with this the pain suddenly went away, and light sleep followed; and within three days from the beginning of the treatment there was no vestige of what had happened. The local means used were the same as in the last case.

I relate these two cases as typical of several others treated in the same or similar manner with equal success.

(11) D., June, 1869.—A maiden female servant, æt. 23, applied to me on account of a threatening abscess of the breast, the result of a blow. She appeared in tolerably good health. One sixteenth of a grain of tartrate of antimony was given in solution every hour for three days (excluding the night); then every three hours for two days more. The inflammation quietly went away, and she did not attend after a week.

I quote this case because of its logical value. It offers a ready-made illustration of one of Mr. J. S. Mills' four experimental methods, namely, the *method of difference*. Nothing was done for the patient but the administration of the medicine specified; no other medicine was ordered, not even an aperient dose; there were no local applications; no alteration whatever was made in her manner of life or in her diet. The tartrate of antimony was the single new element introduced—the factor of the artificial experiment, all the surrounding circumstances being the same. And it is the very nature of an experiment to introduce into the pre-existing circum-

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[1] In this case no local application was used, except a piece of hot wet flannel covered with oil-silk.

stances a change perfectly definite. "We choose a previous state of things with which we are well acquainted, so that no unforeseen alteration in that state is likely to pass unobserved; and into this we introduce, as rapidly as possible, the phenomenon which we wish to study, so that in general we are entitled to feel complete assurance that the pre-existing state and the state which we have produced differ in nothing except the presence or absence of that phenomenon."[1] Hence it is susceptible of complete proof that the tartrate of antimony cured the threatened inflammation in the case last given, as nothing else was done but this, and every other circumstance remained unchanged.

(12) P., August, 1869.—A maiden female, æt. 30, daughter of a tradesman. She had been nursing a sick relative, and had used the left arm beyond her strength. The left breast was enormously swollen and tense, but there was no positive evidence of suppuration. Twenty drops of antimonial wine were ordered every two hours, and were continued perseveringly for more than a week. The result was only a small limited abscess on the outside and upper part of the breast, which discharged and then quickly healed. Quinine and iron were prescribed in order to renovate the general health.

This case looked very unpromising when the treatment was begun; both the local malady and the constitutional weakness had been neglected. It cannot be too often or too clearly stated that the formation of pus is an absolute bar to the success of the antimonial treatment; this treatment is then not only useless, but hurtful. The opportunity for the application of preventive measures has passed, and new plans have to be devised.

In another case of mammary inflammation, which I need not quote at length, and which conveys a very useful lesson, the antimonial treatment was left off too soon; worse still, a blood tonic was prescribed. So obstinately and quickly retrograde was the course of the disease, that, although the mistake was very early seen and the antimony recommenced, the inflammation never stayed its course for an instant, and the patient had to endure a large abscess with all its sequels.

(13) P., July, 1869.—The wife of a farmer, a strong, healthy woman, of middle age. Inflamed varix of right leg, exquisitely painful; much redness of skin over inflamed veins. The leg was placed in the recumbent position; a pledget of hot wet lint was placed over the course of the vein, which was covered by oil-silk. Twenty drops of antimonial wine were given in water every two hours; no other medicine was required. A great improvement was visible in two days, and within five days no trace of inflammatory action was left.

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[1] 'System of Logic,' 3rd edition, vol. i, p. 399.

(14) P., October, 1869.—A gentleman, æt. 33, injured his ankle by an awkward fall. Within a few days a painful red swelling was noticed on the upper part of the inside of the leg, just below the knee. From this red streaks of angeioleucitis ran up the thigh to the femoral glands, which had already begun to swell and to be painful. Health pretty good, but he was rather feverish. A single purgative dose was given, and then fifteen drops of antimonial wine in water every hour for four days, sixteen doses being taken every day. The inflammation of the lymphatics went away almost directly, the femoral glands ceased to be painful after two days, and the swelling below the knee, which was as near suppuration as possible, began to lose its inflammatory characters. The antimonial medicine was continued every two hours for three days, and, finally, every three hours for two days more. The recovery was complete. During the latter part of the treatment the leg was bandaged. It is proper to state, also, that during the earlier part of the treatment the limb was kept in the recumbent position. The diet was not altered.

The cases now adduced are sufficient to exhibit tartrate of antimony as an "antiphlogistic"—to use the old terminology—of unrivalled efficacy and certainty in the treatment of certain external inflammations of the pyæmic or erysipelatous kind. I am indebted to Dr. Beatty and Dr. Churchill[1] for a knowledge of the curative power of tartrate of antimony over inflammation of the breast when occurring after parturition. I discerned no reason why the same treatment might not be employed when we have to deal with mammary inflammations unconnected with the parturient function. Then the argument from analogy led me to hope that kindred inflammations in other external parts of the body might be controlled and cured by the same method. The important fact has been verified that tartrate of antimony can achieve this triumph over disease without the display of any counterbalancing inconvenience or trouble, even of the most trivial degree. A patient may not perceive the slightest physiological sequence any more than if so many rations of cold water had been administered to him. Even nausea is a pure μύθος, and vomiting is a higher μύθος still. I have not found that so-called weakness, or spanæmia, affords any valid barrier to this plan. Indeed, a weak person had surely better take tiny doses of tartrate of antimony and be cured of a local inflammation, than suffer the pain of an abscess and the drain of puriform or pyoid discharge. Common sense dictates the surgical auxiliaries of "rest," "position," and "hygiene;" but the central fact is the administration of the tartrate of antimony, without which all other means are shadows.

Some inflammations are more easily curable by opium or morphia, others by tartrate of antimony. Whence is this difference, and how

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[1] Churchill's Manual of 'Diseases of Women,' p. 752, 4th edit.

are we to know which drug to use? The question is too large to answer fully here, but the presence or absence of pyrexia has some determining influence. Well-marked pyrexia, as there was in some of the patients whose cases are last related, should at least suggest antimony. On the other hand, the predominance of pain or of nervous irritability tells us that opium or morphia should be preferred. But there are cases in which a combination of tartrate of antimony and opium, or of tartrate of antimony and morphia, furnishes results of high therapeutic interest.

What is the value of this method in the management of internal inflammations? All available evidence points to its utility; but that evidence is qualified by a host of surrounding "circumstances," which hopelessly impair its logical force. A simple naked pneumonia, if such a pathological thing exist, how do we treat it? Ethics forbid us to experimentalise when vital function (perhaps life itself) is at stake; and so we put a patient in bed, make him rest, surround him with warm clothes, compel him to inhale warm moist air, "counter-irritate" the skin of the thorax, supply him with food and wine in quantities and qualities easily assimilated, and, finally, in obedience to authoritative dogma, we ask him to take a little medicine! But if the tartrate of antimony be made to produce its specific physiological effects, as in Dr. Cheyne's treatment of croup,[1] we have clearer landmarks of what our drug is doing. It cures now *because* it causes nausea and vomiting, and by virtue of those processes; and we are logically right in attributing to them a portion at least of our success, if success be attained. And our skill is appropriately directed to the guidance and governance of the physiological phenomena, and their subordination to the desired end.

(IV) *Mercury* may be given sometimes with conspicuous advantage in comparatively small and frequent doses. When we wish to produce what is called "mercurialization" with sureness and speed, we can do it by administering *Hydrargyrum c. Cretâ* in a two-grain dose every two hours better than in a four-grain dose every four hours.

(15) D., February, 1869.—A man, æt. 47, a coal-hauler. Chronic syphilitic laryngitis, which suddenly took on an acute exacerbation after exposure to cold and probably to dissipation. The treatment was carried out as just specified, and within thirty-six hours the laryngeal distress had subsided. An inopportune change of weather to a keen damp wind helped the development of a low inflammation of the lungs, which proved fatal on the fourth day of treatment. The mercury was left off as soon as signs of pulmonary disease became manifest, and everything was done for the man *secundum artem*.

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[1] 'Cyclop. of Practical Medicine,' vol. i, p. 497.

The dangerous inflammation of the larynx and epiglottis, which is kindled by the contact of a very hot fluid—an accident so liable to happen to the children of the poor—demands a treatment (*quoad* drugs) swift and energetic in a high degree. A half-grain dose of calomel may be prescribed at first every half hour; after six doses the same quantity every hour, and the quantity and frequency are to be augmented or lessened according as the urgency of the inflammation seems likely to yield. A professional friend recommends one eighth of a grain of bichloride of mercury every half hour, the medicine to be given in solution. An unmistakable collapse or a hopeless spasmodic dyspnœa indicates the further inutility of medicine.

(V) *Iron.*—This great hæmatinic is sometimes needed for managing the acute phases of disease, notably those of erysipelas and pyæmia.[1] The following case is instructive as to the manner in which the iron must occasionally be given:

(16) P., April, 1869.—A girl, æt. 21, daughter of a tradesman. Severe erysipelas of face and scalp; no apparent external cause. After a single purgative dose the administration of the tincture of perchloride of iron was begun, and at first in a dose of ʒss every four hours. The case went on well for four days; she then had an unaccountable relapse, and the iron seemed to lose its beneficial effect. I ordered the same medicine to be taken in a dose of fifteen minims every two hours, both day and night, each dose in a half-tumblerful of water. The result was quick and good, and the medicine was continued in gradually diminishing doses for a whole month. Recovery was complete, and the health became entirely re-established.

However hard, dry, and brown the tongue may be in a patient suffering from an acute erysipelas, we should still resolutely give the iron. It does not act as a mere vulgar tonic; its use and function are probably much more profound, acting to protect the blood from devitalization and decay.

In ordinary cases of chronic anæmia and chlorosis I have not found any special result from the administration of iron in very small and frequent doses, and, therefore, the usual method may hold its ground without fear of disturbance.

(VI) *Gallic acid* is among the first of hæmostatics, but requires to be given in much larger quantities than are sanctioned by systematic writers.

(17) P., June, 1869.—A young lady, æt. 28, pale, but not thin; a morning hæmoptysis of variable amount, always soon after awaking; cough in the morning, not afterwards during the day.

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[1] I may just refer here to the recent valuable investigations of Dr. Russell Reynolds on the treatment of acute rheumatism by iron.

No evidence of tubercle in the chest. I tried various means, but with no success; finally, I ordered four grains of gallic acid in a pill to be taken every hour. This was continued for several days, and then, as usual, left off gradually. No other medicine was given. She has expectorated very little blood since.

(18) P., July, 1869.—The widow of a tradesman, æt. 58. Very profuse menorrhagia for some years; the flux was now and then continuous for several days. Her former medical attendant had not been able to control it. I prescribed five grains of gallic acid in a pill every two hours, with twenty drops of the fluid extract of ergot; and further requested that a quarter of a pint of infusion of matico should be injected into the cavity of the uterus twice a day. The plan succeeded beyond my expectations; the recumbent position was diligently maintained for three or four weeks, and great circumspection exercised about the diet. Finally she got up and walked about as usual, and no blood whatever has been lost since. In this case the suspicion was strong of the existence of polypus uteri, or even of a graver lesion; and if the menorrhagia had not so readily yielded to medical means, an examination *per vaginam* would clearly have been necessary.[1]

In sudden and copious hæmoptysis Dr. Symonds recommends scruple doses of gallic acid, administered at first every hour.[2] My experience leads to the impression that, as a hæmatic astringent, gallic acid is far superior to digitalis and ergot, and possesses advantages of an obvious kind over acetate of lead. Gallic acid seldom constipates the bowels; but if a very *early* and certain astringent effect is desired, acetate of lead is a more reliable agent.

(VII) *Strychnia* has been reported lately to have an excellent effect in the treatment of epilepsy. It is notorious that bromide of potassium sometimes utterly fails in removing epilepsy, or even in alleviating the epileptic convulsion; and I seized the first distinct opportunity of the failure of bromide of potassium in order to try the remedial efficacy of strychnia.

(19) D., September and October, 1869.—An agricultural labourer, æt. 23. Heavy in manner and thick in limb. He had been an out-patient of mine for nearly two years, but cod-liver oil and KBr, pushed to its utmost extent, had done very little towards the mitigation of the "fits." During last summer the "fits" were as severe and frequent as ever. On September 8th he again came under my care, and I ordered one forty-eighth of a grain of strychnia in solution to be taken every two hours. Between September 8th and 15th, the next day of his attendance, there was

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[1] The urgency of this case imperatively forced me to use three trustworthy remedies together, and without delay. *Quà* the medicinal properties of gallic acid, the case is not, of course, intended to be quoted.

[2] 'Brit. Med. Journal,' June 13, 1868.

not a single "fit," and this was the first time for more than a year during which a week had so passed. On the latter date I increased the dose of strychnia to one forty-fourth of a grain. Between September 15th and 22nd there was one small attack of *petit mal.* Again the strychnia was increased to one fortieth of a grain. During the succeeding week he had two attacks of the same kind. On September 29th the dose of strychnia ordered to be taken every two hours was one thirty-eighth of a grain; no toxic effects whatever were produced, and during the whole of the ensuing week he had no "fit" of any kind. Between October 6th and 20th the dose administered was one thirty-sixth of a grain; he suffered one convulsion of the type *grand mal* and three of the type *petit mal.* I saw the patient again on the morning of October 27th, and his report was discouraging. He had had one major fit and two of the minor order during the previous six days. He continued to see me once a week for another month; but about the end of November he had a very severe paroxysm one day when waiting in the out-patient room, and he did not afterwards come. No medicine was given to this man besides the strychnia, and no change was made in his habits. The case goes to prove what has been remarked by many, that epilepsy is sometimes ameliorated for a while by the mere substitution of one empiric treatment for another. Still, the good results of the strychnia in this case were seen in the warmer skin, brighter eye, and brisker walk. The vaso-motor nerves and capillary blood-veesels seemed roused to a more active function. Strychnia is one of our best remedies for headache.

A short time ago I had a private case of "uterine paraplegia" under my care, which surprisingly improved under the use of one forty-fourth of a grain of strychnia every two hours, but the benefit ceased after a certain point had been reached.

(VIII) *Bromide of potassium* may be given in small and frequent doses to children when we have to do battle with convulsions, the origin of which is for the present beyond our scrutiny, but which plainly put life in peril.

(20) P., August, 1869.—A female child, æt. 3, the daughter of respectable persons in the middle rank of life. There was a look as if rhachitis had begun during infancy, but had not gone on. There had been for some weeks, at very irregular intervals, a convulsive attack of a severe kind, engaging nearly the whole body, and accompanied by apparent insensibility. The action of the bowels was regular; the belly was not swollen. The auscultation and percussion phenomena of the lungs and heart were normal, but there was doubtful evidence of cerebral tubercle. Four grains of KBr were administered in sweetened water every two hours, a total of thirty-two grains being consumed every day. The medicine was continued on this plan for ten days, and during this time no "fits" occurred.

The intervals between the doses of medicine were lengthened to four hours for another fortnight; again no "fits." Finally the medicine was left off entirely; but two or three convulsions of a less severe sort than formerly induced the mother to begin the same treatment again, and with equal success. The medicine has since been discontinued for a second time, and without a return of the old malady.

Like most other practitioners, I have administered KBr largely for the treatment of epilepsy. When the salt was introduced for this purpose, and after its apparent efficacy had been established, I prescribed it according to the common use, in doses varying from fifteen to thirty grains three or four times a day. The proportion of absolute relief obtained was large, and a more or less qualified relief was still more common. After a time, I took three or four cases just as they came before me, without any selection, and administered KBr in an average dose of ten grains every two hours (eight doses daily). I have no right to say that this method had any advantage over the other, or that it did any more good; and when no conspicuous advantage of any kind can be claimed, we have no ground for teasing a sick person with what so ceaselessly reminds him of his infirmity. Further, KBr is a medicine which seldom causes ill results of the physiological order, and therefore we have less plea for deviating from the usual plan of prescribing it in the management of chronic neuroses.

When it is desired to produce a pseudo-narcosis with this medicine, it is essential, I believe, to administer it in single large doses.

(IX) *The nutritional tonics,* including not only those which feed tissue, but those which promote oxidation, may be classified together. Iron has been spoken of already, and I wish to write now about *iodide of potassium, chlorate of potash,* and *cod-liver oil.* Concerning the last-mentioned drug (or food), the single conceivable reason for ever administering it in small and frequent quantities would be that only in this manner can it elude the disgusts of palate and stomach. Ten or fifteen drops might be so disguised as to escape rejection, but for this small dose to do medicinal good it must be given very often; and a reasonable critic might inquire whether, under these circumstances, cod-liver oil is likely to be useful at all.

Very rarely, again, can *iodide of potassium* be needed in more than four doses during the twenty-four hours, and each of these may be represented by twenty to thirty grains. I have had only one case of which I can say that positive and enduring benefit was derived from the administration of ten grains of the salt every two hours, after twenty grains every four hours had been taken with benefit, which, though great, was *not* enduring. It was an example of severe syphilitic headache in a woman of middle age.

There are certain cases of aphthous stomatitis, which may be connected with chronic *phthisis pulmonalis,* that are curable in an exceedingly rapid way by frequent doses of *chlorate of potash*. Of several cases which have been under my care, I may allude to one of sudden development, very painful and bad, which was entirely cured within four days by fifteen grains of the salt, in solution, being taken every two hours.[1]

Three medicines remain, each of the highest importance, and I have taken every legitimate opportunity of administering them according to the principle propounded in this essay, and of trying to ascertain its real or supposed superiority. The notes of several of these cases, however, are imperfect, and the references to others are not exact enough for my present purpose; consequently, I have submitted these medicines to a new examination, and the cases which will be quoted presently are all of recent occurrence.

(X) *Arsenic* may be administered in small and frequent doses, with the object of overcoming an unusual susceptibility to its toxical influence. A case (say of psoriasis) is presented to us for which the use of arsenic appears to be imperatively required; we prescribe it accordingly, and are vexed to discover that an ordinary dose (*e.g.* ♏v of *Liquor Arsenicalis*), taken three times a day in solution after food, causes in a little while a general eruption of so-called lichen, and perhaps swelling of the feet and hands; or there may be troublesome irritation of some of the mucous membranes.

(21) D., March, 1870.—A laundress, æt. 42, unmarried, suffering from *psoriasis guttata* over a large area of the body, but chiefly on the legs and arms. She seemed well in other respects. She consulted me first on March 26th; I prescribed five drops of *Liquor Arsenicalis* in infusion of orange peel to be taken three times a day. In a week she came again, and displayed her arms and shoulders covered with a thick and *quasi* lichen; she complained also of some neuralgia in the feet. I attributed these new symptoms to the arsenic, as no other medicine had been given, and there was nothing else to account for them. They went away spontaneously in three days. On April 6th I ordered her to take one drop of the *Liquor Arsenicalis* in plain water every two hours (eight drops daily). This plan succeeded very well; she took the arsenic in this way for seven weeks without any return of the same trouble, and the cutaneous disease improved *pari passu*.

Another case has occurred to me not unlike this.

I venture to affirm, therefore, the possibility of administering arsenic in small and frequent doses for a special disease requiring its use without the apprehension of developing poisonous symptoms. I do not say that the disease will be cured more quickly; that is

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[1] It is well worth a record that chlorate of potash is easily soluble in comparatively small quantities of boiling water, and the salt is not deposited (or only to a most limited extent) when the water cools. I wish that the value of this medicine in certain forms of *phthisis pulmonalis* was more widely recognised.

quite a different matter. I have no facts to warrant me in saying that a psoriasis will be cured in forty days instead of fifty days by the one plan of treatment rather than by the other. It is a question of safety, not of speed. It is possible that the elements of greater speed and greater safety may concur; but when they really, or apparently antagonise, we properly esteem the latter as of much higher price than the former.

Hence, when a patient can take five drops of *Liquor Arsenicalis* three times a day for a psoriasis, or an ague, or a neurosis, without suffering any obvious physiological disturbance, I see no reason for taking the medicine in any other way, such as two drops every two hours, which would be very nearly an equivalent quantity. The only conceivable apology for doing so would be the allegation that the psoriasis, or the ague, or the neurosis, is cured in a shorter time; but if this cannot be proved, the argument falls to the ground. The analogy between arsenic and antimony is, in this respect, complete. We desire to cure the disease, curable respectively by the arsenic and the antimony, without adding to it what may be called the toxic disease of the medicinal agent; and we are able to do so by the method of administering that agent in "comparatively small and frequent doses."

(XI) *Belladonna* is a remedy of great power, but of great capriciousness. I do not profess to enumerate here all its capacities for good and harm. I propose to discuss its use for a single purpose, that of alleviating or removing spasm, and to describe the best manner of administering it as an antidote to some forms of spasm.

When belladonna does good to simple and uncomplicated asthma, Dr. Hyde Salter's plan is a very efficacious one. He recommends a single large dose to be given at bedtime, in order to ward off an asthmatic attack during the hours of early morning.[1] In this manner I have given twenty-five or thirty drops of the tincture of belladonna to an asthmatic person at bedtime, with very decided success; but the great drawback is that one never knows when to expect success, nor why it comes or does not come. And further, success at one time by no means guarantees success at another.

But that more permanent condition of bronchial spasm, improperly called "bronchial asthma," is susceptible of much alleviation by the administration of belladonna in "comparatively small and frequent doses." It is one of the best medicines for this purpose given in almost any way; its unpleasantnesses are never dangerous, provided that they do not exceed a certain degree; it is immeasurably superior to opium as a remedy taken for any length of time.

(22). D., February, 1870.—A man, æt. 55, a hawker of steel

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[1] 'Lancet,' Jan. 30th, 1869.

pens. He consulted me first in the middle of February, with a category of complaints which pointed very strongly to the possibility of intra-thoracic aneurism; indeed, I examined his chest at the very outset with this hypothesis uppermost in my mind, but I discovered no substantial evidence anywhere for the existence of aneurism. Bronchial dyspnœa was the prominent trouble; there was no apparent damage of heart or of great blood-vessels. At first I prescribed ten drops of tincture of belladonna every four hours; then I increased each dose to twelve drops, still at the same intervals, four doses being taken every day. In the middle of March I began the plan of giving two drops of the tincture of belladonna every hour, from eight in the morning until eleven at night. The man complained of some dryness of throat, but no difficulty of vision, and the dyspnœa was much relieved. Simply for the sake of saving him trouble, I allowed him to take four drops of the tincture of belladonna every two hours, nine doses being taken daily (though sometimes, through forgetfulness, only seven or eight). He continued this plan for several weeks, entirely of his own accord, until permanently warm weather set in; the gradual and uninterrupted improvement was very noteworthy. No other medicine was given, nor did it seem to be required; and scarcely any physiological derangement was produced by the belladonna.

It is one of the sound traditions of the practice of physic that belladonna is a drug which may be trusted for allaying abdominal spasm and neuralgia. It is only a sincere desire to keep my essay within moderate limits that restrains me from the relation of cases in which fractional doses of aloes and belladonna, repeated every hour for a number of times, have overcome the pains and perils of acute constipation.

(XII) *Digitalis.*—I cancel some remarks in which I had attempted to give a therapeutic history of digitalis, as the researches of Dr. Fothergill and Dr. Sydney Ringer have put this subject on a new and authentic basis, which need not here be described again. I pass to the question which it is the province of this essay to discuss. May digitalis be beneficially given in comparatively small and frequent doses? and is there any advantage in this plan?

Out of several cases, more or less alike, I choose one which is typical in its pathological and therapeutic outlines.

(23) D., April, 1870.—A woman, æt. 43, wife of a cabinetmaker, in tolerably comfortable circumstances. Had been now and then under my care in past years for minor ailments. Applied to me on April 2nd, on account of palpitation of the heart; there was no murmur, but a quick weak action, with some physical signs of dilatation of the left ventricle. Health pretty good in other respects. At first I gave her iron with iodine, then alkalies with digitalis and chloric ether, afterwards quinine and belladonna. These medicines

were taken mostly three times a day, but the quinine and belladonna every two hours for a short time; no distinct benefit was realised from anything. On Easter-eve, April 16th, I determined to give digitalis alone, five drops of the tincture every two hours. On the 20th I ordered six drops every two hours. Seven drops every three hours, prescribed on the 23rd, disordered the stomach, and the digitalis was discontinued for four days. Up to this time the relief afforded to the cardiac symptoms was of a very positive kind. On the 30th I resumed the medicine in a tentative fashion; on May 4th she began again to take five drops of the tincture of digitalis every two hours, and she continued it with the intermission of only a few days until the middle of June. She speaks in the most unqualified way of the good which she has derived from the persistent use of digitalis, and the objective signs and symptoms correspond thereto.

Will any philosophic therapeutist explain what is meant by the "cumulative" action of medicines—and whether it is possible for medicines derived from the animal and vegetable kingdoms to "accumulate" in the human body? Has such "accumulation" ever been proved? Digitalis has earned a bad name for its supposed capacity to do mischief—a mischief liable to break out without note or signal, and to end, perhaps, in organic damage. Now, behold the woman whose case I have just related. She knows nothing of what is administered to her; she neither loves nor hates this drug or that, and so she takes digitalis in small and frequent doses for several weeks, not only without being poisoned, but with large and grateful benefit.

I am now studying the action of aconite, *veratrum viride,* and chloral, prescribed and given according to the method advocated in this essay.

A careful review of the subject leads us to accept tartrate of antimony and opium (including its derivative, morphia) as typical medicines in their certainty and safety. As a rule, the metallic salts are more constant in their remedial effect, this constancy being sometimes almost mechanical in its accuracy and definiteness. Being more alien to the human body than the vast majority of vegetable and animal proximate principles, an inorganic substance taken into that body resembles a stone thrown into a given bulk of water, where the ripples can be calculated with mathematical nicety, and where we can even announce when the undulating particles will subside into rest. The additional experience which nearly every day brings enables me to state that, within a definite range of quantity —one twentieth to one sixteenth of a grain—tartrate of antimony may be adminstered in solution every hour to an adult person with the infallible result (barring extremely rare contingencies) of hindering the development of an early local phlegmon. And this being so, surely it is a mistake to call tartrate of antimony by the synonym of "tartar emetic." It is a wrong thing to affix a name to a drug which indicates a single coarse property, and to bind that name to it so unalterably that its better properties are hidden, and often scarcely thought of. Do we call opium the "vegetable stupifier," or calômel the "mercurial purgative"? "In poison there is physic"—physic which is dutiful and pliant to nearly all our needs, if we handle it with delicacy and discrimination.[1]

I have attempted to open a new field of research in the history of therapeutics; I am fully conscious how imperfectly the attempt has been made. All discoveries in the art of healing are dimmed by the thought that in the old days there were some pains and weaknesses which were not alleviated and cured, and which are now controlled with certainty and ease. Even death itself has sometimes come without sufficient combat on our side to beat it off; and cases crowd on our memory about the treatment of which, if we had known then what we know now, health, and even life itself, might (God willing) have been given back. But the same cycle of regrets will have to be rehearsed in future years. Nevertheless, the science and art of medicine are so wide in their scope as to afford room for a number of inquirers of different bias. *Here* may be one man who finds delight in inventing new splints and in discovering new ways of reducing a dislocation; *there* may be another man whose faculties are directed to the improvement of the obstetric forceps, or to the abbreviation of the pains of childbirth; and *now* may the energies of many be guided to the investigation of the healing virtues of all substances which can, outwardly or inwardly, do good to mankind. That investigation must be rigid and austere, uninfluenced by superstitious tradition or partiality. The boundless field for observation and deduction ought to attract numerous workers; and in prosecuting the work there are so many prompting motives of high philanthropy that the student may become, without fanaticism, a zealot and a devotee.

Further, it must be evident that speculative therapeutics will always be in advance of the actual materia medica.

In the busy quest after new powers to conquer new diseases, or to cut short old diseases more quickly, we are tempted to assign a value to those powers which will not in every instance bear a scientific scrutiny. Too eagerly we deduce therapeutic hints from the physiological disturbance which a drug produces; too hopefully we assume that a substance has healing virtues, to which some variety of that thing called Disease may or must submit. Every fresh medicine is on its trial by a thousand hands. News of its supposed achieve-

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[1] The latest and most wondrous news about tartrate of antimony comes from India, where it has been given in heroic doses for the treatment of cholera. See pamphlet by Mr. George Barnard, Staff Surgeon, Indian Army, Calcutta, 1869.

ments has gone before, and in our constant battle with pain imagination reinforces our strength before the reinforcement comes. We crave incessantly for more help, and are deceived by therapeutic *mirages*. We believe that every human agony is destined to be mitigated or overcome; our firm silent creed is in the mission-work of doing the greatest good without in the least denying the Christian discipline of suffering.

The therapeutic method which has been elucidated in this essay is not polypharmacy, so called, nor anything akin to it. At the first glance it may seem polypharmaceutical to be repeating doses of a medicine at such frequent intervals, but the whole principle and practice of the method are built upon unity and simplicity—the repetition of a *single* medicine, administered in the simplest form. And, as a final remark, I urge that in this manner we can best study the operations of medicines; for however clearly a substance which we term a medicine works in health, it may work doubtfully in disease. Clearly and doubtfully, I mean, to our unaided understanding, arising from the very nature of the problem which we have to solve; for perfect health is a plentiful abounding thing, which we have only to go into the streets to see, and health only a few shades short of perfect is to be met with on every side. But the conditions of *not*-health are as multiform as the persons who exhibit it, and beneath all are the numberless phases of individual idiosyncrasy by which disease is modified or prolonged; and thus we hesitate sometimes from not knowing whether our weapons are weak or improper,[1] or whether they are blunted or nullified by the resistance which they meet with. Now and then we do daring things, for death faces us, and will smite us with mortal defeat if we falter or look back. Every fresh case of illness is a new labyrinth to unravel, unlike in its combination and permutation of tracks all other labyrinths that we have gone through before. How grateful we should be when Nature grants us an Ariadne to help us out!

The thesis of my essay includes "things new and old;" the old things I have endeavoured to establish on a surer basis, and the new things are the fruit of personal experience, which remains now to be tested by the higher and broader experience of others. I have not claimed for the practice of administering medicine in small and frequent doses a wider application than appears legitimately to belong to it. I plead simply for a *systematic* recognition of it at appropriate times and under appropriate circumstances; I desire that it

[1] That cry of scepticism about drugs which we heard some years ago was the wail of ignorance, not of knowledge. It was the wail of that acute impatience which will not sit down to plain hard work and test, step by step, the fitness of the tools it uses. Whether a patient lived or died it might be the doctor's doings, but, perhaps, it was all chance:

"Casus medicusve levârit
Ægrum ex precipiti."—'Hor. Sat.,' lib. ii, 3, v. 292.

may be used with confidence as one of the authentic methods of medical art. If I am not greatly mistaken in my interpretation of facts, the phenomena of inflammation in certain tissues admit, in the early stage, of complete control by this method; the phenomena are arrested, and their pathological evolution stopped. Further, it has been attempted to show that there are particular phases of chronic disorder or disease susceptible of alleviation by the same plan. Let the axiom ever be borne in mind that usually the most potent remedies are the most useful. A distinguished medical writer[1] asks us not to be *afraid of power;* and I know nothing which more satisfactorily proves mastery of craft than the wielding of strong weapons of medicine as if we were not terrified by them, as if we had dominion over them. It has been one of the purposes of this essay to show how therapeutic and even toxic power may be guided to beneficial ends.

The scope of subject embraced by this essay is not very wide; but the principle selected for discussion and illustration appears capable of becoming a scientific law, which may promote the highest interests of the art of therapeutics, and so, indirectly, enhance the "order and beauty which we see in the world."[2]

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[1] Dr. Barnes, 'Lectures on Obstetric Operations,' p. 22.
[2] Sir Isaac Newton's 'Opticks,' Query 28.

Chronicle of Medical Science.

REPORT ON OBSTETRICS AND GYNÆCOLOGY.

By W. S. Playfair, M.D., F.R.C.P.

I.—The Non-Pregnant State.

1. *One-sided Hæmatometra in two cases of Double Uterus.* By Dr. Ludwig Neugebauer.
2. *On the Sulpho-carbolates as Gynæcological Agents.* By Dr. Mack.
3. *On the Mechanical Treatment of Displacements of the Unimpregnated Uterus.* By Dr. Pepper.
4. *The Treatment of Fibroid Tumours of the Uterus.* By Dr. Meadows.
   Uterine Fibroid Tumours and Polypi, their Pathology and Removal. By Dr. Skinner.
5. *Sudden Death in Patients suffering from Cancer.* By Dr. Barnes.
6. *Restoration of the Perinæum.* By Dr. Matthews Duncan.
7. *The Value of Arsenic in Menorrhagia and Leucorrhea.* By Dr. Aveling.
8. *On Herpetic Eruptions of the Uterus.* By Dr. Noel Gueneau de Mussy.
9. *Dilatation of the Cervix Uteri by Graduated Bougies.* By Dr. Arthur Edis.
10. *Termination of the Nerves in the Vagina.* By M. Chrschtschonovitsch.
11. *Diffuse Ovarian Fibroid of peculiar construction.* By Professor Waldeyer.

1. Dr. Ludwig Neugebauer narrates two instances. In the first menstruation began at seventeen, and was very painful. It reappeared five times, a tumour, the size of a fist, being felt above the pubis, which increased monthly. Amenorrhœa then occurred for nearly two years, with complete absence of pain. On the reappearance of the catamenia pain returned, the tumour on the right extending nearly to the umbilicus. It was opened per vaginam by means of a pointed bistoury, and the patient went on well for fourteen days, when, on the return of the period, acute peritonitis set in, and the patient succumbed in three days. No autopsy allowed.

In the second the tumour was not so large, and was opened by pushing the uterine sound through the lower portion of the swelling. The patient succumbed on the forty-eighth day. No autopsy.

The author gives references to nineteen similar cases; the ages varied from fourteen to twenty-seven; the right side was more frequently involved. In four no operation was resorted to. In fifteen operations, small and large incisions, puncture with gradual dilatation, &c., were tried; of these eight recovered and seven died. The prognosis is bad if nothing be done, but even by operation 50 per cent. die. He advocates large free incision, or, better still, galvano-caustic, to prevent infection.—*Archiv fur Gynäkol.*, ii, 2, p. 246, 1871.

2. Dr. Mack has employed the sulpho-carbolate of zinc with great advantage. He uses it as an injection, half a drachm of the salt to a pint of tepid water. As an intra-uterine application, he had applied it both by injection and with a mop; and it was also beneficial in chronic vaginitis attended with fetid discharge, being used in solution with glycerine, in doses of ten grains at a time, applied with a tampon. — *Journal of Gynæological Society of Boston*, August, 1871.

3. Dr. Pepper publishes a lengthy paper on displacement of the uterus, in which he details the anatomy of the uterine organs, in so far as it bears on the subject, and describes the various forms of uterine supports, and the indications for their use.

His general conclusions are summed up as follows:

"(1) Cases undoubtedly occur in which all, or nearly all, the symptoms are due to uterine malpositions, although these may be associated with other disease; and when these displacements are corrected, a cure may be said to have been effected, so far as the painful sensations are concerned, leaving the residuary disease to be treated and removed at leisure.

(2) In other instances uncomplicated malpositions are capable of giving rise to the most severe local and general disturbances, and their correction will completely restore the patient at once to health and comfort.

(3) Cases occur of displacement coexisting with inflammatory or other diseases, in which, if, from the irremediable nature of the malpositions, or from other considerations, the entire attention be directed to the removal or mitigation of the coexisting trouble, all symptoms will disappear, although the displacement remains as marked as at first.

(4) Cases occur in which inflammatory disease as a result, is associated with uterine displacement, and although skilful treatment be directed against the former condition, no relief can be obtained until the malposition is rectified, when the inflammation will subside spontaneously.

(5) And, lastly, I am satisfied, from repeated observation, that cases occur in which inflammation and displacement coexist, and that the removal of the inflammatory hypertrophy will, by lessening the weight of the uterus, cause the malposition to be gradually corrected.

Hence it would appear that some mechanical contrivances for correcting uterine malpositions play an absolutely essential, though by no means a universally applicable part, in our successful treatment of uterine diseases.—*American Journal of Obstetrics*, August, 1871."

4. The chief object of these papers is to advocate a more frequent

resort to surgical treatment. Dr. Meadows recommends a more general use of those methods of dealing with fibroids which have been so ably advocated and carried into practice by Dr. Matthews Duncan. Dr. Skinner, while admitting their usefulness, advises caution as to their adoption, and states that the only case in which he attempted enucleation on Duncan's plan terminated fatally.—*Brit. Med. Journ.*, Aug. 26th, 1871, and *Liverpool Med. and Surg. Reports.*

[Both authors put themselves to some pains to prove that we possess no therapeutical agent which will effect the removal of these growths. We are not aware that any one has so stated. What has been said is that nature, in some rare cases, effects spontaneously what art is powerless to accomplish. It is, no doubt, difficult for any one to believe in a confessedly exceptional occurrence which he himself does not happen to have seen, but that does not prove that others who have reported such cases are mistaken. The reporter has published some in a paper "On the Spontaneous Absorption of Fibroid Tumours of the Uterus," and since that several others, minutely observed, have been narrated by Matthews Duncan, Sedgwick, Spencer Wells, Scanzoni, and others, so that the fact seems beyond question. Unless Dr. Skinner considers nothing evidence that he has not himself seen, he can scarcely be justified in saying, "I have never come across a particle of evidence to lead me to the conclusion that so much as one grain of a fibroid tumour ever was absorbed by a natural process."—W. S. P.]

5. In a note on this subject, Dr. Barnes refers to Dr. Todd's opinion that in some of these cases the death may be due to embolism, from small portions of the cancerous growths invading the blood-vessels, and subsequently being swept into the circulation. He has, however, searched without success for these cancer emboli in cases in which the final symptoms seemed to point to pulmonary embolism. In a patient who died suddenly in St. Thomas's Hospital death seems to have resulted from an altogether different mechanism. Extensive secondary deposits were found in the abdomen. These so surrounded the aorta, "that it was no longer an elastic tube, expanding under the heart's systole and then contracting, but a rigid tunnel, utterly wanting in resiliency, and with its interior no longer smooth, but distorted by irregular projections. Such an aorta, accompanied by a vena cava similarly affected, is mechanically unfitted to do its work. Under very moderately increased emotion or exertion, causing unusual action in the heart, this rigid tube would throw back upon the heart a portion of the column of blood which the aorta ought to receive and propel. This retrograde dynamic disturbance would overwhelm the feeble heart, and thus death would follow.—*Brit. Med. Jour.*, July 16th.

6. Dr. Duncan details the indications for operation, either after laceration of the perinæum or in cases of procidentia. His method of operating consists in transfixing the fourchette, the patient being in the lithotomy position, and then cutting upwards on each side, removing a long tape-like piece of integument rather less than half an inch in breadth. The raw surfaces are brought closely into

apposition by a number of silver wires after all oozing has ceased. Dr. Duncan speaks favorably of the success of his operation.—*Edin. Med. Journ.*, November, 1871.

7. The author had administered the remedy for twelve years in cases of menorrhagia with great success. Besides the improvement it effected in the general health, he believed it to possess a powerful influence in lessening congestion in mucous membranes. He confidently recommends it in all disorders of the uterus having a hyperæmic origin. He administers small doses, either in solution or granules, increasing them from time to time, and continuing them for weeks or months as the necessities of the case requires.—*Brit. Med. Journ.*, August 26th, 1871.

8. Dr. De Mussy writes a lengthy essay on the constitutional origin of those diseases of the uterus which have been variously described as uterine catarrh, granulations, erosions, or ulcerations of the cervix uteri. These he believes to be chiefly due to a diathetic condition similar to that which causes various skin eruptions, and closely allied to the arthritic diathesis. The peculiar form of the eruptions he believes to be modified according to their site, whether on skin or mucous membranes. He points to the frequency with which erythematous and eczematous eruptions are met with about the vulva in cases of chronic and severe uterine catarrh as confirming his views, and relates cases in which bronchitis and muscular rheumatism, due to the same diathetic state, alternated with the uterine affection. The symptoms described by Dr. De Mussy are precisely those familiar in some cases of erosion and granular disease of the cervix. The author then proceeds to describe the various eruptions which he believes he has recognised on the cervix uteri, such as the vesicular, papular, &c. In long-standing cases these become modified, and merge into the condition generally described as granular erosion of the cervix. Granulations of the cervix he believes to arise from tumefaction both of papillæ and follicles of the cervix. He next proceeds to describe the treatment he adopts, both constitutional and local. He is opposed to intra-uterine medication.—*Archives Générales de Médecine*, October and November, 1871.

9. Dr. Edis advocates the gradual dilation of the cervix some time before the commencement of menstruation, by sounds varying in size from that of a No. 8 to a No. 12 catheter. He considers this treatment preferable to the use of any form of intra-uterine stem in cases of anteflexion causing painful menstruation.—*Brit. Med. Journ.*, November 4th, 1871.

10. M. Chrschtschonovitsch has recently read a paper on this subject before the Academy of Sciences at Vienna. The animals examined were man, the rat, dog, and rabbit, the latter being the best for purposes of demonstration. The results arrived at were as follows:—The mucous membrane of the vagina of all animals contained numerous branched cells, resembling connective-tissue-corpuscles, arranged just beneath the laminated pavement epithelium with almost the same regularity as in the cornea, whilst others are scattered irregularly through the membrane. The nerves penetrating

through the muscular layer contain large bunches of medullated fibres, which here and there contain ganglion-cells. From these smaller trunks are given off, which enter the proper mucosa, or rete Malpighii; as these press towards the surface the medullated sheath is gradually lost, and the fibres either bend back or join with a fibril from a neighbouring trunk, forming a very superficial plexus, or apply themselves to the wall of one of the small vessels ascending to supply the papillæ of the membrane. Some few of them, destitute of mamillary sheaths, may be seen ascending between the epithelial cells, but do not join with the branched connective corpuscles mentioned above. The smooth muscular-fibre fasciculi of the membrane are surrounded by a very rich plexus of non-medullated nerve-fibres, from which individual fibres are given off, that exhibit here and there granule-like enlargements, and penetrate between the several muscle cells. He thinks it probable that these last are encircled by the ultimate fibrils, but has not been able to find any closer relation to the surfaces of the muscle, as stated to occur by Klebs and others.—*Lancet*, November 25th, 1871.

11. Professor Waldeyer, of Breslau, narrates a case of the above. It was about six by four and a half inches large, and weighed a little over two pounds. It was so hard that it was difficult to make a section, almost brittle, and resembled spongy bone or osteoid tumour of the upper jaw. It wanted only the peculiar grouping of the cells on the epithelial-like borders of the trabeculæ, as in osteoblastema, and the solid homogeneous condition of the trabecular tissue itself, in order to constitute a complete resemblance of the tumour in microscopic appearance with the osteoid tumour.—*Arch. f. Gyn.*, ii, 3, 1871.

II.—Pregnancy.

1. *Exfoliation of the Bladder.* By T. Spencer Wells, F.R.C.S. *Exfoliation of the Bladder.* By Dr. Whitehead.
2. *Obliquely Distorted Pelvis.* By Dr. Spiegelberg.
3. *Conception under unusual Circumstances.* By Dr. Olshausen.
4. *Well-marked Anæmia in Pregnant Women.* By Professor Gusserow.
5. *Quadruplets.* By Dr. Leopold.
6. *Case of Twin Compound Conception and Removal of Extra-uterine Fœtus after Four Years.* By Mr. Bleach.
7. *The Vomiting of Pregnancy.* By Dr. Hubert.
8. *A Case of Double Uterus with Concurrent Pregnancies.* By Dr. J. Harris Ross.
9. *On Calcification of the Placenta.* By Dr. Frankel.
10. *The Amnion in relation to Fœtal Malpositions.* By Dr. Fürst.
11. *Cysts of the Placenta.* By M. Jacquet.

1. Mr. Wells reports two cases of this interesting accident; both occurred after severe cystitis following delivery.

Mr. Whitehead describes one from the notes of Mr. Clement Godson. In this case the pregnant uterus was retroverted, thus impeding the emptying of the bladder, and confirming the suggestion of Dr. Phillips, as to retroversion being one of the probable causes.

"The cysts," Mr. Whitehead observes, "cannot be looked upon as simply exfoliations of the mucous membrane of the bladder, as they do not consist entirely of mucous membrane. Muscular fibre, and even serous tissue, are often, if not generally, attached to and incorporated with the mucous lining; and, further, we have the distinct fibrinous casts."—*Brit. Med. Journ.*, July 1st and October 14th.

2. Professor Spiegelberg gives the details of two cases in a most elaborate paper. In the first it was right-sided, with synostosis of the sacro-iliac synchondrosis and shortening of the right leg, from a fall at three years old. In the second inflammation of the right sacro-iliac synchondrosis and secondary atrophy of the pelvis, without synostosis, resulted. In these two cases the author ascribes, as the principal cause—

1st. The increased pressure upon one half of the pelvis.

2nd. Atrophy of the bones forming the ilio-sacral joint.

Either of these may be the original cause, the other following as a consequence.—*Arch. f. Gyn.*, ii, 2, 1871.

3. Dr. Olshausen, of Halle, narrates two curious instances of conception occurring during the presence of an intra-uterine stem.

In one case menorrhagia with anteflexion existed in a multipara. The stem was inserted on June 28th, and removed January 15th. She was delivered on the 25th September, 253 days from the time of its removal, of an extremely vigorous boy, the largest she had ever borne. Nausea, vomiting, &c., had existed during the latter half of December and in January.

In the second case marked anteflexion occurred after the second labour, and an intra-uterine stem was worn for nine months. The catamenia ceased on September 8th, and the stem was removed October 18th. She was confined, on the 24th June, of a vigorous girl, over eight pounds in weight, 249 days only from the removal of the stem.

In the first instance the ovum was probably twenty-seven days in utero, and in the second twenty days, before the stem was withdrawn.—*Archiv f. Gynækol.*, p. 278, 1871.

4. Professor Gusserow, of Zurich, gives the details of five cases of the above. They all occurred in young persons between twenty-four and thirty-six years old, who had always enjoyed good health previously; four had already borne children. The normal condition of the blood during pregnancy seems, in these cases, to have been greatly intensified; premature delivery at the height of the disease, about the eighth month, occurring, and death rapidly ensuing in each case. In three cases transfusion was employed without success, as also internal remedies, with good diet, wine, &c.

The author advises, in similar cases, the induction of abortion, or premature labour and transfusion if necessary.

The organs generally were in a very anæmic condition, but no well-ascertained cause for this could be detected, the fatty degeneration being apparently the effect and not the cause, the hydræmic condition being also secondary; in fact, an increased pathological degree of the physiological condition of pregnancy.—*Archiv f. Gynaekol.*, ii, 2, 1871.

5. Dr. Leopold, of Glauchan, narrates an instance of the above occurring in a multipara at the seventh month. The first child presented by the breech; the second was a cross birth, and necessitated turning, as also the third; the fourth was a head presentation. They were all born within an hour. Flooding occurred, but was arrested on expulsion of a very large placenta, with two cords proceeding from its margin. Two separate smaller placentæ were subsequently expelled, each with its separate bag of membranes; only one bag existed in the large one. The children were all female, living, well formed; one died on the twelfth day, the others on the twentieth and twenty-first day. The mother made a good recovery. Dr. Leopold asserts that out of 20,000 births three cases of quadruplets have occurred, whereas of 152,395 births observed in Dublin only one case is recorded.—*Arch. f. Gynaekol.*, ii, 2, 285, 1871.

6. Mr. Beach relates an interesting and unusual case. The patient aborted about the sixth week of pregnancy, but, her abdomen continuing to enlarge, it was presumed that she had been pregnant of twins, the other still remaining in utero. Symptoms of labour came on at term, but soon passed off. Menstruation was re-established, and the abdominal tumour remained unaltered. Four years afterwards, extra-uterine pregnancy having been diagnosed, an attempt was made to remove the fœtus by abdominal section, but was abandoned. A fistula formed at the site of the wound, through which the fœtus could be felt. This was enlarged, and "a partially decomposed fœtus, weighing about seven pounds," was removed. The patient made a perfect recovery. — *Journal of the Gynæcological Society of Boston*, August, 1871.

7. Dr. Hubert believes that in certain cases the vomiting of pregnancy may be traced to undue mobility or displacement of the uterus, and proposes to treat it by producing immobility of the organ by suitable bandages and the use of Hodge's or Zwancke's pessary.—*Lyon. Médicale*, October 15th.

[Dr. Hubert seems to be ignorant of the fact that Dr. Graily Hewitt has already advanced a similar theory.—W. S. P.]

8. Dr. Ross contributes a case of extreme interest, in which pregnancy occurred in both sides of a double uterus. The patient was thirty-eight years of age, and had had six children, nothing peculiar having been noted with regard to her confinements. On July 16th, 1870, she miscarried of twins, apparently between the fifth and sixth month. During the labour a second opening was felt close to the os uteri, but its precise nature was not then made out. About a week after the labour she declared that she was then pregnant, and the presence of another child was made out. She was delivered of a healthy female child on October 31st, fifteen weeks and two days after miscarrying with twins. After recovery a complete examination was made. Two openings were found in front of the cervix

"Upon introducing the sound into the upper or right aperture, it passed quite freely into a cavity two inches and a half deep to the summit, where it was cautiously moved in every direction with the view of discovering any communication that might exist between this and the supposed adjoining cavity, and on withdrawing the instrument the same caution was exercised, and for the same purpose, but no such outlet or passage could be detected. The sound was then passed into the lower or left aperture, and traversed quite easily a cavity two inches and a quarter deep only, nearly parallel to the preceding cavity. Thus, between the two cavities there was a vertical partition or septum extending from the fundus to the front of the cervix uteri, and completely dividing the latter into two separate or distinct cavities." A curious corroborative proof of the concurrent impregnation in two distinct uterine cavities was, that the patient had menstruated (evidently from the uterus which had thrown off its contents) three times beeween her miscarriage in July and her delivery in October.—*Lancet*, August 5th, 1871.

9. Dr. Frankel, in an able paper, gives us the result of his own and others' observations. 1. Calcification of the placenta follows—*a*, the course of the capillaries and small villi, and this is by far its most frequent course; *b*, it is more diffuse and intermediate; then, according to Langhaus, it takes its origin from the epithelial layer of the trunk of the villi. 2. The more frequent capillary incrustation proceeds from the wall of the vessel, spreads on all sides, and forms, as it were, a mantle round the tube of the vessels, without making them, however, completely impervious; it begins generally, not always, in the end of the villi, and may also appear in separate points in the trunk. 3. Diffuse, not too extended, calcification is without influence upon the nourishment of the fœtus, whilst even in less extended capillary petrefaction, especially in the earlier months of the pregnancy, the fœtal blood and gas changes are prevented, and this may lead primarily to the death of the fœtus. 4. The calcification of the mature placenta is only an expression of the completed intra-uterine growth of the fœtus. 5. We must regard from a similar point of view secondary calcification where the fœtus is withered and macerated from other causes.—*Arch. für Gyn.*, ii, 3, 373.

10. Dr. Livius Fürst, of Leipsic, narrates two cases. In place of thread, string, or membranous formations of the amnion, he desires to call them all by one common name—filamentous adnexa. He describes the three theories which are set up for the explanation of the development of these adnexa. 1. Plastic adhesions, which have no relation with inflammation, but which are produced by the formative power of the embryonic cells. 2. Fœtal inflammation with plastic exudation. It is well known that we find this in the tissues as well as in the cavities and on the surface of the skin of the fœtus; and everywhere, where the plexus of vessels forms the basis, we find hyperæmia, exudation, suppuration, and new formations, from normal as well as pathological tissues. 3. The third theory is that which assumes, as a cause of the filamentous adnexa, obstructed formation

of the amnion; this is specially advocated by C. and G. Braun, whilst they point out abnormal folding and progressive pathological metamorphosis as the cause of the malformation. According to them threads arise which hinder the development of the extremities, either partly or entirely. The most frequent cause of the process is the small quantity of liquor amnii, or too late secretion of it, which retards the separation of the amnion from the fœtus, and leads to adhesions. The author agrees with this latter theory.—*Arch. f. Gyn.*, ii, 3, 315.

11. M. Jacquet describes the various forms of cysts that may be observed in connection with the placenta, especially some that he describes as found between the placental villosities, varying in size from that of a pea to that of a filbert. They seem to be connected with the vascular ramifications of the organ, and Jacquet conceives them to be formed from the perivascular sheaths; hence he proposes to call them perivascular cysts. He recognises four varieties of cysts in connection with the placenta:—1. Gelatinous cysts developed from the tissue existing between the chorion and amnion, which have been particularly described by M. Millet. 2. The above-mentioned formed from the perivascular sheaths. 3. Blood-cysts, specially described by M. Bustaniente, and which probably are developed in connection with the placental sinuses. 4. The well-known cysts of hydatiginiform degeneration of the placenta.—*Gaz. Médicale de Paris*, October 14th, 1871.

III.—Labour.

1. *Conjoint-Extra and Intra-Uterine Fœtation; Cæsarean Section; both Children saved.* By Dr. E. P. Sale.
2. *On Catheterization of the Uterus.* By Professor Valenta.
3. *Impaction as a Cause of Vesico-vaginal Fistula.* By Dr. S. C. Busey.
4. *Observations on Craniotomy.* By Dr. Carl Rokitansky.
5. *The Management of the Perinæum during Labour.* By Dr. Hart.
6. *On Inertia of the Uterus from the presence of Fibroid Tumour.* By M. Depaul.
7. *On the Value of the Artificial Induction of Premature Labour in Contracted Pelvis.* By Dr. Litzman.
8. *On the proper Management of Tedious Labour.* By Dr. Hamilton, of Falkirk.

1. This interesting case occurred in an unmarried negress, in whom the existence of intra-uterine pregnaney was diagnosed. The symptoms being urgent, gastrotomy was determined on. A living child, with placenta, was removed, when the uterus was found to be enlarged and to contain a second child, a complication not previously suspected. After consultation hysterotomy was performed, and a second living child and placenta removed. The mother died on the fourth day from septicæmia. No post-mortem was allowed. Both children survived.—*New Orleans Journ. of Medicine.*

2. Professor Valenta, of Laibach, recommends, after ten years' experience, the introduction of the catheter as a harmless method of increasing and exciting uterine pains.

He gives details of sixty-eight cases. He advises the use of an English elastic catheter, passed to the left and posteriorly. He says six and a quarter hours are enough to regulate the pains until the os uteri is dilated from two to three inches; if to accelerate labour, it should be left in until the os is fully dilated. It is the best method of inducing abortion and premature labour. It can be adopted whilst other measures are also employed, as in placenta prævia, with the tampon as well. In primiparæ it is better before the pains have commenced, in multiparæ after. In deviations of the uterus the stillette can also be used.—*Wien. Med. Presse*, 23—39.

3. Dr. Busey analyses the particulars of sixty-five cases of vesico-vaginal fistulæ reported by Dr. Emmett, from which he draws the now pretty generally admitted conclusion, "that though instruments were employed in very many of the cases, it is perfectly apparent that the error was in not having resorted to artificial means sooner. A curious corroborative proof of this opinion, and of the fact that vesico-vaginal fistula has a direct relation to impaction and protracted labour, is that out of sixty-five cases no less than fifty of the children were stillborn.—*Amer. Journ. of Obstetrics*, August, 1871.

4. Dr. Karl Rokitansky, chief assistant in the Lying-in Hospital at Vienna, states that in ten years 52,394 births occurred, and of these 103 were craniotomy cases, or 1 in 508·7, eighty-six being head presentations. He discusses the merits of Braun's modification of Simpson's instrument, which is stronger and longer than Simpson's original. 1st, its total length being seventeen inches eight lines; 2nd, there is a compression apparatus at the end; 3rd, the ends, when the instrument is closed, remain nine lines apart: 4th, at the upper end are hooks on either side, to facilitate traction.

In each case perforation was first effected with Braun's trepan.

Of the 103, forty-one died—a result less successful than in England.

He concludes that the cranioclast can be used in much more contracted pelves than the cephalotribe, and also more readily where the head follows the body. Excerebration is best performed by Braun's trepan.—*Wien. Med. Press*, xii, 8—19.

5. Dr. Hart's paper is written with the view of confirming Dr. Goodell's views on supporting the perinæum, reported in the July number of the 'Med.-Chir. Review;' but the author does not agree with him in his recommendation of hooking up the sphincter ani with his fingers and pulling it towards the pubes, so as to relax the perinæum. After discussing this point he suggests the following rules:

(1) So long as the sphincter ani is not freely dilated a resort to the bowel for the purpose of adjusting the relations of the fœtal head to the maternal soft parts is not indicated.

(2) If the anal sphincter is freely dilated, and if at each recurrence of the pain the posterior margin of the perinæum protrudes

beyond the vulva, pass one or two fingers of either hand (as the position of the patient may indicate) into the rectum, and with their palmar surface press the fœtal head forward towards the vulva, and at the same time press the fourchette gently backwards, either with the thumb or the fingers of the other hand, so as to bring the vulva as nearly as possible within the axis of the distending force.

(3) When it is evident that the head is advancing too rapidly for the safety of the parts, plant the thumb and fingers against it, so as to check farther progress until the pain has subsided, and then proceed to rectify the position, or to enucleate the head if it be practicable or expedient to do so.—*St. Louis Med. and Surg. Journ.*, No. 3, 1871.

6. The author narrates a case of labour in which there was complete inertia, due, it was believed, to the presence of an interstitial fibroid interfering with the action of the muscular tissue of the uterus, and he believes this to be a prominent danger when such growths exist, even when they do not encroach upon the true pelvis. —*Gaz. Méd. de Paris*, August 12th.

7. Dr. Litzman concludes that the induction of premature labour is not to be recommended, as the child is either born dead or survives only a few days, whilst the danger to the mother is increased, for though the child is smaller the placenta is more firmly attached, and the risk of puerperal complications is increased, especially when we consider the bruising that ensues from artificial interference.

He gives the details of 373 cases, all grades included.

By natural premature labour the maternal mortality is only 6·5 per cent., whilst in artificially induced it is 14·7 per cent. More living children are born by the latter method, but they die in a few days.

He gives four groups of distorted pelves:

1st.—*a*. Uniformly contracted pelvis, with conjugate diameter of 9 to 10 centimètres. *b*. Uniformly contracted, with conjugate diameter specially narrowed (9·5 to 8·25 centimètres).

2nd. Uniformly contracted pelvis, with conjugate diameter of 9 centimètres.

3rd. Simply contracted and generally narrowed pelvis, with a conjugate diameter of 7·3 to 5·5.

4th. Ditto 5·4, where Cæsarean section is necessary.—*Archiv f. Gynaekol*, ii, 2, 1871.

8. Dr. Hamilton contributes a very valuable paper, which is, in the main, an extension of a former paper of his on the same subject published in this Journal in 1853. Dr. Hamilton is, as is well known, one of the most prominent advocates of a frequent and early use of the forceps in the second stage. His results are most remarkable, and claim careful consideration. Up to December, 1860, he had in his own practice 731 consecutive births without a stillborn child. Since that he has lost only one other child, and that a footling case. "*In other words, that every head presentation in both series has yielded a living child.*"

"This," says Dr. Hamilton, "is such an extraordinary departure from anything that I know of in the history of obstetrics, and presents such a serious view of the possible sacrifice of human life that may have hitherto been going on in this department of our profession, that I am sure my professional brethren will be not unwilling to hear from me how I think I am able to explain the very opposite results that have been obtained by me compared with those of many other professional gentlemen, for whom I entertain the highest respect."

With the view of still further showing the success of his practice, Dr. Hamilton then refers to the annual reports of two well-known lying-in charities, in one of which the infantile mortality ranges from one in five to one in twenty-nine, in the other from one in eleven to one in twenty-seven.

Dr. Hamilton rarely or never interferes with the first stage of labour. He attaches great importance to having the os uteri fully dilated before applying the forceps, and with rare exception he never applies them *within* the uterus. He approves of gentle but firm manual dilation of the os in the second stage, if the head has not passed through it, and has never seen any ill effects follow this practice, and has never met with a case of laceration of the neck of the uterus. "It is with the commencement of the second stage of labour that our active interference should generally begin, if we be called upon to interfere at all. If the membranes have not been ruptured and the head is presenting fairly, I then do so at once, and gently, but firmly, continue to press up the uterus with one or two fingers, or with the whole hand if necessary, until I have got it pushed over the head; and, as previously observed, I like to use the forceps as seldom as possible before this has been effected."

Dr. Hamilton holds that it is not safe to the child to allow the labour to continue more than two hours after the head is in the pelvic cavity.

With regard to the method of application, he never, if possible, applies the forceps except over the ear. He never uses a forceps with a double curve, and he never greases the instrument before introduction. He prefers not to use chloroform, as he likes to have the full power of the uterus, and to use traction to assist the natural labour-pains. (*To be continued.*)—*Med.-Chir. Rev.*, October, 1871.

[The more frequent use of the forceps in the second stage is now becoming much more prevalent. Dr. Hamilton's views as to the manual dilation of the os will, doubtless, not be accepted without considerable hesitation. The necessity of feeling the ear is also very open to question, and, as Dr. Barnes has well shown, and as German practitioners have always taught, it seems to be neither necessary nor advisable. Dr. Hamilton's results, however, are quite unique, and speak in the strongest terms in favour of his views.—W. S. P.]

REPORT ON PHYSIOLOGICAL AND PATHOLOGICAL CHEMISTRY.

By Professor A. H. Church, M.A.

Mineral Constituents of Food.—The ash or mineral matter of food has scarcely received that attentive chemical study which its important physiological bearings demand. Messrs. Lawes and Gilbert, and other experimenters as well, have shown how necessary it is in feeding experiments on animals to supply phosphoric acid and certain bases when starch, gelatine, sugar, and other proximate principles freed from ash, are given to animals. Karl Voit has just made some important experiments in this direction, especially with regard to the relations of acids and bases. He proves that compensations and transferences of many kinds occur in the organism when the composition of the ash of the food does not correspond exactly with the mineral requirements of the blood or organs. When an animal is fed on bran (which leaves an acid ash) the excess of phosphoric acid appears in the urine, while the blood is not less alkaline than usual. The reaction of the ash of the urine is, in fact, either acid or alkaline, in accordance with the acidity or alkalinity of the ash of the food. Voit caused some experiments to be made also on the source of the alkalies in the blood of the chick, and from these experiments it appeared that the white of the egg furnished these alkalies, and in such quantity that, though the ash of the yolk of the egg is acid, the ash of the white and yolk together is alkaline. —*N. Report. Pharm.*, xx, S. 422.

Extract of Meat.—Some of the recent experiments on the nutritive value of the *Extractum carnis* do not give a high estimate of its physiological importance. Herr G. Bunge, who has been lately investigating this subject, under the direction of Dr. Schmiedeberg, of Dorpat, assigns a lower value (as a nutrient material) to the extract than to coffee, tea, or alcohol.—*Archiv fur die gesammte Physiologie*, Bd. iv.

Easy Formation of Blood-crystals.—To an aqueous solution of blood add just enough nitrate of silver to remove the chlorine; agitate and filter. Then introduce an equal volume of pure ether and a little pure acetic acid. The materials having been shaken together, the ether takes up a peculiar colouring matter (doubtless an alteration product), which exhibits four absorption bands, and when evaporated slowly yields abundance of needle-shaped crystals, which, however, seem somewhat different from those previously studied.—Dr. Preyer, *Chem. Centralblatt,* No. 7, 1871.

On Hæmatin.—Hæmatin differs but little in appearance from hæmin, from which it is best obtained. To hæmatin is given the formula $C_{68}H_{70}N_8Fe_2O_{10}$, while hæmin, which is the hydrochlorate of hæmatin, may be represented by the expression $C_{68}H_{72}N_8Fe_2O_{10}Cl_2$.

By the action of sulphuric acid on hæmatin in the presence of water and oxygen, the iron combines with the sulphuric acid, and a substance soluble both in sulphuric acid and caustic potash is obtained. To this substance is assigned the formula $C_{68}H_{74}N_8O_{12}$; it is called hæmatoporphyrin. When hæmatin is acted on by sulphuric acid in sealed tubes, a substance insoluble in caustic potash, and only slightly soluble in sulphuric acid, is produced. It has been called hæmatolin, and has the formula $C_{68}H_{78}N_8O_7$. When a solution of hæmoglobulin is reduced by hydrogen, and decomposed by alcohol containing sulphuric acid or caustic potash in absence of oxygen, a colouring matter is produced characterised by definite absorption bands. It is called by Hoppe-Seyler hæmochromogen. On oxidation it forms hæmatin. Bilirubin seems closely related to hæmochromogen, and is probably formed from it by the action of water and an acid in absence of oxygen.—F. Hoppe-Seyler, *Med. Chem. Untersuch.*, 1871, p. 523.

Blood and Urine in Chyluria.—The urine was milky white in appearance, and contained over 0·7 per cent. of fat. The whole blood contained 0·17 per cent. of fat, and the serum 0·359 per cent.—Hoppe-Seyler, *Med. Chem. Unters.*, 1871, p. 551.

Action of Carbonic Oxide on Blood-globules.—Blood absorbs carbonic oxide in preference to oxygen, and as the affinity is greater the oxide cannot be expelled by the oxygen. The hæmoglobulin of the blood has a great affinity for carbonic oxide, and forms with it a true chemical compound, which is very stable. In cases of poisoning with carbonic oxide, with proper precautions the gas can be extracted from the blood, and its presence accurately determined. As carbonic oxide preserves blood, Bernard proposes to use it as a means of preserving meat. The experiments, however, already tried in this direction are not very promising.—Claude Bernard, *J. Pharm.* (4), xiii, 255.

Earthy Matter in a Human Lung.—In the lung of a workman who was exposed to the dust of a mixture used in the preparation of ultramarine 3·193 grms. of silicate of alumina, 0·33 grm. of quartz sand, and 0·329 grm. of iron oxide were found, in 227 grms. of lung. Estimating the weight of the lungs at 1500 grms., they would contain 29·86 grms. of earthy matter.

In the lung of a woman who had inhaled the dust of iron oxide used in preparing books for gold leaf Gorup-Besanez found 0·828 grms. of iron oxide in 57 grms. of lung, or equal to about from 21 to 22 grms. for the whole lung.—E. von Gorup Besanez, *Ann. Chem. Pharm.*, clvii, 287.

Variations in the Size of the Red Blood-corpuscle.—Nearly all the conditions which increase the temperature of the body diminish the size of the blood-corpuscles. Poisoning an animal by the injection of putrid matter into its vessels, compelling it to breathe an atmosphere highly charged with carbonic acid gas, or submitting it to an increased temperature, are amongst the causes which produce the above-named effect. The administration of oxygen, prussic acid, hydrochlorate of quinine, or alcohol, produced, like the application of cold, an increase in the size of the blood-corpuscles.—Dr. Manassein, *Chem. Centrablatt*, No. 44, 1871.

Blood and Urine in Leukhæmia.—In the urine urea and uric acid were abundant, and nearly in their normal proportions, the amount of uric acid being slightly increased. The urine was acid—no albumen present. No hypoxanthine could be detected. In a portion of leukhæmic blood, after the albumen was separated, gelatine and acetic acid were found. Another sample of blood, about thirty grms., yielded 0·123 grm. of gelatin and 0·055 grm. of hypoxanthine, as well as formic and lactic acids, with a doubtful trace of acetic acid. It also contained a new substance, *albukalin*—a crystalline body, for which Thaile obtained the formula $C_4H_8NO_3\frac{1}{2}H_2O$. —Dr. Reichardt, *Archiv Pharm.* (2), cxlv, 142.

Decomposition of Animal Substances containing Phosphorus.—Flesh of fishes beaten into a pulp was allowed to putrefy. The gases given off were collected and examined for phosphorus and sulphur. Sulphuric acid was found in abundance, but no phosphoric acid.—P. Ploesz, *Med. Chem. Natur.*, 1871, 521.

Blood-corpuscles of Birds and Snakes.—The nuclei were separated and examined. In birds and snakes the nuclei consist of the same substance, but this substance was not found in ox-blood. The substance seems to resemble mucin, but differs from it in containing 2·4 per cent. of phosphorus, and not being soluble in dilute mineral acids.— P. Ploesz, loc. cit., 461.

Subcutaneous Absorption of Starch.—Starch-granules derived from rice flour have been found to be absorbed, not only when injected into the abdominal cavity, but also when introduced into the subcutaneous connective tissue. The observation is due to Herr Auspitz, who thinks that the starch-granules gain access to the circulation, passing into the lymphatic system through the openings between it and the serous cavities.—*Wiener Med. Jahrbüchern.*

Artificial Digestion of Casein.—Casein was digested in pepsine solutions for periods varying from five hours to eleven days, at a temperature of from 40° to 45°. The greater part was dissolved, bnt a portion always remained behind. The digested solution contained pepsine, peptone, leucine, and tyrosine. The author also thinks that he has obtained a definite compound of barium and peptone.—N. Lubarin, *Med. Chem. Untersuch.*, 1871, 463.

Metamorphosis of Albuminous Substances in Ruminating Animals.—From this paper it appears that the whole of the albuminoids undergoing decomposition appear as oxidation products in the excretions. The metamorphosis of albumen is dependent on the quantity in circulation. When albumen in the food is deficient the body loses albumen. Drinking large quantities of water increases the metamorphosis of nitrogenous matters. When the quantity of nitrogen in the food is increased the excretion of nitrogen soon adapts itself to this increase. The animals were found to increase in weight when, along with a sufficient quantity of albumen, a quantity of non-nitrogenous substances were given.—Stohmann, Fruehling, and Rost, *Chem. Cent.*, 1871, 377.

Properties of Egg-albumen.—It is well known that the albumen of eggs, when treated with acids, acquires new properties, chemical and polarimetric; alkalies also produce certain changes. When one gram of caustic potash is added to 75 c.c. of a one tenth solution of egg-white, and saturated immediately with acetic acid, total precipitation of the albumen occurs without the application of heat. This does not occur when the albumen is from urine. Animal charcoal removes albumen from its solution in liquids. Gautier ('Jour. Pharm.' (4), xiii, p. 16) remarks that egg-albumen consists of two albumens, one coagulated at 63°, the other at 74°; that it is an alkaline albuminate, its coagulation being preceded by displacement of the base, Wurtz's albumen being the acid of this base.—A. Petit, *Jour. Pharm* (4) xiii, 14.

Influence of Food in the Production of Milk in the Cow.—By increase of the albuminons and fatty elements of the food, an increase (up to a certain maximum) in the quantity of milk is caused, but this ceases sooner or later during lactation. Diminution of these elements causes a diminution in the quantity of milk. The quantity of casein and albumen does not seem to be altered by diet.—Kücher, *Chem. Centralbt.*, 1871, 102.

The Albuminoids of Milk.—Three flesh-formers are known to exist in the milk of many animals. The casein may be removed by precipitation with acetic acid, the albumen by ebullition, and the third albuminoid by aqueous carbolic acid or solution of corrosive sublimate, and, according to M. Morin, by the addition of alcohol to the whey, after it has been freed from fat by ether, concentrated, and filtered. M. Morin calls the substance he thus obtains "galactin," and regards it as a normal constituent of the gastric juice, blood, &c., and many plants. He also detects it in many morbid secretions. As the precipitate which its solution forms with tannin redissolves at 60° C., the detection of galactin is easy.—*J. Pharm.* (iv), xiv.

Excretion of the Nitrogen of the Albuminoids.—Seegen, in opposition to Voit, concludes that the whole of the nitrogen derived from the decomposition of albuminous substances was not excreted as urea in the urine. By using one method in collecting the urine the deficiency of nitrogen was found to be 25 per cent. of that ingested. By Voit's method of collecting, *i. e.* making the animal micturate into a vessel every two hours, the deficiency was only 1 per cent. If the increase in weight of the animal was reckoned as muscle, then there would be no loss, but an overplus of 4 per cent. More urine was collected by Voit than by Seegen, who merely collected what was voided in the cage.—J. Seegen, *Wien Akad. Ber.*, lxiii, Abt. ii, 11.

Differences between Albumen and Casein.—By oxidation with an alkaline solution of potassium permanganate casein yields 6·5 per cent. of ammonia, while albumen gives 10 per cent., thus indicating a considerable difference in chemical structure of these allied substances.—J. A. Wanklyn, *Pharm. J. Trans.* (2), ii, 66.

Constitution of Milk.—Dumas thinks that minute quantities of substances difficult to recognise may exist in milk which are essential to nutrition. As these are certain to be omitted in artificial substitutes, the nutrition of children fed on these substitutes would be sure to suffer.—M. Dumas, *Arch. Sci. Phys. Nat.* (2), xli, 105.

On the Mucin of the Submaxillary Gland.—In a paper by J. Obolensky ('Med. Chem. Unters.,' 1871, 590—593) an account is given of this substance, first obtained by Staedeler from the salivary glands. Mucin is prepared by rubbing down the salivary glands of the ox with pounded glass, placing the mass in water for a night, then filtering and again treating the residue with water. Acetic acid in excess is added to the filtrate and the precipitate washed with water, acetic acid, and warm alcohol; it is then dried. Mucin is only obtained from the submaxillary gland, and is insoluble in acetic acid. Mucin contains no sulphur, and from 1·06 to 1·07 per cent. of phosphoric acid. This probably exists as metallic phosphates, adherent to the mucin. It yields about 2·44 per cent. of pure ash, 1·64 parts being soluble in water, the rest in hydrochloric acid. The percentage composition of mucin free from ash is C 52·2, H 7·18, N 11·87, O 28·75. Moist freshly precipitated mucin swells in water, is easily soluble in lime and baryta water, is not precipitated by tannic acid, ferric chloride, or mercuric chloride. It is also soluble in concentrated hydrochloric and nitric acid and in sodium carbonate. When treated with hot alcohol and dried in the water bath it scarcely swells in water, and is only slowly soluble in lime or baryta water or soda solution. Dried and pulverised mucin, when heated with dilute sulphuric acid for twenty-five minutes, gives a rich precipitate of cuprous oxide with excess of caustic soda and copper sulphate. If heated longer the quantity of the reducing agent diminishes, and lastly disappears. This, taken along with the insolubility of the reducing agent in absolute alcohol, shows that it is not the same as either grape sugar or milk sugar.

Effects of Change of Climate on the Human Economy.—Tropical climate causes loss of weight and depression of both mental and bodily energy. With hard work and diet of salt meat the loss is greatly increased.—Dr. Alexander Rattray, *Proc. Roy. Soc.*, 1870, p. 529.

Effect of Diet and Exercise on the Elimination of Nitrogen.—These experiments show that there is an increased elimination of nitrogen during the period of rest after severe exercise, and that the necessary amount of force needed for great muscular work can be obtained by the muscles from fat and starch.—Dr. E. A. Parkes, *Proc. Roy. Soc.*, 1871, p. 349.

Urea a normal and constant Constituent of Bile.—Dr. O. Popp ('Zeitschr. f. Chem.' (2), vii, 88) states that urea is a normal and constant constituent of bile, and gives a mode of extracting the urea from bile. Popp states that bile from the pig contains more urea than that from the ox.

Sarcolactic Acid in Urine of Patient suffering from Trichinosis.—Sarcolactic acid, recognised by its zinc salt, was obtained from the urine of a patient in the Frankfort Hospital who was suffering from trichinosis.—Th. Simon and F. Wibel, *Deut. Chem. Ges. Berlin.*, iv, 139.

Estimation of Urea by Sodium Hypobromite.—G. Huefner, in 'Jour. pr. Chem.,' 2, iii, 1, proposes a modification of Knop's process for the determination of urea. Knop's solution was made by placing 100 grms. of sodium hydrate in 250 cubic cent. of warm water, leaving the solution to cool, and then adding 25 cub. cent. of bromine. 50 cub. cent. of this solution, diluted with 200 cub. cent. of water, will liberate from 130 to 150 cub. cent. of nitrogen from a solution of ammonium chloride. Huefner proposes to modify Knop's process by employing a gentle heat to complete the reaction. On mixing the urea and the oxidizing agent, nitrogen and oxygen gases are liberated and are collected in a graduated cylinder. After absorption of the oxygen the percentage of nitrogen found by experiment in urea by this method closely corresponds to the theoretical amount.

Pigments of Bile and Urine.—The spectroscopic appearances of the bile which has given the usual colour reaction with nitric acid are described, also of the coloured solution obtained by exhausting bile with dilute hydrochloric acid. This reddish solution gives a well-marked absorption band between Fraunhofer's *b* and F lines. The same band, and others described by Jaffé, were also met with in healthy urine.—Dr. Max Jaffé, *Arch. Pharm.* (2), cxlv, 148.

Chemical Composition of Pus-corpuscles.—Pus-corpuscles are composed chiefly of albuminoids. These are—1, alkali-albuminates; 2, an albuminoid coagulable at 48°—49°; 3, an albuminoid coagulable at the ordinary temperature of serum-albumen; 4, Ronda's hyaline substance; and, 5th, an albuminoid unaltered in water and sodium chloride, with difficulty soluble in hydrochloric acid. The alcoholic extract of pus gives lecithin and cerebrin and a substance similar to it. The ash in 100 parts amounted to 1·269, and contained—NaCl, 0·143; K_2O, 0·655; MgO, 0·087; Na_2O, 0·262; CaO, 0·830; Fe_2O_3, 0·039. Phosphoric acid and chlorine are also present. The nuclei of pus-corpuscles give the same reactions as mucin. The substance of the nuclei contains nitrogen and sulphur, and is rich in phosphorus.—F. Miescher, *Med. Chem. Untersuch.*, 1871, 441.

Chemical Composition of Pus.—Hoppe-Seyler agrees with Miescher in the results obtained by the latter author.—F. Hoppe-Seyler, *Med. Chem. Untersuch.*, 1871, 486.

The Alkaloids of Tobacco Smoke.—The physiological action of tobacco, when used as a narcotic, is commonly attributed to the nicotine which it is known to contain, although Zeiss, nearly thirty years ago, could not discover this alkaloid in tobacco smoke. H. Vohl and H. Eulenberg differ from this view; they find mere traces of nicotine in many prepared snuffs and quid-tobaccoes. The effective constituents of tobacco smoke appear to be carbonic oxide, cyanogen compounds, and especially certain organic bases, which are well known as occurring amongst the products of the destructive distillation of peat and shales. These bases are oily bodies discovered by Anderson and Greville Williams, and belong to the pyridine series. This series commences with pyridine, C_5H_5N, and ends with viridine, $C_{12}H_{19}N$; picoline, the second member of it, is isomeric with the well-known alkaloid aniline or phenylamine, a derivative from indigo and coal-tar.—*Archiv der Pharmacie* (2), cxlvii, S. 130.

Action of Crystallized Aconitine.—MM. Gréhaut and Duquesnel detail some experiments made with this substance. Small doses of aconitine destroy the motor power of the nerves, the physiological action thus resembling that of curarine. Experiments are detailed, made both on frogs and rabbits. In the rabbit the action was very rapid, one milligramme in half an hour rendering the sciatic nerve incapable of conveying impressions to the muscles, the muscles themselves not having lost their contractility. In this case artificial respiration had to be employed.—*Compt. Rend.*, lxxiii, 209.

Detection of Curarine.—The method of separating curarine from the flesh, blood, urine and fæces, &c., is described. When ·00006 grm. of curarine is treated with $\frac{1}{4}$—$\frac{1}{2}$ c.c. of concentrated sulphuric acid (monohydrate) an immediate red coloration is produced, which becomes darker on standing, and passes into rose-red after four hours. Concentrated sulphuric acid and potassium chromate give the same reaction as with strychnine.—Karl Koch, *Chem. Centr.*, 1871, 219.

Synthesis of Alkaloids.—The alkaloid coniine, the active principle of hemlock (*Conium maculatum*) has been produced by Schiff. Butyraldelhyde, when acted upon by alcoholic ammonia at a temperature not exceeding 100° Cent., produces two bases, one of which is dibutyraldine. By dry distillation of dibutyraldine, among other products coniine is produced.

$$\underset{\text{Dibutyraldine.}}{C_8H_{17}NO} = HO_2 + \underset{\text{Coniine.}}{C_8H_{15}N}.$$

—Schiff, *Pharm. J. Trans.* (3), 1, 605.

Strychnine-oxethyl Compounds.—Compounds of strychnine and quinine with ethylene hydroxychloride (C_2H_4OHCl) have been prepared and examined by R. Messel. The formula of the strychnine derivative is $C_{21}H_{22}N_2O_2 + C_2H_4OHCl = C_{23}H_{27}N_2O_3Cl$. When the solution of the chloride of strychnine-oxythyl was administered to medium-sized frogs by subcutaneous injection three to four mgrms. caused death. In smaller doses total paralysis of the motor nerves was produced, which lasted for hours; during this time, however, the muscles of the animal were capable of contracting powerfully on the application of a direct stimulus.—*Ann. Ch. Pharm.*, clvii, 7; *Jour. Chem. Soc.* (2), ix, p. 148.

Decomposition of Solution of Morphine Acetate.—Morphine acetate in solution undergoes decomposition if kept for some time. J. M. Maisch has made experimeuts on a solution of eight grains of the acetate in half an ounce of distilled water. On keeping for several months the solution became of a pale brownish colour, and deposited a quantity of brown matter. A single large crystal formed, which on examination proved to be pure morphine. The liquid appeared still to contain a small quantity of the morphine acetate in solution.—*Pharm. Journ. Trans* (3), 1, 664; *Jour. Chem. Soc.* (2), ix, p. 148.

Tissue Changes in Phosphorus Poisoning.—Voit attempts to explain the fatty degeneration of organs during disease by a comparison of the results obtained in poisoning with phosphorus. The fat might be produced in three ways:—1. Derived directly from the food. 2. Brought from other parts of the body, as the subcutaneous cellular tissue. 3. Produced in the cells of the organ by the splitting up of the albuminous bodies contained in them. By experimenting on dogs deprived of food he found that the fat could not be derived from the food or from fat brought from other parts of the body. The combined albumen of the organ undergoes oxidation, thus causing complete disorganization and destruction of the tissues.—K. Voit, *N. Rep. Pharm.*, xx, 340.

Method of distinguishing the Arsenic Deposit in Reinsch's Process from Mercury.—In the 'Chemical News,' xxxiii, 73, Mr. James St. Clair Gray describes a mode of distinguishing the arsenic deposit in Reinsch's process from mercury. The method employed is to take the slip of coated copper and rub it on a flat piece of pure gold. If mercury exists in the deposit on the copper it will mark the gold with a clear, white, shining streak, which is removable by the action of nitric acid.

Absorption of Oxygen by Fish.—It has been found by M. Gréhaut that the dissolved oxygen of water is completely removed by the respiration of fish before they become asphyxiated. This absorption of oxygen and consequent production of carbonic acid gas is by no means confined to the bronchiæ, but the skin of the anima seems nearly as effective in bringing about the change. In connection with this observation we may note the recent experiments of Signor Panceri on the phosphorescence of fish, which he attributes to the penetration beneath the skin of dissolved oxygen, and the resulting oxidation of the fat of the subcutaneous adipose tissue.

Animal Starch.—In the yolk of the hen's egg starch granules exist. During a certain stage of incubation they are easily demonstrated, and appear as granules about ·025 mm. in diameter, giving the usual optical characters of starch when examined with polarized light. In this state they do not always give a blue reaction with iodine, but sometimes turn red. During incubation three or four successive series of granules are formed, which disappear. The disappearance and reappearance of the granules Dareste thinks may be due to the transformation of the starch into glucose, and its re-formation from this glucose.—C. Dareste, *Compt. Rend.* lxxii, 845.

Researches on Alcoholic Fermentation and Nutrition of the Yeast Plant.—The author points out the different substances necessary for the nutrition of the yeast plant, one of the most important being acid potassium phosphate.—Adolph Mayer, *Pogg. Ann.*, cxlii, 293.

Flesh-juice of Phocæna communis.—The following comparison between the juice of the flesh of porpoise and the horse is given in this paper:

	Porpoise.	Horse-flesh.
Creatine	6·10	7·60
Sareine	1·05	1·23
Xanthine	traces	0·11
Inosite	0·08	0·30
Sarcolactic acid	7·45	4·47
Taurine	—	0·70

—Oscar Jacobsen, *Ann. Ch. Pharm.*, clvii, 227.

Putrefaction and Disinfection.—Hoppe-Seyler finds that 1 per cent. of carbolic acid will put an end to all organized life, but that 2 per cent. is required to stop putrefactive changes. The most effective way of destroying germs in the air is found to be by the use of sulphurous oxide gas, as 14·3 to 28·6 grms. of sulphur burnt for each cubic metre of space entirely prevents the growth of fungi.—Hoppe-Seyler, *Med. Chem. Unters.*, 1871, 561.

Artificial production of Calcareous substances, such as exist in the Organism.—M. Harting ('Compt. Rend.' (2), xxiii, p. 361) has succeeded in imitating calcareous bodies, such as biliary concretions, otolites, pearls, spiculas of alcyonium, &c., but has failed to imitate the peculiar calcareous skeleton of the echinoderms and bone. The length of time required for the deposit seems to be of primary importance, as M. Harting found the calcareous combination to be formed in organic fluids by double decomposition when the action was retarded by slow diffusion. Mr. Rainey has anticipated M. Harting.

On the Purification of Fat.—M. Debrunfant ('Compt. Rend.,' lxxi, p. 36) states that tainted meat is deprived of its disagreeable odour by being fried, and after frying may be used for any culinary purpose. Fish oils can be deprived of their odour by heating to a temperature of 330°. In the kitchen, fats may be easily purified by placing in a frying pan, raising the temperature to between 140° and 150° C., and then sprinkling the fat cautiously with small quantities of water; the steam which is evolved carries off the offensive fatty matters, which are thus decomposed. This process would, in many cases, prove very useful, but it is evidently an operation which must be conducted with considerable caution.

Detection of Turmeric in powdered Rhubarb and Mustard.—Inferior rhubarb may be adulterated with turmeric to improve its colour. It can be detected by agitating a small quantity of the rhubarb for a minute or two in strong alcohol and filtering; to this add a strong solution of borax, and a considerable excess of hydrochloric acid. If pure, the filtrate will be light brown, the borax rendering it red-brown, a tint at once discharged by the hydrochloric acid. If adulterated, the brown colour remains after the addition of the acid. The same process is applicable to mustard.—J. M. Maisch, *Pharm. J. Trans.* (3), i, 1027.

REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIENE.

By Benjamin W. Richardson, M.D., F.R.S.

I.—Toxicology.

Toxical studies on chloral hydrate.—The most important subject, in a toxicological point of view, that has been under discussion since our last report, has relation to chloral hydrate. The increasing employment of this narcotic by the members of the general public, without the advice of the professors of medical science, has been attended by several fatal occurrences; while in instances where fatal results have not taken place, peculiar symptoms have followed the frequent self-administration of the narcotic—symptoms of serious import, and demanding ready recognition. We have, consequently, devoted some labour to the investigation of certain facts bearing on the toxicological history of chloral hydrate, to which, and the results of our inquiries, we would now direct attention.

1. We have endeavoured to ascertain what is a dangerous and what a fatal dose of chloral hydrate. The conclusion at which we have been able first to arrive on this point is, that the maximum quantity of the hydrate that can be borne, at one dose, bears some proportion to the weight of the animal subjected to its influence. The rule, however, does not extend equally to animals of any and every class. The proportion is practically the same in the same classes, but there is no actual universality of rule. A mouse weighing from three quarters of au ounce to an ounce will be put to sleep by one quarter of a grain of the hydrate, and will be killed by a grain. A pigeon weighing weighing twelve ounces will be put to sleep by two grains of the hydrate, and will be killed by five grains. A guineapig weighing sixteen ounces will be put by two grains into deep sleep, and by five grains into fatal sleep. A rabbit weighing eighty-eight ounces will be thrown by thirty grains into deep sleep, and by sixty grains into fatal sleep.

The human subject, weighing from one hundred and twenty to one hundred and forty pounds, will be made by ninety grains to pass into deep sleep, and by one hundred and forty grains into a sleep that will be dangerous.

From the effects produced on a man who had of his own accord taken a hundred and twenty grains of the hydrate, and who seemed at one period to be passing into death, we were led to infer that in the human subject one hundred and forty grains should be accepted as dangerous, and one hundred and eighty as a fatal dose. Evidence has, however, recently been brought before us which leads us to think that, although eighty grains would in most instances prove fatal, it could, under very favorable circumstances, be recovered from.

Dr. Hills, of the Thorpe Asylum, Norwich, has, for example, favoured us with the facts of an instance in which a suicidal woman took no less than *four hundred and seventy-two grains* of the hydrate dissolved in sixteen ounces of water, and actually did not die for thirty-three hours. Such a fact, ably observed as it was, is startling; but it does not, we think, militate against the rule that one hundred and forty grains is the maximum quantity that should, under any circumstances, be administered to the human subject.

2. A second point to which our attention has been directed is, what quantity of hydrate of chloral can be taken with safety at given intervals for a given period of time, say of twenty-four hours. To arrive at some fair conclusion on this subject, we calculated from a series of experiments the time required for the development of symptoms from different doses of the hydrate, the full period of the symptoms, and the time when they had entirely passed away. Great difficulties attend this line of investigation; but we may state, as a near approximation to the truth, that an adult person who has taken chloral hydrate in sufficient quantity to be influenced by it, disposes of it at the rate of about seven grains per hour. In repeated doses, the hydrate of chloral might therefore be given at the rate of twelve grains every two hours for twenty-four hours, with less danger than would occur from giving twelve times twelve (144) grains at once; but we do not think that amount ought, except in the extremest emergencies, to be exceeded, in divided quantities.

3. A third point to which we have paid attention is, the means to be adopted in any case when, from accident or other cause, a large and fatal dose of chloral hydrate has been administered. We can speak here with precision. It should be remembered that this hydrate, from its great solubility, is rapidly diffused through all the organism. It is in vain, consequently, to attempt its removal by any extreme measures after it has fairly taken effect. In other words, the animal or person under chloral, like an animal or person in a fever, must go through a distinct series of stages on the way to recovery or death; and these stages will be long or short, slightly dangerous or intensely dangerous, all but fatal or actually fatal, according to the conditions by which the animal is surrounded. One of the first and marked effects of the chloral is reduction of the animal temperature; and when an animal is deeply under the influence of the agent, in the fourth degree of narcotism of Dr. Snow, the temperature of its body, unless the external warmth be carefully sustained, will quickly descend seven and even eight degrees below the

natural standard. Such reduction of temperature is itself a source of danger; it allows condensation of fluid on the bronchial pulmonary surface, and so induces apnœa, and it indicates a period when the convulsion of cold (a convulsion which sharply precedes death) is at hand.

We offer these explanations in order to indicate the first favorable condition for the recovery of an animal or man from the effects of an extreme dose of chloral hydrate. It is essential that the body of the animal be kept warm, and not merely so, but that the air inspired by the animal be of high temperature. The first effort to recovery, in short, should consist in placing the animal in a warm air. This fact is perfectly illustrated by experiment on the inferior animals. In the pigeon an air of 95° Fahr. is most favorable, in the rabbit an air at 105° to 110°, in the dog the same. In man the air to be breathed should be raised to and sustained at 90° Fahr. at least.[1]

The next thing to be remembered in the recovery of persons under the fatal influence of chloral hydrate is to sustain the body by food. I find that even under deep sleep from the narcotic, although the process of waste is less than is common under natural conditions of rest, there is still a very considerable waste in progress, which, if not made up, is against recovery. I find also that the digestive and assimilating powers, though impaired during sleep from chloral, are not arrested, but may be called into fair action with so much advantage, that if two animals be cast into deep sleep by an excessive quantity of the narcotic, and one be left without food and the other be artificially fed on warm food, one fourth of the chance of recovery is given to the animal that is supplied with food. In the human subject warm milk, to which a little lime-water has been added, is the best food. Milk is very easily administered mechanically, and it should be administered in the proportion of half a pint every two hours.

4. The fourth point to remember is to sustain the breathing; in the inferior animals the question of life or death can be made to turn on this pivot. But the artificial respiration must be carried out with great gentleness; it must not be done by vehement movements of the body or compressions of the chest, but by the simple process of inflating the lungs by means of small bellows, through the nostrils. We have devised in the course of our researches various instruments for artificial respiration, viz. a small double-acting bellows, a small syringe, and a double-acting india-rubber pocket-bellows; and we have lately made an instrument which acts by a simpler method still, *i. e.* we merely attach to a single hand-bellows a nostril-tube, and gently inflate the lungs, letting the elasticity of the chest-wall do the work of expiration. A little valve near to the nostril-tube effectually stops all back currents from the lungs into the bellows. For the human subject, five

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[1] We have no doubt it will be found, as the chronicle of deaths from chloral hydrate increases, that the mortality from the agent will be the greatest when the thermometrical readings are the lowest, and *vice versâ*.

charges of air from the bellows should be given at intervals of five seconds apart.

The symptoms of acute poisoning by chloral hydrate are, briefly, profound coma, great muscular relaxation, apoplectic breathing, and flushing of the face and neck, with intermissions of pallor. The eyes are usually rolled upwards as under chloroform, and at times, as impressions of motion are made on the surface of the body, there is muscular tremor, which may pass into convulsion. In time the extremities become cold, and the bronchial surfaces become charged with frothy mucus, which greatly impedes the breathing and hastens the final result.

The chronic symptoms of chloral poisoning are—sleeplessness, unless the narcotic be taken in very large doses; great mental irritability and muscular prostration; uncertainty of movement, with tendency to fall forward; caprice of appetite and frequent nausea. In some cases there is injection of the conjunctivæ, and in other cases yellowness. The urine, in extreme cases, contains albumen, and the bowels are commonly constipated, the evacuations being white and hard. Chloral hydrate does not produce the ecstatic dream or delirium caused by opium or haschish; on the contrary, it causes, through all the stages of its action, a sense rather of depression than of elevation of mental faculty.

Volumetrical determination of Arsenic.—Mr. E. Waitz has investigated with care the different methods for determining arsenious acid, viz. 1st, by means of free iodine; 2nd, by means of dipotassic dichromate; 3rd, by means of potassic permanganate; as well as the method for estimating arsenic acid by means of uranic acetate.

The iodine method depends, as is well known, upon the conversion of arsenious into arsenic acid in alkaline solution. In an acid solution arsenious acid can exist in the presence of iodine or chlorine, and is only partly converted into the higher oxide. The alkali must be in the form of a carbonate, for a caustic alkali combines with the iodine. The author's experiments show that normal sodic carbonate fixes iodine, but that the acid carbonate does not; a solution saturated in the cold should be used. This only confirms the previous observations of Fresenius. On employing a standard solution of arsenious acid in hydrochloric acid, the free acid has first to be neutralised by means of caustic soda or acid sodic carbonate. With an excess of this latter salt good results were obtained,

Experiments made with a view of converting precipitated arsenious sulphide into arsenious acid by the action of an ammoniacal solution of silver nitrate upon the sulphide dissolved in ammonia, as well as by means of freshly precipitated bismuthous hydrate, and of substituting in this manner the trisulphide—which can be obtained in a state of great purity—for the trioxide, where unsuccessful, as the reaction is never quite complete, owing to the formation of sulpho-salts of silver and bismuth.

The method first proposed by Kessler of determinating arsenious acid in an acid solution by means of depotassic dichromate, and standardizing back by means of a ferrous sulphate solution, gave good results. Excess of hydrochloric acid has to be avoided. Kessler succeeded in utilising arsenious sulphide by treating it in a hydrochloric acid solution with mercuric chloride, a saturated solution of which converts the sulphide slowly at the ordinary temperature, more rapidly on the application of a gentle heat into the trioxide. The reaction is over when the mass has become white. The author's experiments show further that the mixture of trisulphide and sulphur which is obtained when a solution of arsenic acid is precipitated with sulphuretted hydrogen, cannot be converted directly into arsenious acid by digestion with mercuric chloride, on account of the dense nature of the precipitate, but that it is readily acted upon after dissolving out the trisulphide by means of dilute ammonia and reprecipitating with hydrochloric acid.

The oxidation of arsenious into arsenic acid by means of potassic permanganate is never perfect, and a volumetric method based upon this reaction was found by the author to be most untrustworthy; but by adding excess of permanganate—more than double the amount required according to theory—and standardizing back with a solution of ferrous sulphate, very accurate results were obtained.

The volumetric determination of arsenic acid by means of uranic acetate in the presence of free acetic acid and an alkaline acetate, did not yield trustworthy results.—*Zeitschr. f. Anal. Chem.*, x, 158—183; and *Pharmaceutical Journal*, November 25th, 1871.

On the distinctions betweem edible and poisonous Mushrooms.—A writer under the initials W. G. S., gives the following concise account of the differences between the edible mushroom and the poisonous fungi resembling it. First and foremost, the true mushroom (Agaricus campestris) is invariably found among grass in rich open pastures, and never on or about stumps or in woods. Many cases of poisoning have occurred owing to the supposed mushrooms being gathered from stumps or in woods. It is true there is a certain variety found in woods and woody places (A. silvicola), but as far as amateurs are concerned it is best left alone. A second very good point is the peculiar intense purple-brown colour of the spores (which are analogous to seeds); the ripe and fully matured mushroom derives the intense purple-brown colour (almost black) of its gills from the presence of these innumerable coloured spores. To see these spores, and so become acquainted with the peculiar colour, remove the stem from a mushroom, and lay the upper portion with the gills lowermost on a sheet of writing-paper; in a few hours the spores will be deposited in a thick, dark, impalpable powder. Several dangerous species, at times mistaken for this mushroom, have these spores umber-brown or pale umber-brown in colour, and belong to *Phaliota* or *Hebeloma*. There are innumerable varieties of the true mushroom (and of the horse mushroom), but all are equally good for the table. Sometimes the top is white and soft, like kid leather; at other times it is dark brown and scaly. Sometimes on being cut or broken the mushroom changes colour to yellow, or even bright red; at other times no change whatever takes place. But observe, the mushroom always grows in pastures; always has dark purple-brown spores; always has a perfect encircling clothy colour; and always gills which do not touch the stem, and a top with an overlapping edge.—*The Garden;* and *The World of Science*, December 10th, 1871.

Phosphorus Poisoning said to be cured by Oil of Turpentine.—Dr. Lichtenstein reports of a girl nineteen years old, who cooked with a steak of meat the heads of eight friction matches, and partook of the same for food. Soon after—precise time not given—she was attacked with a pain in the stomach and vomiting of phosphorescent matter mixed with coagula of blood. Twelve drops of Ol. Terebinthinæ in milk being administered, the pain in the stomach and vomiting ceased. Subsequently the vomiting returned, the discharges presenting traces of blood but none of phosphorus. An emetic of Ipecacuan. and Tartrate of Antimony was given, and the turpentine in barley water continued. The vomiting now became almost entirely free from blood, and at the end of two hours the only complaint made was of loss of appetite; no after symptoms. The case is adduced by Dr. Lichtenstein as a further evidence of the correctness of the reports of Personne, Andans, Köhler, and others in favour of oil of turpentine as an antidote against poisoning by phosphorus.

In a comment upon the report of Dr. Lichtenstein and his conclusion, Dr. Schultzen remarks, that during a few years past he has observed in the wards of the Berlin "Charité" some thirty to forty cases of phosphorus poisoning, of which nearly one half terminated favorably, notwithstanding no oil of turpentine was given. The prognosis of a favorable event is to be based solely upon the occurrence of free vomiting, either spontaneously or induced, soon after the poison has been taken. In Dr. Lichtenstein's case vomiting set in immediately after the phosphorus was swallowed, and, according to Dr. Schultzen, to this is to be mainly, if not entirely, attributed the safety of the patient.—*Berlin Klin. Wochenschr;* and *American Journal of the Medical Sciences*, July, 1871.

Snake Poison.—Dr. Shortt states that the numerous experiments he has conducted during the last four years, not only in Madras, but in most of the districts of this presidency, lead him to believe that when an animal is fairly wounded, and has had poison injected into the wound, be the quantity small or large, in man or beast, death is sure and certain, with this exception, that when the poison is long retained in the poison sacs, and is thus to a certain extent concentrated by the absorption of the watery parts, and the quantity large, death is almost instantaneous, and it is a question of so many minutes; but when the poison is weak from frequent and rapid secretion, or the quantity small, death is more prolonged, and it is then a matter of hours. The cobra poison is so active and energetic that five sixths of a grain of the fluid poison killed a large Persian horse in twenty-four hours and twenty-five minutes, and one twelfth of a grain killed a full-grown dog, weighing 18 lbs., in twenty-five minutes. These cases, as well as the experiments conducted by Dr. Shortt and others, are, he says, fully conclusive as to the worthlessness of ammonia as an antidote.—*Ibid.*

Three cases of poisoning by Whiskey in children.—Dr. P. De Marmon, of King's Bridge, New York, reports in a most able paper three cases of acute poisoning by whiskey in young children, two of the cases ending fatally. In one case a child aged five years took a tumbler full of whiskey at six o'clock in the evening, it was not seen by a medical man until nine in the evening, it was then comatose, the respiration was stertorous and 82 in the minute; the pulse was full, irregular, but rather slow; the temperature taken in the axilla was $93\frac{1}{2}^\circ$ F. The action of the bowels and of the bladder was involuntary: death occurred nineteen hours after the ingestion of the fatal drink. In the second case a little girl five years old took a tumbler full of whiskey and beer at five o'clock in the afternoon. The draught was given to her by a lad fifteen years old. She soon became comatose, and her temperature fell to 94° F., her respirations being 80 per minute, and the beats of her pulse 60. The child was kept warm, and was treated freely with liquor ammoniæ acetatis, two ounces of which was administered at once in an enema. In the end recovered. The third case was that of a child, a boy eight years old, who took a quantity of whiskey at eight o'clock in the morning. This patient was treated by another physician with mustered emetics: he died twenty hours after the ingestion of the poison. The *post-mortem* revealed imperfect cadaveric rigidity and intense congestion of lungs; the internal bronchial surface was livid, and coated with bloody spumous mucosities. The kidneys were also congested. The head was not examined. The liver was congested, and the gall bladder was less than half full.

Dr. De Marmon in commenting on these cases gives the following suggestions on the subject of treatment in cases of acute alcoholic poisoning.

1. When called to see a case of poisoning by alcohol, care must be taken if but a short time has elapsed after the ingestion of the liquid, and none has been vomited, to give an emetic immediately. Dr. De Marmon prefers brown emetine to any other, inasmuch as, in a dose of from two or four grains for an adult, it produces emesis almost immediately without nausea.

2. Keep the patient warm, but not, as some persons do, take him out of doors under the pretext that fresh air will do him good; it is a very great mistake, as a man who is only half intoxicated in a warm room, may become quite drunk by going out in the cold air.

3. Give liquor ammoniæ acetatis in large and repeated doses to fluidify the blood and re-establish the circulation.

4. When the patient is not seen until a long time, say, from two to six or eight hours after the alcohol has been taken, the comatose state, the stertorous respiration, the decrease of the pulse, cold temperature, and accelerated respiration are noticed; it is then probable, if the patient has not vomited, that the greatest part of the liquid taken may have been absorbed. In this case warm applications, and liquor ammoniæ acetatis in large and repeated doses must be given immediately, an emetic being of no use, for several reasons: 1. Be-

cause in such bad cases and after such a lapse of time, the stomach, œsophagus, and pharynx, are almost always paralysed, and the emetic can have no effect; 2. Because, being usually given in larger doses for the purpose of producing emesis, it only adds to the severity of the case. The author would say the same of all mechanical means used for irritating the pharynx and larynx, with the view of producing emesis, inasmuch as these parts are, as already said, paralysed, and the stomach pump being, if supposed necessary, the only available means.—*New York Medical Journal, December*, 1870.

Case of poisoning with sugar of lead.—Dr. Maschka relates the following case of poisoning with sugar of lead:

A draper, J. R—, 69 years old, married a woman of 25, and lived happily with her, the girl was in love, however, with a postilion. After J. R. had been ill several days, and had refused all advice, he became so seriously ill that a physician was obliged to be called in on the 31st of March, 1868. At that time he was suffering from yellowness of the conjunctiva, loss of appetite, eructation, accumulation of phlegm on the chest, and attacks of giddiness. The evacuations were normal, the thirst not increased, the pulse from 80 to 90, the tongue coated. The man also felt weak. The physician ordered an emetic which took effect several times, and lemonade and such things were given. On the following day the weakness and other symptoms had so increased that sulphate of quinine and bicarbonate of soda were administered to the patient, on the 2nd of April he was evidently better and stronger, ate some soup and plums, and wished to leave his bed. On the evening of the same day he became worse and vomited several times when Dr. Maschka was summoned. The quinine was discontinued and lemonade only given. At eleven o'clock the same evening the patient again became worse, and on his second arrival the physician found the man dying, his eyes fixed, his breathing short and rattling, the pulse weak, and the extremities cold: in a short time death ensued. As during the whole time of the attendance neither colic, cramp, stoppage nor contraction of the stomach had been observed, neither any evacuation of blood seen, the physician considered the illness and death arose from want of proper diet, and cold, and when the report of his death having been caused by poisoning by sugar of lead was spread he immediately denied the probability of the report. As the report, however, continued and a great deal of sugar of lead had been found in the house, kept nominally for killing vermin and cleaning the linen, suspicions fell upon the widow of the deceased, and twenty days after death the body was exhumed. The contents of the stomach and bowels were chemically analysed, and found to be very rich in lead (yielding in 240 grammes of the contents of the stomach, 0·45 grammes of sulphuret of lead.) The whole quantity of sugar of lead taken just before the increased symptoms preceding the death was said to have been 20·12 grammes.—*Wien Med. Wochenschr.*, xxi, 14, 1871.

II. Hygiene.

Dr. Polli, of Milan, publishes an instruction on the method of applying the therapeutic sulphites for the prevention and cure of epizootic diseases, carbuncular fever, bovine typhus, cattle plague. The belief of the value of the sulphites rests on the theory that in the diseases named there is a peculiar morbific fermentation of the blood; to arrest this decomposition constitutes the cure, to prevent it constitutes the prophylactic of the disease.

With this intent a remedy is needed which the animal economy can tolerate even in large doses without inconvenience, which may possess the property of rendering the organization unsusceptible to the influence of contagious ferments, and which, while arresting the effects of them, may permit it to fulfil its healthy eliminations. This remedy is the combination of sulphurous acid with alkaline and earthy bases. Sulphite of soda, from its solubility and cheapness, is the most suitable as a curative, while hyposulphite of soda, which undergoes oxydation during its passage into the organism, albeit converted into sulphate, is the most convenient as the prophylactic.

Preservative treatment.—When sheep, oxen, or other animals are liable to infection, either directly or indirectly, the preservative treatment should be carried out in the following manner: For every ox a solution should be made consisting of 100 grammes of hyposulphate of soda in 1 litre of water, with the addition of 20 or 30 grms. of common salt to render it more palatable and to strengthen its action. One half of this solution should be administered in the morning and the other half in the evening, by means of the bottle; or it may be given in the form of boluses, after which the animal should be made to take several litres of water. No food should be given to the animal for one hour after the salt has been administered, and two hours' fast should be observed before it is taken.

This treatment ought to be continued while the epidemic is in the vicinity where the cattle are kept.

If the animals under treatment suffer from too profuse action of the bowels the doses should be modified, or not given so frequently, as the proposed remedy is not intended to be purgative, but to be absorbed and conducted with the blood into the whole economy, so as to give to the blood the property of resisting morbific ferments.

For sheep and goats the dose should be reduced about one third; that is to say, 20 grammes in the morning and the same in the evening of the hyposulphite in the form of potion or bolus, with the addition of a little common salt as before mentioned.

During this treatment the utmost cleanliness should be observed in the stables, plenty of fresh air admitted, and the purest water and the best food given to the animals.

Curative treatment.—Where the epidemic shows itself among cattle all the healthy ones should be immediately subjected to the already-named prophylactic treatment, and to the sick animals the sulphite of soda should be administered.

For oxen the dose would be from 100 to 150 grammes of sulphite of soda dissolved in one litre of water, together with 50 grammes of common salt, one half to be taken in the morning and the other half in the evening. If made into boluses a plentiful supply of water shonld be given to the animal, and the same regulations should be observed as regards the food as after the administration of the hyposulphite.

Great cleanliness of the stables, keeping up of constant ventilation, pure water, and frequent renewal of the straw or bed, are indispensable requisites for the success of the treatment.

In "L'afta epizootica" the pustules and ulcerations of the lips, mouth, and tongue, and also the teats and clefts in the hoofs should be washed and bathed frequently with tow dipped in a solution of one part of sulphite of soda and ten parts water, made fresh every day. The same treatment should be observed with ulcers after carbuncular fever or the cattle plague. The most useful detergent when the flesh is in wounds during any epidemic, is that made of a concentrated solution of sulphite of soda, which can be used in the proportion of one part of sulphite with six of water.

If the animal is unable to take the dose prescribed the same solution must be administered, increasing it from one third to one fourth of the salt, or diluting it with double the quantity of water by means of clysters, by the straight gut, dividing it into four doses. During the employment of this remedy great care should be taken to avoid, externally and internally, every acid substance.

The dose of sulphite of soda for smaller animals must be reduced in proportion from 30 to 40 grammes per day, in two doses, and administered with the same rules as were to be observed with the larger animals.

The fæces of animals treated with hyposulphites and sulphites give off a strong exhalation of hydrosulphuric acid, which, although disagreeable, is not injurious. It may almost be considered as a purifier in the stable, since on entering the respiratory tract there is reproduction of sulphurous acid and of sulphites. If the smell should inconvenience the attendants the stable might be fumigated with chlorine once or twice during the twenty-four hours. With this the hydrosulphuric acid gas diffused in the atmosphere is destroyed, and it suffices for this purpose to place half a kilogramme of chloride of lime in an earthen vessel and mix it with twice its own weight of water, and then add, drop by drop, about one of its weight of oil of vitriol; the gas which immediately evolves and diffuses itself in the air purifies it from the sulphuretted hydrogen by direct decomposition.

The prophylactic treatment may be carried on for any length of time without injury either to the flesh or the milk of the animals; the milk requiring less rennet to prepare it for cheese, and the flesh remaining fresh and free from putrefaction longer than that of the animals not under treatment.

As the sulphite treatment has not the effect of destroying the cause or the morbific ferment, but by means of a particular action of rendering the organic components of the animal economy unassailable by morbific ferments, it will be easy to understand its use in all these diverse epidemics. as well as in others which have the common characteristic of morbific fermentation.

In introducing modifications of this treatment it must be remembered that the hyposulphites of soda are rather more purgative than the sulphites; and that in affections of the blood the mucous membrane gastro-intestinal tract is always more or less inclined to hypersecretion.—*Istruzione intorno al modo di applicare la terapeia folfitica nelle epozoozie, e principalemente nell' afta epozootica (taglione) nella febbre carbonchiosa (antràce) e nel tifo bovino (peste bovina). Dell Dott. Giovanni Polli.*

On the relative powers of various substances in preventing the generation of animalculæ or the development of their germs, with special reference to putrefaction.—Dr. John Dugall, under the head given above-named, communicates one of the best essays we have for some years past read on the question of putrefactive change. There is in the paper solid experiment and very little speculation. The object of the author was not to inquire whether the minute animated specks, called in general animalculæ, are generated or developed, but how their propagation could be retarded or prevented. Three series of experiments were made, one on vegetable and two on animal matter; while sixty-seven substances were selected for the trial of their antiseptic properties. The mode of experiment was as follows:—To three drachms of a solution consisting of one part of the substance to be tested, in 500 parts of water, was added one drachm of a filtered solution of hay, of the strength of half a drachm of dried hay to a fluid ounce. This mixture was put into a phial; when the substance was volatile the phial was kept closed, otherwise it was left open. The *modus operandi* with the animal substance was similar; the only difference being that human urine was substitued for infusion of hay in one set of experiments, and a mixture of beef juice and egg albumen in another, only half a drachm of the latter mixture being added to three drachms and a half of the test solutions. Three blank experiments were first made; that is to say, three phials were filled respectively with water and infusion of hay, water and urine, and water with juice of flesh and egg albumen only, in order that any contrast in the growth of animalculæ in the simple and supposed preventive solutions might be noted. All the phials when filled were placed in a medium temperature of about 60° Fahr. and exposed to a moderate light. In two to six days the microscopical examination of each series with a magnifying power of 700 diameters, was begun and concluded. The simple solutions were found teeming with bacteria, vibriones, monads, amœbæ, torulæ. The results of the test examinations of the various test solutions were necessarily of two kinds, *life* or no *life;* the one or other being inferred from the presence or absence of moving bodies only. No notice was taken of fungi, which were generally more or less present, especially in the vegetable solutions. The experiments were afterwards repeated under various forms of modification. The substances used for preservation were classified into fifteen groups, viz. metallic salts, organic acids, salts of the alkaline earths, inorganic acids, alcohol and its derivatives, inorganic bases, organic salts, inorganic alkaline salts, volatile juices, aromatic oils, organic bases, bitter extracts, animal substances, aromatic extracts, poisonous vegetable extracts, spirituous and aqueous. Of all these in their respective groups sulphate of copper shows the highest average preventive power amongst the metallic salts, benzoic acid amongst the organic acids, chloride of aluminium amongst the salts of the alkaline earths. Amongst the organic acids the carbolic holds a very low place; it occupies the "fifth rank," a position we were fully prepared to see it in. Permanganate of potassa stands second in its group, but has a comparatively low average of preservative power. Sulphurous acid stands low in its group; hydrocyanic acid stands very high, and so does chromic acid. This latter acid, says the author, is unquestionably an antiseptic of surpassing power, and must ere long take the foremost place as a sanitary agent.

After giving the details of this inquiry, Dr. Dougall draws certain important conclusions from them. First, he maintains that *it does not appear that germs are the cause of putrefaction;* on the contrary, decomposition precedes germs, and the first inference, therefore, is drawn —after the teaching of Professor Owen, on the uses in nature of the infusoria—that the uses of the animalculæ are to destroy and remove the products of putrefaction, in which they find the proper conditions and elements for their temporary existence. Secondly, Dr. Dougall supports the old theory that the cause of putrefaction is oxygen; and, thirdly, he suggests that the mode in which certain substances prevent the appearance of putrefaction and of animalculæ is by exhaling constantly a destructive atmosphere on the myriads of animalcules which are perpetually remaining in the neighbourhood of organic matter; the preventive matter either permanently destroys the ova, or suppresses development for an indefinite time, or renders the soil inimical to their growth, whether they, the ova, be considered as lodged in the putrescible substance previous to the addition of "the preventive," or as continuously depositing on it subsequently.—*From paper read at the meeting of the British Association for the Advancement of Science,* 1871, *and separately published.*

III.—Summary.

The complete History of the Welsh Fasting Girl, Sarah Jacob, with Comments thereon and Observations on Death from Starvation. By Robert Fowler, M.D.—This book, for it is a goodly volume, supplies a full history of the painful case of Sarah Jacob, and a report of the trial of her parents for manslaughter. Dr. Fowler has acquitted himself of his task well, and has introduced many points of peculiar interest in a medico-legal sense. His definitions of death from acute as distinguished from chronic starvation, and of the appearances after death, are most judicious, and we had almost said judicial. As we propose to notice the work in a separate article, it is sufficient to say now that the author sets forth two propositions:—*one,* that death from starvation is equivalent to death from cold; *two,* that the presence of fat in the body and the absence of attenuation of the intestines are conditions compatible with death from *acute* starvation.

Process of Disinfection with Chlorine. By M. Regnauld.—The fumigation to which mattresses and bedding of all kinds more or less infected during contagious diseases are subjected, is carried out by the author in the following manner:—500 grammes of a mixture of hypochlorite of lime and the chloride of lime used in trade are placed in a linen bag holding about one litre, at a temperature of 100° C.; the bag is then closed. This bag is placed in an earthen pan containing 1 litre of ordinary hydrochloric acid and 3 litres of water. As soon as the chloride comes gradually into contact with the liquid acid, all the openings of the room in which the mattresses and bedding are suspended are to be closed, and the infected things are to be exposed to the gaseous vapours for twenty-four hours; the doors and windows of the room should then be left wide open for forty-eight hours.

Ten vessels prepared as above, giving off 500 litres of chlorine, are sufficient to disinfect from twenty to twenty-five mattresses more or less infected.—*Journal de Pharmacie*, Jan., 1871.

Case of Saturnismus from taking Snuff containing Sugar of Lead. By Dr. Wenz.—A case of much value is communicated by Dr. Wenz, of a man forty-nine years of age, who had long suffered from gastric disease. He was melancholy and misanthropic, became indifferent to his wife and children, and complained much of having to move, eat, write, dress; he suffered from extreme tremor of the limbs and from dysuria, and had considerable atrophy of the muscles of the shoulder and forearm. The muscles supplied by the radial nerve on the dorsal side of the forearm and the interossei were specially affected (with the exception of the supinator longus), and this circumstance, which the electro-therapeutic treatment of the patient suggested, first led to the correct diagnosis of lead poisoning.—*Würtemb. Corr. Bb.*, xl, 24, p. 187, 1870; and *Schmidt's Jahrbucher*, Band 151, No. 8, 1871.

Poisoning with Mercury in the form of Ointment. Dy Dr. Leiblinger.—In January, 1869, three persons were found dead in their beds who had some days before rubbed their bodies over with an ointment made from quicksilver, as a cure for itch. At the post-mortem the bodies were all alike in this particular, that there was nothing abnormal except several post-mortem spots and marks on the skin arising from the eruption produced by the acarus. The internal examination showed much blood in the skull, extreme fulness of the sinuses with dark venous blood, abnormal abundance of blood in the brain, thyroid, and windpipe; adhesion of both lungs with the pleura, congestion of the lungs with dark blue-red fluid and frothy blood, distension of the stomach, and hyperæmia of the kidneys, liver, and spleen. In the visceral organs, on their being subject to chemical analysis, large quantities of mercury were found to be present.—*Wien. Med. Wochnschr.*, xix, 96, 1869; and *Schmidt's Jahrbucher*, Band 151, No. 7, 1871.

Letters to the 'Times' on Smallpox Encampments, and a word on the Contagious Diseases Acts. By Surgeon-Major T. Atchison.—Surgeon Atchison in this pamphlet suggests as a means of meeting smallpox successfully when it is epidemic, that instead of the costly, injurious, and tardy system of congregating the sick in hospital asylums or improvised lazarettos, in a hitherto uninfected neighbourhood, the simple plan resorted to in India should be at once adopted. His plan would be to pitch tents in high and airy situations, to quarantine the encampment, and on the subsidence of the disease to disinfect or burn the camp.—*Pamphlet*, London, 1871.

On Fluid Meat: a new preparation of Meat, especially adapted to weak stomachs. By Stephen Darby, F.C.S.—Mr. Darby's fluid consists of animal flesh brought by artificial digestion with pepsin, and hydrochloric acid, to the state of a peptone. Bitter taste is removed by the addition of a small proportion of fresh pancreas. The fluid meat differs from Liebig's extract in that it contains the albuminous flesh-forming material of meat. One ounce by weight is said to equal the quantity of extract obtained by boiling from twenty ounces of meat.—*Pamphlet*, London, 1871.

Dangerous effects of a Hypodermic Injection, with extraordinary slowness of respiration, and recovery. By Dr. Frederick D. Lente.—In this case a female patient, aged 49, and somewhat intemperate, was treated with an injection of two thirds of a grain of morphia, and, this taking no effect after twenty minutes, one third of a grain more was similarly injected. About ten minutes later the patient suddenly fell back on her pillow and lapsed into complete insensibility, the respiration falling to three and a half per minute, and at one time to one in a half minute, the pulse getting up to 150. In six hours, after profuse perspiration, she began to recover, and ultimately did recover.—*New York Medical Journal*, vol. xii, No. 5.

Death from Chloral Hydrate.—A case of death at Leicester, from chloral hydrate taken in an overdose, is reported in the 'Pharmaceutical Journal' for November 25th, 1871. The person poisoned was a Mrs. Turner, and it was proved that she took the narcotic of her own accord in order to produce sleep, as she suffered from neuralgia. A point of special medico-legal importance raised at the coroner's inquest was whether the bottle containing the narcotic ought to have been labelled "poison" by the druggist (Mr. Cox) who dispensed the article. The evidence showed that chloral hydrate is not among the substances obliged to be registered and labelled as a poison.

Adulterations of Foods and their Microscopical Detection. By W. Morris.—Mr. Morris maintains that adulterations of common articles of food still exist to a great extent. Of forty-seven samples of coffee, eighteen were found pure; of the rest most were half and some were wholly composed of chicory, being worth about sixpence a pound, while the article called coffee sold at one and fourpence. Even the chicory itself was impure; in fifty-seven samples half were adulterated, the adulterants being roasted wheat, acorns, beans, carrots, and sawdust. In bread, in fifteen out of twenty loaves he found sulphate of copper added to give whiteness. Mustard is invariably adulterated with flour.—*World of Science*, December 22nd, 1871.

Drainage of Calcutta. Remarks on Mr. Clark's Calcutta Drainage Scheme. By David B. Smith, M.D.—Mr. Clark's plan for the drainage of the capital of India is to convey all liquid and solid sewage into the river, as we do in London. Dr. Smith considers this plan a system of excellent drainage, but he objects to the pollution of river water with solid fæcal sewage, and proposes that the fæcal sewage shall be separately collected and turned to useful account as manure. From his report, or address, it appears that this is already done to some extent in Calcutta. His argument, in a sanitary point of view, is conclusive.—*Pamphlet*, Calcutta, 1871.

The Phosphate Process with Sewage. By Messrs. Forbes and Price.—These experimenters described their process at the meeting of the British Association in Edinburgh in August last. Their plans are epitomised as follows:—The process is in operation at Tottenham. The sewage, after passing through some depositing tanks which had been constructed for the lime process, was pumped up at the rate of 800 or 1000 gallons per minute along a carrier into a tank a hundred yards long and of gradually increasing breadth. This tank took three hours to fill. As the sewage passed along the carrier the chemicals were mixed with it thus:—Two boxes were placed on the carrier, one a few yards further along it than the other; the first contained the phosphate mixture, the second milk of lime. Men were continually stirring the contents of each box, which were allowed to run continuously into the sewage as it passed beneath the boxes. The amount of the preparation added was not determined, but was certainly much less than the proportion indicated by previous experiments, viz. one litre to 500,000 gallons of sewage.—*Popular Science Review*, October, 1871.

The progressive Physical Degeneracy of our Town Populations. By H. W. Rumsey, M.D.—Dr. Rumsey, in an excellent paper on this subject, states briefly the leading causes at work in deteriorating town populations:

"The destructive influences at work in the dwelling places of the million include Soil-poisoning, Water-poisoning, and Air-poisoning. The *physique* of the people tends inevitably to deteriorate—(1) where the *soil* is wet or chemically bad, or where it is sodden by decomposing filth; (2) where the *water* for domestic use conveys injurious matters, inorganic or organic, from the soil, or is polluted by sewage, by the offal of slaughter-houses, or by the refuse of trades and factories, or where it distributes lead from pipes, or metallic and organic matters from roofs; (3) where the *air* within dwellings is vitiated by the overcrowding of inmates, by the want of common cleanliness, by sewer gases which often convey specific infections, or by arsenical fumes from wall-coverings; and (4) where the *atmosphere* outside of dwellings is defiled by smoke and noxious vapours from mineral and manufacturing processes, by middens and other putrefying accumulations, and above all, by the exhalations of an excessive population, the density of which in some of our worst town districts is not less than 1000 persons to the acre."

He concludes, from all the facts he has observed, that comprehensive, yet cautious, measures are necessary for the improved house accommodation of the working classes of our great towns over larger areas of habitation; that, as for the overcrowding of persons in a house, so also for the overcrowding of houses on a given area, there should be a limit to density of population fixed by law, at all events in the building of dwelling houses on fresh ground, as well as in rebuilding them on ground previously occupied; and that, for the success of such measures, the establishment of superior administrative authorities, with adequate powers, is as essential as it is for the execution of measures intended to prevent the adulteration and to secure the good quality of food; or, again, for those which protect labour from abuses and unhealthy conditions known to sap the vigour of the race.—*Paper read before the Social Science Association at Leeds*, October 10th, 1871.

Is Consumption Contagious? By D. Travers Condie, M.D.—Dr. Condie relates at length three observations of cases of consumption which came under his notice, and which seem to favour the view that without any predisposition the disease may be induced, as if by contagion, subsequently to a close association with patients labouring under the malady. He admits, however, with perfect candour that his illustrations, striking as they are, are simply exceptional cases, a trifling percentage of the entire number of cases he has treated in a practice of fifty-four years.—*American Journal of the Medical Sciences*, July, 1871.

REPORT ON SURGERY.

By Alfred Poland, F.R.C.S.

SELECTIONS FROM THE FOREIGN JOURNALS.

I.—Three Cases of Bronchocele successfully removed by Wm. Warren Greene, M.D.

The subject of extirpation of the thyroid gland in part or whole has always engrossed the serious attention of the operative surgeon, and up to late years this operation has been generally considered as inexpedient, in consequence of its danger and fatality.

Butcher, in his 'Observations on Surgery,' has revived the operation, and proved the possibility and the practicability of the removal of the gland, and with success. Its removal has also been attended with favorable results in other countries.

But it is to Dr. Warren Greene that we are indebted for the more elaborate detail of this interesting and vital question respecting the propriety of the operation. Our own personal experience in the matter so fully accords with the views and sentiments expressed by Dr. Greene, that we cannot do otherwise than quote his very practical suggestions and opinions. Dr. Greene gives a full detail of his cases, and then makes the following remarks on the operation:

"I prefer to submit these cases to the profession with very little comment. They are the only ones in which I have ever attempted the excision of bronchocele, and if they are the last I shall not regret it; for while their issue has been so fortunate, I am sure that no man could witness even, much less perform, these operations, and envy the man upon whose lot it fell to undertake them. Yet, under similar circumstances, I should not shrink from such responsibility, and this for the reason that the possibility of successful extirpation, even of the worst cases, is established; and I believe the operation, performed in the manner I have indicated, may claim quite as secure a place among legitimate *derniers ressorts* as amputation at the hip-joint.

"The several steps of the operation, as I perform it, are briefly these:

"1. Exposure of the tumour by linear incisions of ample length, avoiding most sedulously any wounding of the tumour or its fascia propria.

"2. Division of the fascia propria upon the director,

"3. Its reflection and the enucleation of the tumour with *the fingers and handle of the scalpel*, paying no attention to hæmorrhage, however profuse, but going as rapidly as possible to the base of the gland and compressing the thyroid arteries.

"4. Transfixion of the pedicle from below upwards with a *blunt* curved needle armed with a double ligature, and tying each half, or when practicable, dividing the pedicle into as many portions as there are main arterial trunks, and tying each portion separately.

"5. Excision of the gland and subsequent dressing of the wound, as in ordinary cases.

"In conclusion, I cannot refrain from one word of warning to my younger brethren whose ambition may make their fingers tingle, lest they should, in the light of these successful cases, be too easily tempted to interfere with these growths. It is, and always will be, exceedingly rare that any such interference is warrantable; *never* for relief of deformity or discomfort merely, *only* to save life; and if it is beyond all question determined in any given case that such an operation gives the only chance for snatching a fellow being from an nntimely grave, be it remembered that accurate anatomical knowledge, and a perfect self control under the most trying ordeals through which a surgeon can pass, are indispensable to its best performance."

We cannot forbear quoting in full the accurate details of the operation as performed by Dr. Greene in each of his cases, and the perusal of which may prove of essential service to those who may be disposed to undertake the operation in future.

Case 1.—On raising upon the director the little thin layer of fascia immediately investing the tumour, several veins were wounded and bled profusely; this was controlled by the fingers of an assistant, and the delicate envelope carefully reflected from the gland; but although this was done with the utmost caution and gentleness, several other veins were ruptured. I now found that the entire growth was completely covered with a network of these vessels; and so thin and tender were their walls, that the forceps tore and the ligature cut their coats; and now, although the blade of the knife had not touched the surface of the tumour, so many of these veins were opened, that in spite of all pressure that could be made the hæmorrhage was fearful. I now rapidly separated the areolar attachments, and in a few seconds was at the pedicle, which I found containing three large arteries whose pulsations were very distinct, and which were my guide for dividing the pedicle into three parts, which I also accomplished with my fingers. I immediately tied each third with a ligature composed of eighteen strands of saddlers' silk, saturated with wax and *loosely twisted.* As I drew the last cord all hæmorrhage instantly ceased. The pedicle was divided close to the goître and it removed. During the dissection I found at one point the tumour quite firm, and adherent to the structure of the vessels; and while separating it, a gush of venous blood indicated the rupture of a large vessel. The finger of an assistant controlled it until the ablation of the bronchocele, when on examination it proved to be the internal jugular vein wounded; this was tied with a ligture of three strands of silk loosely twisted; no other vessel needed interference. The main operation occupied twenty-two minutes.

Case 2.—Having divided its delicate fascia propria, I proceeded with the greatest care to separate the fascia and enucleate the tumour. This was done almost entirely by the fingers and the handle of the scalpel. The difficulty of this procedure will be appreciated when I state that the deepest portion of the gland was imdedded in what seemed to be a mass of organized lymph, through the medium of which it was attached to the œsophagus. So firm was the adhesion, that I refrained from completing the separation entirely with the fingers or instruments, lest I might lacerate the tumour and produce severe hæmorrhage. No vessels of large size were found on the surface of the growth, but two large arteries were felt pulsating powerfully at its base. Between these I passed a blunt curved needle, armed with a double ligature of eight strands of saddlers' silk, as near as possible to the œsophagus, and yet separate the vessels. Each portion of the pedicle was then firmly tied, and the tumour removed without the loss of an ounce of blood. Only two or three small superficial vessels required ligature.

Case 3.—Dividing the successive layers of fascia upon a grooved director, I came directly down upon the tumour, and began to reflect from it its thin investment. Immediately, as in the first case, at very many points the thin coats of superficial vessels gave way, and uncontrollable oozing resulted. Still the hæmorrhage was not immediately alarming until the dissection, which was carried on by the fingers, reached the calcareous portion of the tumour on the right side. Here adhesions were encountered of considerabe firmness, and as they yielded to the most careful efforts I could make, other large branches, which had given the aneurismal thrill, and whose coats were extremely attenuated, burst, and immediately we had the most fearful hæmorrhage, such as one who has not seen it can hardly realise. The only expedient was the rapid completion of enucleation and seizure of the vessels at the base of either lobe. This I accomplished in a very few seconds, and was enabled so to seize the pedicle with the fingers as to suppress the bleeding measurably until I could transfix it with the blunt needle, armed with a double ligature; this was carried in the median line from below upwards, close upon the trachea, and either ligature tightened sufficiently by a single knot, to control the hæmorrhage and give time for examination. These were subsequently tied as lightly as possible, and the tumour carefully severed from its attachments. Several small arterial twigs, not connected with the growth, were tied, The wound was left open for a short time, and in fifteen minutes from the ablation of the tumour the inferior thyroidal artery of the left side escaped from the ligature and spirted with great violence; the vessel was equal in size to the common carotid, and was seized and secured. The entire left half of the pedicle was transfixed by a tenaculum, and a new ligature was carried underneath both the original one surrounding the pedicle and the separate one around the inferior thyroid artery, and drawn as tightly as possible. Secondary hæmorrhage occurred on the tenth day, which was controlled by digital compression for seventy-two hours.—*American Journal of Medical Science*, January, 1871.

II.—On the Galvano-caustic Treatment of Angioma.

Dr. H. Maas has collected and tabulated 130 cases in which this method was carried out. The cases include congenital nævi and teleangiectasis, cavernous venous angioma, arterial racemose angioma, and mixed allied tumours.

The following is a table of the results:—

Kind of Angioma.	Cured.	Improved.	Unknown.	Deaths.	Total.
Capillary	32	1	1	—	34
Cavernous (venous) . .	72	8	1	3	84
Arterial (racemose) . .	2	1	—	—	3
Mixed angioma . .	6	1	2	—	9
Total . . .	112	11	4	3	130

The instruments employed were:—

The loop wire in 39 cases; the porcelain burner in 17 cases; the setaceum candens, or pointed wire, in 12 cases; and various other galvano-cauteries in 59 cases.

The results of the cases are shown in the above table; the perfect cures amounted to somewhat over 85 per cent. Under the eleven cases which were considerably improved, one was afterwards extirpated, and one involving the finger to a considerable extent underwent amputation; four subsequently returned. In four cases the result is not given, and under the three deaths, the fatal occurrence was due in one instance to hæmorrhage from the cicatrix—a boy nineteen months old, which was unobserved and neglected;—the second case, a man aged thirty-five, died four weeks after the operation, from chronic trismus and tetanus; in the third case the cause of death is not stated, but occurred some days after the operation in a girl aged eight days.—Langenbeck's *Archiv f. Klin. Chir.*, 1870, Bd. 12, p. 518.

III.—Observations on Ulceration of the Jugular Veins, communicating with an Abscess or Open Sore.

Dr. S. W. Gross has collected from various sources, and given an abstract of twelve cases of ulceration of the jugular veins with hæmorrhage into the sacs of closed abscesses, or into abscesses several days after their contents had been evacuated, or into acute and chronic ulcers. The majority of these occurred in children in connection with grave forms of scarlet fever, while others were due to the extension of inflammation to the walls of the vessels from scrofulous sores and abscesses. All the cases proved fatal. Of these twelve cases, eleven affected the internal jugular vein, and one the external. Nine cases were examples of ulcers of the internal jugular vein from inflammatory action extending to its walls, communicating with closed or open abscesses or ulcers, and occurring in children after an attack of scarlet fever. The single case relating to the external jugular vein in connection with the same disease, presented the interesting feature of being an example of hæmorrhage into the cavity of closed abscess, the blood flowing therefrom immediately upon an incision being carried with it. The above ten cases occurred in early life, between two and thirteen years of age. The two remaining cases of ulceration of the internal jugular vein were attended with fatal hæmorrhage, due to the extension of the morbid action from open scrofulous abscesses or ulcers to the walls of the vein; these cases run a chronic course, and occurred in adult subjects.

The efficient cause of the destruction of the coats of the vessels was, in eleven instances, diffuse cellulitis following their course, and that disorder in at least ten of the cases must be regarded as one of the secondary expressions of the morbid poison of scarlatina, developed immediately after its termination, or during convalescence from it.

Hæmorrhage from perforation of the jugular veins may be looked for within the first week after the so-called resulting abscess has been evacuated, surgically or spontaneously. In two of the cases the bleeding was immediate, and in one it was deferred to the seventh day, but the average date of its appearance was the fifth day.

After hæmorrhage has once occurred a fatal result may be anticipated. In three instances it was immediate, two of these being children who were found dead in their beds; one was fatal in a few hours, while in six cases death varied from thirty hours to five days after the first hæmorrhage, the average being the third day, and the death due to repeated recurrence of loss of blood.

Three distinct pathological processes are probably included in the perforation of veins; in one, the vessel appears to have been affected with limited necrosis, from the cutting off of its vascular supply—an eschar of upwards of an inch in extent having been plainly visible before the fatal termination. In another instance, the enlarged and softened internal jugular vein gave way from the withdrawal of the contents of an overlapping abscess, through which it lost its support. In all the remaining instances the ulceration was due to progressive inflammatory charges, or diffuse (suppuative) phlebitis.

In only two cases of the entire number was there thrombosis of the affected vessel. The appearance of the ulcer in the vein is noted in eleven cases; in that of the external jugular vein the vein was perforated like a sieve in a space of three quarters of an inch in extent; in one, there were two openings in the internal jugular vein, of the size of a pea, and in the form of a slit; in all of the remaining cases the ulcer was single, two being circular, while the rest were oblong or ovoidal, and varying from four to twelve lines in length.

Introduction of air through the perforated vessel occurred in one case only, and was attended with alarming syncope, but restoration was effected by stimulants.

The practical lessons to be deduced are:

1st. That acute, destructive inflammation of the tissues of the neck and deeply-seated abscesses, which has existed for some time and suddenly takes an acute action, may, if unchecked in their progress, lay bare and perforate blood-vessels, and that this result is to be feared, more particularly when diffuse cellulitis follows grave forms of scarlatina or other acute specific diseases.

2nd. That scrofulous abscesses and ulcers are not always indolent, but may, under unfavorable circumstances—that is, in an enfeebled, broken-down condition of the system, rapidly assume phagedenic action, and lead to the same complication.

3rd. That the large arterial and venous trunks are more liable to be involved than their branches.

The treatment will be, first, to direct measures to prevent the accident, by cutting short or aborting the diffuse cellulitis during the stage of exudation, by incisions carried through the deep aponeurosis, to relieve tension, care being taken to arrest any hæmorrhage by light and agreeable compression. During the suppuration and mortification incisions are also imperatively demanded, with the use of antiseptics; attention to general health by hygienic and medicinal measures.

The second object will be to arrest hæmorrhage when the vessel has been perforated. Little can be said, as all the cases proved fatal. In all instances in which it is possible a ligature should be cast above and below the dead portion of the vessel, in preference to other hæmostatic agents. It is perfectly safe, and is not open to the objections which can be justly urged against compression. Should compressive treatment be deemed advisable, an assistant should place his finger on the vein above the opening, to prevent further loss of blood. A small piece of sponge or lint wrung out of a dilute solution of perchloride of iron should then be held in contact with the orifice until it adheres, when it is to be supported by a compress retained by adhesive strips.—*American Journal of Med. Science*, April, 1871, p. 337.

IV.—*On resection of the œsophagus in cases of cancer* of that tube. By Dr. Thomas Bilroth, in the current 'Archiv f. Klin. Chir.,' by Langenbeck, 1871, p. 65.

The author remarks that, when examining persons who had died of cancer of the œsophagus he was struck with an idea whether it were not possible to extirpate that portion of the œsophagus. He states that, as a rule, cancer of the œsophagus does not involve the lymphatic glands, but that it remains chiefly confined as a purely primitive disease. In such cases bougies, œsophageal tubes, &c., have to be passed, so as to dilate the constricted part, and thus prevent starvation; but too often the evil is rendered worse by such proceedings, in consequence of the irritation caused by the frequent passage of instruments setting up ulceration and softening of the new growth.

The failure of œsophagotomy and the opening of the stomach by abdominal section having proved hopeless, he was disposed to entertain seriously the question of resection of the œsophagus, and removal of the disease itself. He considered that after extirpation of a portion of the entire circumference of the œsophagus, the lower end of the tube must be drawn upwards through cicatricial contraction, and thus a ring-like stricture remain, which could be readily dilated by bougie without danger.

However, although there are not over many cases of cancer involving the œsophagus high enough up for the operation, still there are many in which the operation might be undertaken in order to save life.

In order to carry out this idea this operation was performed on dogs, so as to make observation as to how the cicatrization would be accomplished. The first operation was made upon a middle-sized dog, and the animal seemed to be going on favorably and satisfactorily, as far as could be desired; but, unfortunately, on the fifth day, the servant, who was in the habit of feeding the dog by injection of milk through the œsophagus, introduced by the wound, passed the tube into the mediastinum, and injected the milk into this space. The animal died, in consequence of this error loci, from mediastinitis and pleuritis.

The second case was operated on in April 21st, 1870. The animal was a large yellow dog, and was placed under chloroform. The œsophagus was laid bare on the left side of the neck, and was pretty deeply situated. With the finger and the handle of the scalpel the œsophagus was separated from the surrounding parts to the extent of two inches. The tube was then cut across *in toto*, and one inch and a half of its calibre completely removed. The lower end of the tube was fastened by two stitches to the integuments at the lower end of the wound, so as to have a more ready means of introducing the œsophageal tube in order to feed the animal artificially. On April 26th a soft bougie was able to be passed from above through the mouth, and, by the assistance of the finger in the wound carried downwards into the stomach. The sutures were removed at the end of a week. Subsequently there occurred enormous mucous discharges through the wound, apparently coming up from the stomach. By degrees this discharge subsided entirely, and the wound contracted; but the wound did not heal rapidly, owing, probably, to the dog being kept with other dogs, and allowed to partake of their food, so that he could not well swallow the bony and fleshy masses; in fact, these stuck at the wound, and had to be removed therefrom with much trouble. At the end of June the fistula became completely closed. The passage through the œsophagus was daily kept open by means of bougies of the thickness of the little finger. The dog could eat meat, potatoes, &c., and swallow well, and became in good condition. On July 26th the dog was killed by cyanide of potassium. The preparation is delineated in a plate accompany the communication; and Bilroth remarks that it shows that his proposition on the healing process after resection was correct, viz. that there was a simple, fine, ring-like cicatrix, scarcely half a line broad, which remained amenable to extension. Bilroth says, in conclusion, "I should feel justified, after such results, to undertake the operation of resection of the œsophagus in certain cases."

Amputation of Redundant Scrotum.—Dr. Henry, Surgeon to the New York Dispensary, would revive a method of treating varicocele originated by Sir Astley Cooper, but which has long since fallen into disuse both in this country and elsewhere. For the purpose of the operation he has devised a "scrotal forceps," whereby its performance is said to be rendered easier and safer.

The treatment consists simply in enclosing within the blades of the forceps or clamps so much of the skin of the scrotum as can be spared with advantage, cutting it off with a pair of strong scissors, stitching up the incision closely, and then removing the clamp and dressing the wound on ordinary principles. That such an operation cannot be free from danger—danger of cellular inflammation—seems certain; and so long as we have safe and simple means of dealing with varicocele by obliterating the veins, there can be no advantage in returning to a more severe and hazardous method of treatment. After all that can be said in favour of the removal of redundant skin, it may very well be asked whether this does more to effect a cure of the disease than can be accomplished by cold sponging and a well-adjusted suspensory bandage. For it deals, not with the enlarged or tortuous veins, which are the cause of the mischief, but only with the pendulous scrotum, which they have produced. We are inclined, therefore, to think that the operation which is at present in use for obliterating the veins, and which has been carried to a great degree of perfection, is not only the simplest and safest, but that it goes most directly to the root of the malady, and is the most likely to lead to a permanent and satisfactory cure.

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Sull'ultimo Stadio del Colera Asiatico, o stadio di morte apparente dei colerosi e sul modo di farli risorgere. Memoria del Prof. Filippo Pacini. Firenze. 1871.

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Welche Zelle in den Pepsindrüsen enthalten das Pepsin. Von Ernst Friedinger.

Sopra un Caso di emisezione del Midollo Spinale nell'Uomo. Lettera del Prof. Pietro Burresi al Prof. M. Schiff.

Clinica Medica di Siena. Diretta dal Prof. P. Burresi. I. Sugli Effetti Terapeutici della Inalazione d'Ossigeno. II. Percussione del Cuore.

Reports, Journals, Reviews, &c.

Sixth Report of the Quekett Microscopical Club, and List of Members. July, 1871.

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Report of the Royal Lunatic Asylum of Montrose.

Report of the Association of Certifying Medical Officers for the Year 1871-2, with Annual Address by the President.

Weekly and Quarterly Returns of Births and Deaths, &c., from the Registrar-General.

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L'Abeille Médicale: Révue Hebdomadaire de Médecine et de Chirurgie, July—October.

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O Correio Medico de Lisboa. September to November, 1871.

CORRIGENDA AND ADDENDA.

No. XCVI, p. 467, line 26, *instead of* "as many cases of cholera did not subsequently follow in the city," *read* "as no case of cholera subsequently occurred in the city itself."

Id., p. 468, line 11, *dele* "but was not successful in 1857." This sentence conveys a wrong impression, inasmuch as the partial evacuation spoken of was not put in force at all in 1857, but for the first time in 1866.

Dr. Schleisner, in noting the above errors in his paper, takes occasion to state that the same measures have been taken at Copenhagen in the presence of cholera in various parts of northern and eastern Europe during the past year (1871), and have been followed by complete success. To show that the city enj[illegible] [illegible]unity due to other causes than the preventive measures described i[illegible] Schleisner recals the fact that in the epidemic of 1853, when the [illegible] at 150,000, there were 7219 cases of cholera, and as many as 47[illegible] from.

In the present number, p. 69, line 26, *instead of* "American," r[illegible]

285

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1872.

Analytical and Critical Reviews.

I.—Reynolds's System of Medicine.[1]

The nature of this work precludes a general criticism, each separate portion standing on its own merits, and apparently never being sacrificed to or aggrandized at the expense of another.

"Diseases of the Digestive System" are continued in articles by Dr. Squarey, "On Diseases of the Mouth, Fauces, Pharynx, and Œsophagus." Here is condensed a fair *résumé* of the usual teaching at metropolitan schools on the affections of these parts. We confess, however, to a sense of disappointment that the opportunity afforded for discussion of the lesions of a visible mucous membrane was not taken, so as to place before us the more recent views on catarrhal and other morbid states of this tissue. The mouth shares with the eye the privilege of exhibiting normal and abnormal processes in action; so that we are not referred to fatal cases and post-mortem appearances for our information as to what is going on. We trust this omission may be made up by a general article on "Inflammation," or something of the sort, in the fourth volume.

We really must beg Dr. Squarey's pardon while we make his writings the text of another complaint. It concerns a fault which runs through not only his essay, but more or less the whole volume; and not only the whole volume, but all English medical literature. In treating of therapeutics, no order is observed, and no comparison is made of the different activity of reagents. If the writer thinks them all equally useless, let him have the courage to say so, and to leave the page blank for other purposes. If he has confidence in their efficacy, let him not say, this "*may*" be given, or that "*may*" be used,

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[1] *A System of Medicine.* Edited by J. Russell Reynolds, M.D., F.R.S. Vol. iii, containing Local Diseases (*continued*). 1871.

one after another to a dozen drugs, but let him urge instantly the importance of pressing one to its logical consequence. Let us not have such uncertain sounds as in the following paragraph about Ulcerative Stomatitis:

"Of the topical applications, powdered alum, nitrate of silver, nitrate of mercury, and hydrochloric acid may be named, and of these, alum, either used as a powder or in the form of a wash, will generally be found quite sufficient; but if the ulcers be slow to heal, their surface should every now and then be cauterized with nitrate of silver. Gargles of chlorate of potash, from five grains to one scruple to the ounce, or even stronger, may be used, *other means failing* (!!)"

Does the author really mean that the use of chlorate of potash should be postponed till such a painful application as nitrate of mercury has been premised? It is a loose mode of expression, sadly common, which we would gladly see reformed.

"Enteralgia" is an essay by Dr. Wardell, of Tunbridge Wells, on a disease very important from its frequency, and from the likelihood of its symptoms being misunderstood. The scattering through several volumes of the various neuralgies which are pathologically related to each other, such as hemicrania, hepatalgia, sciatica, colic, lumbago, angina pectoris, &c., has this advantage, that they are not forgotten in the formation of a diagnosis; for, by the catenation of literary ideas, they are associated in the mind with conditions which they resemble, and are likely to be mistaken for. Dr. Wardell rightly holds that pain in the abdomen, which is variously called belly-ache, gripes, colic, spasmus intestinorum, ileus spasmodicus (*Sauvages*), spasmus ventriculi (*Wiessner*), &c., to be a true neuralgia. And in its pathology he recognizes several distinct classes of causes—first, those which mechanically distend the bowel, such as the sudden secretion of a large quantity of flatus from the blood, which partially paralyzes the peristaltic movements, as retained urine paralyzes the bladder; secondly, those which paralyze by reflex action, such as harmful ingesta; thirdly, those whose depressing influence is conveyed through the blood, as in lead poisoning; fourthly, where the nerve tissue is imperfectly nourished, as in anæmia and nervosism; fifthly, where the mind withdraws from the corporeal substance the normal use of the involuntary nerves, as in emotional hysteria and melancholia; sixthly, organic lesions of the cerebro-spinal axis (?); seventhly, "reflex" or misplaced pains from lesions of parts normally insensitive, as the uterus or kidneys. We do not mean to say that Dr. Wardell has thus formulized the causes, but we have taken the liberty of orderly arranging and condensing his pages on the pathology of enteralgia. Recognizing its intermittent and neuralgic character, and with such just views of its etiology, it is strange that Dr. Wardell does not in the paragraphs about treatment make his science bear fruit. He tells us of anodynes and the usual list of antispasmodics (we will not repeat this list here; it is described pithily by Sydenham as "Quæcunque fœtent"[1])—but why is no mention made of cinchona, quinine, and, above all, of arsenic? The writer must surely have heard of these remedies being administered, and if he does not approve them should say so.

"Enteritis," by Dr. Bristowe, is a model. The subject is a most difficult one to treat satisfactorily in a work of the kind represented by the 'System of Medicine;' but we must say that the author has completely succeeded. The word, or its equivalent, inflammation of the bowels, is of ancient date, and from the earliest times until now has had a loose and various application: so that there was the constant temptation to include matter which has been, or will be, properly handled under more precise headings; or, on the other hand, to treat too lightly the clinical importance of the fact of the ilia being really inflamed as a consequence or part of the special affections elsewhere noticed. Dr. Bristowe has avoided both these pitfalls. While leaving catarrhal and croupous conditions of the mucous membrane to be discussed under "Diarrhœa" and "Dysentery," he has yet pointed out the necessity for remembering that the bowels really are liable to those interstitial changes whose commencements we group together as "inflammation," and which may lead consequently to persistent modifications of the tissue; and while identifying many of the post-mortem appearances anciently assigned to enteritis as really due to the objective causes of enteritis, has deduced most valuable therapeutical inferences from the frequency of its presence. He well points out that intestinal obstructions may last for many weeks, even in an absolute degree, without destroying life; whereas, if the bowel becomes inflamed, the duration of the case is usually measured by days. Its rapid fatality is doubtless due in a serious measure to the perturbative treatment still often adopted in cases of obstruction, and therefore the author gives a solemn and earnest warning against the use of purgatives or any other mode of moving the bowels about.

"It has been shown quite conclusively, principally by experience derived from the after-treatment of strangulated hernia, that it is always dangerous to endeavour to propel fæcal matters through an enteritic length of bowel, that in most cases the effort is useless so far as their effectual propulsion is concerned, while, by the augmented muscular and excretory action which is thus produced in the bowel

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[1] 'Diss. Epist. de Affect. Hystericâ,' 120.

above, the diseased tract below becomes more and more softened, congested, and inflamed, not unfrequently becomes ruptured, and, at the least, has its progress towards recovery delayed. And, indeed, when one considers the great length of time during which constipation may continue with little or no influence on the general health, how long patients with impassable stricture of the bowel may continue to survive, it must be obvious that the constipation of a disease of so short a duration as enteritis is not of itself a source of danger. Clearly, if the patient is to get well, his recovery must, in the first instance, be dependent on the recovery by the diseased bowel of its healthy tone and capability of peristaltic action; and to this end our efforts must be directed."

The article on "Obstruction of the Bowels," by the same author, may be considered as a part of, or natural complement to, the former. It discusses the most frequent cause of enteritis, and its pages on pathology and treatment are largely influenced by the views which we have already quoted. As might have been expected, surgical interference is looked upon as a very fragile, if not broken, reed. The authorities quoted are, as a rule, Dr. Brinton, Dr. Barlow, and Dr. Fagge, especially the first named.

"Ulceration of the Bowels," also by Dr. Bristowe, in its pathological part usefully contrasts the different appearances presented by ulcers arising from tuberculosis, typhoid fever, and other causes. The remarks on therapeutics seem to be intentionally meagre, as by pointing rather to principles than to details, the moral might be enforced that the treatment should be of the initiatory disease, not of the secondary.

Then follow four pages on "Cancerous and other Growths of the Intestines," the subject having only a pathological and no clinical interest.

"Diseases of the Cæcum and Appendix Vermiformis" gives a full account of the acute evils resulting from inflammation and ulceration of those parts, typhlitis and perityphlitis. But we miss what we should have been glad to have had, namely, information about the chronic ailments of this part of the intestinal canal. Even the result of typhlitic ulceration in chronic fistula is scarcely alluded to; and atonic sluggishness, dilatation, and other defects of defæcation indubitably referable to the cæcum, are passed over in complete silence. We look upon this as a serious omission; for led by the index to this volume, the student would be in danger of learning that constipation was a disease of the small intestines, and that its only injurious result was ileus. We would suggest to the editor that the 'System of Medicine' will be very incomplete without an article on deficiencies of defæcation, under some heading which will involve a thorough examination of the subject, without robbing former contributors. We should have been glad also to have heard something about neuralgia of the cæcum, the chronic form of which is by no means rare, and important from the paralytic distension of the part which it causes, simulating tumour and fæcal masses.

In the succeeding essay on "Colic," Dr. Begbie tells us that it is "a disease which is to be distinguished from" enteralgia or neuralgia of the bowels. We should have been glad to know "How?"—for his page on the pathology of the ailment appears to exhibit them as identical. The article, and especially its etymological part, would have been better devoted to showing that the symptoms described by former writers, and by the current writer himself, have no pathological connection with the colon or function of defæcation at all. When he tells us that the adjectival expressions "Colica Hepatica," "Colica Nephritica," "Colica Uterina," are "eminently faulty, and it is desirable that their use should be entirely abandoned," we feel that he is bound to make out a much stronger case in favour of the substantive "Colica," derived (as he derives it) from Colon, than he has done, before he allows it a superiority over the localizing epithets he falls foul of. Inasmuch as "Dolor Colicus" is entered by Dr. Wardell among the synonyms of enteralgia not 100 pages back, we think this essay might have been omitted altogether.

The same may be said of "Colitis," the post-mortem appearances of which there is allowed to be no possibility of distinguishing from the results of dysentery, and the symptoms of which no attempt is made to differentiate in the page devoted to it.

In reviewing the first volume of this work we remarked on the fact of "Dysentery" and diarrhœa being treated of as if they were only tropical and epidemic diseases. Another article here on "Dysentery," by Dr. Warburton Begbie, is designed to remedy our complaint; which it does, and adds some interesting historical facts concerning a plague which will always attract the attention of mankind. We should have been glad to have read, either in this article or in another supplementary one, something about a form of dysentery very fatal, though in the view of a mere morbid anatomist of inferior moment, namely, that which affects suckling and teething infants from improper food.

We think it is judicious in the editor to insert, even in a strictly medical work, an article on "Diseases of the Rectum and Anus." The question of diagnosis often occurs to the physician in respect of them; they are often capable of relief by purely medical remedies, at the same time with the medical diseases they are concurrent with; and, indeed, surgical interference so often does them harm, that the purest of the pure will welcome this pithy practical *résumé* by Mr. Curling of what his life-long experience selects as the most important points of the subject.

As the separate essays are not placed in strictly anatomical order, we have not noticed, till arrival at the rectum excluded all hope, the absence of the duodenum from the list of viscera whose morbid states are commented upon. To judge by the already published volumes, one would suppose that congenital malformation, the occasional presence in it of a worm which has never been seen by an Englishman, and its perforation, which are incidentally alluded to in discussing others matters,[1] are the only diseases affecting the duodenum independently of the ilia. Its special physiological relations to digestion, the frequency with which catarrhs of the part are induced by meteorological changes, its common atony from psychical causes, its causation of jaundice, its problematical connection with emaciation, its neuralgia (vulgarly called "liver"), its acute ulceration after burns, the special symptoms by which cancer here is differentiated, of all which we cannot discover any mention, would make up an excellent article in the inevitable appendix.

"Intestinal Worms," by Dr. Ransom, is written *con amore* on a most enticing subject. Can we pay the author's literary talents a higher compliment than to say that he has condensed all that is important into twenty-five pages? The history of these disgusting guests of ours always reads like a fairy tale. The lamp of science, which has remorsely scattered the shreds of poetry from so much of our forefathers' physical notions, has only added to the wonder excited by the story of their lives. They are shorn of the mysterious interest which their apparent origin by spontaneous generation once gave them, only to become tenfold more romantic by the strangest travels and changes of residence in the shape of eggs, larvæ, and perfect animals. Though a great number of "tales of horror" related concerning what were of old considered intestinal worms are shown to be deserving of scepticism, yet a nearer and deeper source of interest is opened up by the facts of social life which our lowly fellow-creatures reveal. "Every person who is shown to be infested by those very common Entozoa *Oxyuris vermicularis* or *Trichocephalus dispar* is thereby demonstrated to have swallowed minute portions of his own or another person's fæces." With that sentence Dr. Ransom concludes; and we think that the curdling of the blood which it will produce in

[1] Viz. in vol. iii, p. 72, vol. iii, p. 202, and vol. ii, 913.

the reader is an effective substitute for the sensational matter concerning *Trichina spiralis,* of which it has been determined by the editor to make a separate article.

Dr. Ransom does not think there is sufficient evidence for attributing to the intestinal parasites found in the stools the more severe, especially cerebral, symptoms, with which they are credited by writers up to the present generation. Microscopic investigation finds these creatures to be so common without any signs at all of their presence, and the indubitable local phenomena are so often unaccompanied by loss of health, while at the same time the severe symptoms so generally are explicable by other causes, that he would condone their serious offences as not proven. However it must be allowed that loss of sleep from anal titillation, abnormal lustfulness, innutrition and bad temper are no slight evils, and we cannot rank as superfluous any attention paid to proper means of avoiding them. It is a happiness to reflect that these means are far from being abhorrent to our better tastes: cleanliness, good cookery, the self-respect of not doing in private what one abstains from in public,[1] and the careful supervision of children, are worthy of advocacy for so many other reasons than the exclusion of worms, that we see no difficulty in connecting their complete extinction with the future advance of civilization. It is interesting as a matter of sanitary police to learn that complete submersion in water for a few days kills the ova of the thread-worm (*Oxyuris vermicularis*), though they retain their vitality through desiccation, freezing, or a considerable rise of temperature; that it is through human excrement that pigs are infected with cysticerci, and that they revenge themselves on their poisoners by supplying in cysticercous or "measly" pork the larvæ of human tapeworm. The obvious inference is the importance of letting wet sewage stand for a time before it is distributed on plants eaten raw, and of preventing human and porcine ordure from becoming dust to be blown about by the wind.[2]

"Peritonitis" by Dr. Wardell is a difficult matter to review, and still more difficult to write. The difficulty consists in what

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[1] "One would not surely be nasty when alone." (Chesterfield's 'Letters to his Son.')

[2] It seems likely also that carnivorous animals contribute to the distribution of tænia; the larvæ, swallowed by them in raw pork or beef, may pass alive through their digestive canal and adhere to green vegetables cultivated for food. A few years back we attended a boy of four years old for tapeworm; and having, in a case so exceptionally young, the opportunity of accurate information on the daily habits, we were able to trace the probable source of the worm to the child's habitually eating the nasturtiums growing in a sculptor's yard, which was frequented by the suburban dogs as a privy. The danger of the *tænia echinococcus* being thus introduced is still more serious, as is urged in the article on "Hydatids of the Liver."

is technically termed "making points." An idiopathic form of the disease is exceedingly problematical, and, if it exist, is certainly of minor importance. It cannot therefore be made the typical example, with which in illustration to compare other forms. Without denying the possibility of its being primary, or absolutely incapable of connexion with a foregoing or coëtaneous disease, Dr. Wardell occupies his pages by steadily going through the several instances in which affections of the underlying parts cause inflammation of the peritoneal sac, extracting with great judgment the bulk of his matter from the several special authorities on the diseases of those parts. The pages on therapeutics are more *ex cathedrâ*, and in prompt and decided tones urge the duty of promptitude and decision of purpose in the selection of those agents which are the most effective auxiliaries in combating the rapid progress of serous inflammation. Bloodletting, both local and general, is advocated for patients of the sthenic type, and its application recommended to follow immediately on the recognition of the lesion—"as soon as possible after the pulse has become hard and quick, the pain urgent," &c.

Dr. Wardell mentions, but does not commend, the use of cold, externally in the way of water compresses and evaporating lotions, and internally by iced enemata. Doubtless as an universal remedy it would seem hazardous, but some cases not yet defined appear to have done well in the hands of Dr. Abercrombie, Dr. Sutton of Greenwich, and M. Smoler of Prague. We would suggest that probably thermometric observations on the general temperature, applied to the disease, would throw light on this point of therapeutics.

After bloodletting has been premised, opium is advised in unsparing doses without delay or hesitation. So far we have gone along with the author without a check. But here we are pulled up by an unpleasant change of manner *apropos* of mercury: he begins a clause on this most serious question thus—"*If* we wish to influence the system by mercurials, one grain of opium, and three grains of calomel, &c., &c." "If"!!—Why the very point the reader prays to have decided, is whether mercurialization checks serous inflammation or not. And when he is held capable of determining that momentous question, surely he may fix the quantity without hesitation. A sentence, which passing over the weightier matter, merely tells us that three grains of calomel every four *or* six hours, *or* two grains and a half of calomel every second hour, accompanied by inunction with linimentum hydrargyri, will mercurialize the patient, is simply frivolous. The only excuse we can make is that a page or so on the question of mercury has dropped out in arranging the type. Let us have it in the "Errata" next volume.

After a few short articles on irremediable pathological states of the peritoneum by Dr. Bristowe, we have one by the same physician on "Ascites." In the treatment of that disease we are happy to notice that tonics, such as quinine, iron, and cod-liver oil are commended with a view of expediting the absorption of fluid.

"Hepatalgia" might have been with advantage fused with "Enteralgia." For it is evident that the deep situation of the liver within the body must render it next to impossible to localize such an impalpable a thing as pain in the hepatic nerves. Even Valleix, whose business as a systematic writer, and French birth, naturally dispose him to differentiation, discredits its existence as a separate malady. We should be sorry, however, if the reader fail to see one sentence at the end of the article, which gives the experience of the author on the use of chloride of ammonium in painful affections of the right hypochondrium, when accompanied, as they not rarely are, by functional hepatic disturbance.

The sheets on "Jaundice" and "Biliary Calculi" are centos of the established works of the present day on those subjects, but they supply us with less of the author's opinion than we had hoped to see from one of such wide-spread Indian experience as Dr. Goodeve.

Dr. Maclean's essay on "Suppurative Inflammation of the Liver" is redolent throughout of personal experience, and has a corresponding value to the practitioner. We are not alluding merely to the cases of diagnosis which he relates so pithily and well, but more especially to his remarks on treatment. Without hesitation he tackles the important questions of bleeding and mercury, which he unhesitatingly condemns. He does this on the real ground of personal experience, for his argument from Cohnheim's researches, on the identity of white blood-cells with pus-corpuscles, is rather beside the mark. He continues, "Bleeding and the administration of mercury being thus objectionable, what remains? I answer, Ipecacuanha. For years past in my lectures at Netley, I have urged the free use of this invaluable remedy, not only in dysentery, but in suppurative inflammation of the liver. I give it in the same large and efficient doses as in dysentery, from 20 to 25 grains, and even more, &c." When an abscess has formed, Dr. Maclean raises a warning voice against unnecessary surgical interference. He says he has tried by valve-like openings, by drawing off small portions of the pus at a time, by closing the opening with collodion, and by canulæ provided with stop-cocks, to prevent air entering into the cavity, but all in vain. He suggests, and indeed has used successfully, the syringe recently introduced by Dr. Bowditch of Boston for evacuating the pleura. But these expedients failing, his inclination evidently is to leave the evacuation of the abscess to Nature, who working from inside, at her leisure, with the choice of the convenient spot, and the opportunity of making several holes, or one, as suits best, has an indubitable advantage over the shrewdest diagnosticator. Another *non nocere* with which we may credit Nature, is that she will probably not puncture the gall-bladder by mistake, an accident which Dr. Maclean has witnessed in France.

The article on "Fatty Liver" by Dr. Warburton Begbie is very valuable, especially to junior practitioners, by the warning which it indirectly but earnestly puts forth against over-treatment. The portion on the "pathological import" of the lesion shows clearly that the feature which gives it the name is the least important of all. "It is not to be lost sight of," says the writer, "that the presence of fat in the liver-cells is not morbid; but that the increased or excessive amount of fat in the organ constitutes disease. Deposition of fat in the liver-cells is a natural physiological process—one which is continually at work, and the activity of which, with its subsequent reabsorption of the fatty matters is determined by the nature of the food, whether rich in fat or not, and the consequent impregnation of the blood with the same material." Attention is directed to a diagnostic mark of this condition, first observed by Dr. Addison, namely, that it occurs mostly in suet-pudding-faced individuals:—"It is earliest observable in the integuments of the face and backs of the hands. To the eye, the skin presents a bloodless, almost semi-transparent, and waxy appearance; when this is associated with mere pallor, it is not very unlike fine polished ivory; but when combined with a more sallow tinge, as is now and then the case, it more resembles a common wax model. To the touch the general integuments, for the most part, feel smooth, loose, and often flabby; whilst in some well-marked cases all its natural asperities would appear to be obliterated, and becomes so exquisitely smooth and soft as to convey a sensation resembling that on handling a piece of the softest satin."

With respect to "Cancer of the Liver," which is handled by Dr. Warburton Begbie, the main points are diagnosis and prognosis. It is of the utmost importance to both patient and physician to ascertain whether he has to deal with an uniformly fatal disorder or with one of the seven curable or the three less rapidly destructive lesions which, according to Frerichs, may be mistaken for it—viz. (1), waxy liver, (2) syphilitic hepatitis, (3) liver depressed from habit of tight lacing, (4) hydatid disease of liver, (5) hepatic abscesses, (6) dilatation of the bile-ducts and gall-bladder, (7, 8, and 9) cancer of the omentum, cancer of the stomach, and cancer of the right kidney, (10) accumulation of feculent masses in the transverse colon. For the *means* of diagnosis we are referred to the articles containing separate descriptions of the affections themselves. Now we must warn Dr. Begbie, that is a mode of giving information most objectionable, not only to a reviewer, but to a general reader also. A peevish expression escapes him as he sees the prospect of looking in the index for the references to ten articles. But what is he likely to "say in his haste," when in the first of these he reads (page 967) no detailed comparison of the two lesions; when on syphilitic hepatitis (No. 2) there appears to be no article at all; nor is there on No. 3, the lesion not being named in the index? By the time that No. 4 is arrived at, calm thought is out of the question. Besides which, some of the matter probably referred to is not yet published, some is in former volumes and by different authors. This is a glaring instance of the difficulties engendered by the anatomical classification of disease, and the impossibility of avoiding repetition without injuring the usefulness of the work.

A propos of "Hydatid Disease of the Liver," Dr. Warburton Begbie has some interesting remarks on the geographical distribution of the echinococcus: these would be a guide to the adoption of means to prevent its spread, if it is found dependent on varieties of social habits. Now, recent investigations seem to have made it certain that this parasite of the interstitial structure of man is the larva of a very small tape-worm, not a quarter of an inch long, which attains its perfection in the intestines of the genus Canis. But how do dogs get hold of the larva? After we have extirpated the wolves that "tore up the liver and lights" of our Red Riding-hoods, do their domesticated cousins still devour our entrails? No; but the tænia echinococcus is found in the larva state in the sheep as well as in man, and it is from the offal of butchers' slaughter-houses that dogs probably get infected. The inference is obvious, that in proportion to the proper management of these sources of infection the disorder is rare.

"Diseases of the Pancreas" is a careful *résumé* by Dr. Wardell of the more cautious handlers of this obscure subject. The rasher speculations of Dr. Dobell do not appear to find favour, as they are not alluded to; and the statement that "there are no medicines which have a special power of counteracting its maladies" would seem to indicate an active unbelief in the modern therapeutics of the gland.

We now arrive at Diseases of the Respiratory System.

Dr. Morell Mackenzie's sedulous prosecution of the pathology and therapeutics of the larynx makes him the most suitable person to undertake the lesions of that part. It is a well-defined subject for a monograph, and we are glad that he takes the whole of it, which it is scarcely necessary to say is effectively, fully, but always pithily discussed. A certain tincture of egotism was inevitable, and is not ungraceful, in one who has been so much before the public in relation to the organs of voice.

Sir William Jenner on "Emphysema of the Lungs" is still more worthy of unqualified commendation. Since the time when he gave the public his observations in the 'Medico-Chirurgical Transactions,' on the degenerations of the pulmonary tissue, he has made this disease one of his specialities, and has at his leisure examined and compared with nature all that has been published by authors of note, as his list of authorities consulted shows. And what is the result? An exhaustive collection of conflicting opinions, an elaborate subdivision of forms on minute differences, winding up with a list of drugs which "*may* be administered"? No—six-and-thirty pages from which not a word can be spared, and which therefore impress themselves upon the memory with a sharpness of outline equalled only by the aphorisms of Hippocrates. The pathology is illustrated by three striking woodcuts, just enough to be remembered and to bring before the mind of the practitioner what is inside the chest he is examining. Care is taken in the selection of epithets to convey as much of the true state of knowledge on the subject, and as little fallacious assumption as possible. What can, for example, be more graphic than the division of vesicular emphysema into "large-lunged" and "small-lunged"? Autopsies immediately recur to the mind in which the pale lungs crowded out, like opaque soapsuds, as the sternum was removed; others in which equally emphysematous pulmonary tissue fell back black and flabby, and left a hollow space; and we feel prepared immediately by our clinical recalled experiences to endorse the following pithy sentences:

"General large-lunged vesicular emphysema is a very serious disease. The symptoms directly due to it are grave; the diseased conditions dependent on it for their origin are frequently fatal.

"Thus a large proportion of cases of heart disease have their starting point in large-lunged vesicular emphysema.

"It rarely occurs in a marked form before the middle of life, and more commonly affects fat people. Lungs, the subject of this form of vesicular emphysema, are larger and drier than healthy lungs.

"The parts uncoloured by pigment are paler than healthy lung.

"The lungs overlap the pericardium to a considerable extent, and meet above it even to near the top of the sternum; they have a downcushion-like feel, and retain the impression of the fingers. When the thorax is opened they contract less that healthy lungs do under like circumstances.

"Large-lunged vesicular emphysema is, in the great majority of cases, preceded by attacks of bronchitis, by congestion of the lungs, by dry winter cough, or by chronic bronchitis; that is to say, by diseases having as immediate consequences toughening and thickening of the tissues of the lungs, and severe cough; in other words, diminished elasticity of the lungs, and powerful expiratory efforts with closed glottis."

In the few concluding words of the extract which we have made, as a specimen of style, are contained all that the author thinks needful on a discussion often much protracted in systematic works, namely, whether expiration or inspiration is the final determining cause of the breaking up of the pulmonary tissue. We are glad to see the controversy is not deemed worth entering into, as any medical student who has shirked the lectures on the mechanism of respiration, will probably in his boyhood have found out the way to burst an old pair of bellows by converting the tube into a pop gun.

In what follows on small-lunged emphysema the wrinkled and shrivelled subjects of the disease with their small immovable chests, and the length of time they remain under our care, no better and no worse, are vividly depicted.

The therapeutical part of this monograph is particularly good; no drug is recommended without a reason for its use being given in the physiological action which it introduces, not, of course, each time that it is named, but in some part of the rules for treatment.

The pages on "Asthma" have a melancholy interest. The gentle-hearted and accomplished author is dead—"dead ere his prime"—and from the consequences of the disease which he so graphically describes, and the treatment of which no one was so well qualified to weigh, for he himself was the balance and the test-tube.

The culminating point of the article is the "Pathology of Asthma." To this lead the description of the symptoms, physical signs, causes, and indirectly the diagnosis; and from it the treatment and prognosis are deduced. It is not, therefore, exhibiting a brick to show what a house is like, but rather giving an idea of a temple by its pediment, to quote this part as representing the whole monograph. Dr. Salter says:

"Our views respecting this (the pathology) must be greatly influenced by our views of the immediate condition in asthma. My belief is that the immediate and essential condition of the asthmatic paroxysm is a state of contraction of the bronchial tubes. What proof have we of this? In the first place, the sudden induction and remission of the asthmatic paroxysm is consistent with its depending on muscular spasm; in the second place, there is abundant proof that the air in the lungs is locked up, and can neither be got in or out; there is evidently plenty of air in the chest, percussion is even hyper-resonant; the patient is as unable to drive air out as to draw it in, can neither inspire nor expire, cannot discharge breath enough to whistle or blow at a candle or blow his nose. The muscles of respiration tug and labour to fill and empty the chest, but the chest-walls remain almost immovable; the inspiratory muscles cannot raise them, the expiratory cannot depress them. On listening to the chest, we find corroborative evidence of the stagnation of the air. The respiratory murmur is in a great degree lost. This absence of respiratory sound, accompanied by violent respiratory effort, is one of the most striking and suggestive of the facts of asthma. How can we explain it, except by supposing that there is some bar to the ingress and egress of air, and what can this bar be, unless it is spasm of the bronchial tubes? It cannot be inflammatory thickening of the mucous membrane lining them; for the sudden, almost instantaneous, establishment and remission of the dyspnœa is incompatible with this. It cannot be mucous plugging of the tubes; for the attack will often come and go without any expectoration whatever. But we have still more positive and precise evidence of circumscribed narrowing of the air-tubes in the musical sounds that are present in asthmatic breathing. This symptom has all the certainty and precision that characterize physical phenomena, and shows that the air-tubes are the seat of constrictions that throw the air passing through them into vibrations, and convert them into musical instruments; and since these musical sounds are multitudinous, the points of constriction must be many; and since they are constantly varying in character, the constrictions of the tubes must be undergoing similar change. Lastly, the effects of remedies and their nature tell the same tale, and point to muscular spasm as the immediate essential condition. The most powerful remedies of asthma are what are called cerebro-spinal depressants, such as emetics, tobacco, &c., remedies whose direct effect is to relax muscular spasm."

Dr. Salter's last sentences may seem perhaps at variance with our having previously said that he deduced the treatment from the pathology; but the fact is he is speaking above only of medicines for the alleviation of the paroxysm, and that what we refer to are the real therapeutical, or curative, remedies. In prescribing these, the author makes a contribution to rational, as distinguished from empirical, medicine, which shows how truly philosophical a high class of mind is when most practical, and how truly practical when most philosophical.

He divides the curative treatment into—first, the treatment by air, that is, by locality; second, dietetics; third, avoidance of exciting causes. If *vere scire est per causas scire,* still more surely *vere curare est per causas curare,* and we look upon the latter as the characteristic feature of the rational medicine of the present day. The aim is to try and find out what is the megacosmic or external cause of the patient exhibiting certain morbid phenomena, and to divide the attention between the removal of that cause and rendering the sufferer able to endure it without injury. In the instance of asthma Dr. Salter claims no more than the first intention, observing that his recommendations resolve themselves essentially into the avoidance of the provocations of the attacks; their applicability depending upon what, in each particular case, is the special exciting cause. We must, however, remind the reader, not alas! the author himself, that in diseases of habit, of which functional nerve diseases are the very types, each day that the paroxysm is postponed becomes in itself an element of cure, and that in direct proportion to the length of the interval so is the power of future resistance of the affected body.

As a therapeutical classification derived from pathology, Dr. Salter distinguishes secondary asthma into bronchitic, gastric, and cardiac; but he distinctly pronounces for an idiopathic form of the disorder as the best marked, most typical, and characteristic, and at the same time the most severe. He points out the analogy between it and epilepsy, as essentially nervous intermittent diseases.

The views of Dr. Hughes Bennett on the pathology of tubercle are well known, and those who wish to read them pronounced in a condensed form *ex cathedrâ* will find what they require in a contribution from that physician on "Phthisis pulmonalis." Those who expect other aspects of the question, as looked at by Virchow, Niemeyer, Sanderson, and others, to be sufficiently displayed in a 'System of Medicine,' will be disappointed. We cannot agree with the writer that the differences between them are merely verbal. All, however, will be satisfied that the therapeutics represent the best common-sense inferences which can be drawn from a general view of the scientific explanations of the origin and progress of the disease. To treat the patient's body as a whole, to restore that which is deficient in it, in respect of both substance and function, and to shun the obvious external causes of aggravation may be stated to include the whole of the remedial measures which Dr. Bennett considers useful. And with them we may be fairly satisfied, for by such help the evil is so mitigated that one experienced physician gives statistical evidence of the average duration of life in consumptives being actually doubled; while the Registrar-General for Scotland reports a decrease of one third in the annual number of deaths from phthisis. We would not attach too much importance to the exactitude of the numerical results obtained, but they certainly exhibit a marked diminution in the gravity of the disease during the last twenty-five years, that is to say during the period in which this mode of treatment has more and more prevailed.

Dr. Beigel has in an article on "Cancer of the Lungs" made the most of an unpromising subject by industriously collecting all that has been written about it for several years.

The really learned articles of the whole book, however, are the next, on "Pneumonia," by Dr. Wilson Fox. The notes and appendices alone (where counting is easiest) contain upwards of six hundred references to authorities for various statements, while the text also in some parts bristles with names of various degrees of familiarity. And let it not be understood that this is a heterogeneous collection of quotations, like the works of a second-rate seventeenth-century writer; all are carefully selected, so as not to be repetitions, and where known are such as must command respect. This array of colleagues in research somewhat overshadows the author's own name both as a witness and as a judge in the important questions here debated before the reader. Without any affectation of modesty he certainly does keep himself in the background, and we think it right to notice this, because when we do become conscious that the author is relating his own experience, we are struck with the peculiar healthy vigour of observation which it displays, and when we are specially pleased with some judicious apothegm we are usually led to infer from the style that it is the author's own. Yet he does not shrink from his responsibilities; the names of clinicists, such as Graves and Chambers, whose position as summers up of evidence is similar to his own, rarely appear, nor do those of systematic writers, such as Watson and Aitken; but any one whose statistics are valuable is immediately pressed into the service. It is evident that we here possess a monograph of the thoughts of the current generation on the subject that will be absolutely essential as a mine of reference for all who are discussing it for many years to come. We cannot expect another to go so thoroughly into it for a long time.

As to treatment everybody of course quotes pneumonia as the chief battle-field where opposite principles of therapeutical pathology have fought for the mastery. Its frequency, the facility of diagnosis, and the gravity of the symptoms seem to have commended it for this purpose to all parties. And they are right in their selection, when the reckoning of the value of a treatment is made according to the rough average mortality on a large scale. But when you come to judge of the immediate action of a remedy upon this or that part of the disease, or upon its duration in individual cases, Dr. Fox well points out that no malady can well be chosen less suited to afford logical proof, by means of statistics, of the relative value and the curative effects of any system of treatment. An acute disease with a natural tendency, under favourable circumstances, to terminate spontaneously by a sudden crisis occurring at periods varying from the third (? the second) to the seventh or eleventh days, presents the most singular elements of fallacy in reasoning from the phenomena following active interference. The author's reasonings on therapeutics are therefore rational, rather than statistical, and he is quite open to allow the occasional advantage of a treatment whose general adoption has proved injurious. Even the unpopular venesection is liberally spoken of, and allowed to have saved life in cases of apnœa occurring early in the disease. The external application of cold water to reduce the pyrexia, is a treatment we would gladly know more about. Dr. Fox gives only one instance of its employment, and that an unsatisfactory one; and the immediate distress it causes to the patient is not encouraging; but still from experience of it in other pyrexias we know it to be safe and to be worthy a fair trial.

We may observe here that Dr. Fox speaks about this method of treatment as if the low temperature were the only element to be considered—for he introduces the ice bag as an equivalent for aqueous compresses. But it should not be forgotten that warm water also, and probably more generally than cold, has been thought beneficial; indeed by some in the shape of "jacket-poultices" is considered a sort of panacea for pneumonia. It is possible, therefore, that the temperature may be of less importance than the medium of its application. It should not be forgotten that tepid baths were used in Greece for pneumonia by even such an expectant physician as Hippocrates.

We may remark in favour of the wet tepid treatment of pneumonia that its virtues are most visible when employed in the most serious varieties of the disease, namely, the secondary pneumonias — broncho-pneumonia, lobular pneumonia, &c. Now fierce and vigorous therapeutics always vaunt their most startling victories over frank idiopathic cases, which are shown by statistics to be much less fatal in their nature, as may be read in Dr. Fox's words.

That destruction of the lung tissue by its conversion into an impermeable fibrous structure which has been variously sermed "Cirrhosis," "Sclerosis," "Scirrhus," "Induration," "Interstitial Pneumonia," "Melanotic Phthisis," "Fibroid Phthisis" (the last two names being an attempt to combine an anatomical and a clinical momenclature), has its titles here compounded for by the compromise of "Chronic Pneumonia." And Dr. Wilson

Fox again appears as the collector of the material required to illustrate the subject. He confines himself strictly to such forms of chronic induration of the lung as may be reasonably presumed to have been caused by processes in which tubercular changes have had no share.[1] In this sense the disease is of great rarity, and examples of it can only be found in isolated cases scattered in different journals and in monographs on diseases of the lungs. At the same time it has from some accidental circumstances excited lately considerable interest among practitioners, and we may therefore not be considered impertinent if we allot more space to it than its comparative importance to mankind would justify.

Let it be understood that the adoption of the name is not a meaningless ceremony, or to prevent a disease being utterly anonymous when all its previous titles had been rejected as unsuitable. It is upheld that this condensation of the pulmonary tissue, without tubercle, or other heterogeneous formation, is really the result of a pneumonia which has passed into a chronic stage. Other accounts of its origin are fairly and favorably set before the reader—namely, first Sir D. Corrigan's view of it as a growth of fibrous tissue analogous to "cirrhosis" of the liver, independent of any inflammatory process; secondly, that it is a primary "fibroid degeneration" of the alveolar walls. To the first Dr. Fox would reply that the interior of the air-sacs almost invariably, if not constantly, exhibit an accumulation of the products of inflammation, as is shown in the woodcuts inserted in the text. Further, the change in the liver takes place mainly through an increase of fibrous tissue between the acini; whilst in the lung, though some thickening be found in the interlobular septa, the most important pathological alterations are those which occur in the walls of the pulmonary alveoli, which certainly are not interstitial tissue, but correspond rather to the terminal extremities of gland-ducts. In short, in a cirrhotic liver the fibrous thickening is perilobular; in an indurated lung it is intralobular. To the suggestion that this alteration in the alveolar walls is a *primary* lesion, as implied by the words "fibroid degeneration," Dr. Fox also demurs on account of the deficiency of sufficient positive evidence for admitting such a class of lesions into our nosological categories. He says that during many years of much attention to the subject he has never seen any pathological specimens supporting such a view,

[1] The author classes all cases as tubercular which present granulations—grey, or soft, or cheesy—in the lungs or other organs. The question, "What is a tubercle?" is as vital an one to future pathology as "What is a pound?" to political economy. Dr. Fox announces that he will shortly enter the lists with an answer.

and nearly all the cases which have fallen under his own observation "have been connected with previous chronic pneumonia associated with the presence of tubercles."

The reader is probably familiar with the tale of the aged divinity clerk (the bearer of the silver poker before the university preacher) at Oxford, who on his death-bed told his medical attendant, "Ah, sir, I have for fifty-five years been hearing, at St. Mary's, the excellent gentleman in the afternoon contradicting the learned gentleman in the morning; yet, thank God, I remain a Christian." He will be reminded of this, and perhaps also have a struggle to maintain faith in the revelations from professional chairs, when he reads in an article by Dr. Bastian, which immediately succeeds those by Dr. Fox, "It seems expedient to me to do away altogether with the name 'chronic pneumonia,' as an appellation for the pathological changes in question." Dr. Bastian gives his reasons, which may be shortly described as the reading in a reverse direction of the phenomena on which Dr. Fox grounds his argument. Cases which the one calls the rule the other calls the exception, and both are disposed to draw into their meshes instances from previous observers which have an indeterminate clinical history. The truth of the matter is that the post-mortem appearances do not decide the questions at issue, and the disease is too rare to furnish clinical evidence of its primary nature.

We will not presume to give any opinion upon the subject, and think it may be well left undecided till some practical point be found to hinge upon it. Perhaps we may be allowed to suggest that it would have been as well if the editor of the 'System of Medicine' had been actuated by corresponding feelings to spare the public these unripe fruits, and to let the contending opinions balance one another outside, rather than within, his limited sheets. The 110 pages of close print occupied by the four articles entitled "Chronic Pneumonia," "Syphilitic Affections of the Lung," "Brown Induration of the Lung," and "Cirrhosis of the Lung," none of which possess the slightest clinical interest, might with great advantage have made way for several subjects the omission of which can be excused only by want of space. We would specify, as suggested by this part of the work, "Coryza," "Ozæna," "Ulcer of the Nose," "Epistaxis," "Acute Capillary Bronchitis" (which is in the article on bronchitis merged in bronchial catarrh), "Disease of the Bronchial Glands" (in its local aspects), "Disease of the Thymus," "Miller's Asthma,[1]" "Grinder's Rot," and other lesions of the lungs by dust, "Foreign Bodies in the Air-

[1] We do not mean Dr. Miller's *Asthma thymicum*, but that peculiar to the trade.

passages," "Hydatids of the Lung," "Pulmonary Extravasation"; while in the earlier portion of the volume we miss "Lead Colic," "Constipation," "Neuralgia Ani," "Peritoneal Abscess and False Anus," "Hydatids of Peritonæum," "Disease of the Omentum," and "Obesity." We do not see either where diseases of the ductless glands, such as "Goître," are to come in, for they cannot be said to belong to the "blood-glandular system," the lesions of which are promised in the fourth volume. We think also that a 'System of Medicine' cannot be called complete in the present day without at least a short review of instruments of precision, the 'scopes and 'graphs and 'meters and tests of various sorts, which much require valuation and classification. We are the less reticent of our wants, inasmuch as we foresee an inevitable fifth volume looming in the distance.

The *sesquipedalia verba* used in the modern naming of the diseased womb and skin will assuredly require a volume to themselves, instead of being disposed of, as Dr. Reynolds proposes, in the fourth, in the wake of the circulatory, the blood-glandular, and the urinary systems.

The next contribution is by Dr. Graily Hewitt on "Apneumatosis," which he defines as a "condition of the lung-tissue characterised by the return of certain of the air-cells to a quasi-fœtal state," to distinguish it from "Atelectasis," which is the non-expansion of the air-cells, or the retention of the fœtal state. It is a clear, sensible monograph, and the only defect we can lay our finger on is that the author confines his pathology and treatment of the lesion solely to the case of children, thus passing over in silence that discipline of the thorax in boyhood which is so important to the health of the patient in adult age.

"Bronchitis," by Dr. Frederick Roberts, seems written rather against the grain. The author has not gone into his subject *con amore*, and consequently has neither contributed anything which bears the stamp of personal observation, nor collected the harvest grown by others. This produces a painful vagueness, of which the following sentence may be taken as a haphazard specimen :—

"If plethora exists, this must be reduced by appropriate diet and general management, and the use of watery purgatives."

The reader for practical instruction cannot but ask, What is plethora? What are its signs? Does it ever "exist"? What diet is appropriate? and why is the management to be "general" instead of also "appropriate"? Are "*watery* purgatives" those which are soluble in water or are they hydragogues? Then, to wind up, we are told that "a course of mercury is said to have a very favourable influence over some cases of chronic bronchitis." Surely nothing but harm can follow from the administration of teaching like that to junior practitioners. They are not told who says it, what sort of favourable influence mercury exerts, nor in what sort of case, and the result can only be either promiscuous drugging or a general disrespect for the valuable parts of the author's writing.

In less than fifty pages Dr. Anstie has managed to convey a vast amount of valuable matter on the diseases of the pleura, viz. "Pleurodynia," "Pleurisy," "Hydrothorax," and "Pneumothorax," the titles of his papers. In the first the main point is the removal of stitch in the side from the category of rheumatic ailments and its reception among the neuralgies. This considerably affects both prognosis and therapeusis, rendering the expectation of future allied ailments more correct and the treatment more decided.

In "Pleurisy" the author fixes with uncompromising decision the status of the disease in modern pathology. He points to two facts eminently characteristic of recent investigation as modifying in a sovereign manner our thoughts and practice in relation to this disease: first, the increasing certainty that primary acute pleurisy is but rarely fatal; and secondly, the discovery that those chronic cases which are merely the prolongation of the acute primary variety, may be safely treated with an energy which helps greatly to abridge the course they naturally tend to run. With our present means and new maxims of treatment it is not too much to say that primary chronic pleurisy has lost its most important features and its peculiar terrors; and the only reason for regarding pleurisy of chronic type in any special way is the fear, that underneath the apparently mere local affection there may lurk the taint of a constitutional diathesis, such as tuberculosis for example; while the regular result of an acute inflammation of this serous membrane is an illness of one week to three, ending in trifling local lesions of no injury to the patient's subsequent health and strength. The cases which last longer are those where external circumstances have been especially adverse, the patient neglecting ordinary instinctive precautions, either from obstinacy, or insensibility, or from the slight nature of the early symptoms. This is of course more likely to happen in the slighter cases; the protraction of the severe sort is, we fear, justly to be laid to the charge of the doctor. On the other hand, secondary pleurisy, of which our predecessors would seem to have taken less account, exhibits a much greater variety of type, and is a much more serious subject for prognosis. The vital significance of this class depends on two factors—the virulence of the original disease and the power of resistance which the organism has so far presented to it. The main points which the history of pleurisies secondary to acute fevers present, are the protracted course, the tendency to become prevalent, and the frequent end either in death or in constitutional disease, especially tuberculosis. The form of pleurisy which is secondary on acute or subacute diseases of other viscera are of very various types. Pneumonia, for instance, in numberless, perhaps almost in all, cases, is attended with a certain amount of fibrous pleurisy: but fortunately this is usually limited to a circumscribed development of plastic lymph, which acts as its own cure; while that which is consequent on Bright's disease is always an untractable ailment; but still its history differs greatly according to circumstances. Where it is the immediate consequence of the acute albuminuria of scarlatina, the collected fluid is rapidly changed to pus, and the least disastrous result in a chronic empyema, with too often a fatal termination. A different type of pleurisy may be often seen occurring as a complication of the amyloid form, still oftener of the cirrhotic (contracting) form of renal lesion. There is not here the same proneness to the production of pus, but rather to the collection of a copious fibro-serous (chiefly serous) fluid.

This short abstract of what Dr. Anstie gives as the history of pleurisy shows clearly enough, to those used to modern medical logic, what his treatment of it is. He denounces in no measured terms bloodletting, mercury, blisters, and says that in poultices, the subcutaneous injection of morphia, and rest, we have quite sufficient to counteract all the risks and pains of ordinary pleurisy. When these means have been insufficient to prevent severe fits of orthopnœa, or the time for their agency, say three or four weeks, has gone by without a renewal of the power of absorption, Dr. Anstie would proceed at once to *paracentesis thoracis*. He says truly that a new era has upsprung in the treatment of pleurisy since the recent development of this operation. Till lately it was a clumsy proceeding, which, notwithstanding all skill, admitted air into the pleural cavity, gave rise to suppuration at great risk of life, often established a fistulous opening, and was therefore only spoken of as a last resource, and had its chances of success diminished by being performed too late. The best that the 'Enclyclopædia of Practical Medicine' can say of it in the year 1834 is, "Still, even the few cases in which either complete recovery or relief for a considerable time has followed it, prevent us from despairing."[1] It is startling to contrast this lugubrious expression with the statement by Bowditch that since 1850 he has performed the operation "250 times in 154 persons, without once seeing any evil, or even any

[1] 'Encyclopædia of Pract. Med.,' vol. iii, p. 399.

very distressing symptoms resulting from it; while on the other hand it has saved a large number of lives which must otherwise have been sacrificed." What paved the way to this eminent success was the invention at the date given of his excellent suction instrument by Dr. Morill Wyman. The most important advance that has been made is the employment of apparatus which allows of the operation being made either simply exploratory, or carried on at once to evacuation of the fluid. Dr. Anstie quotes the approbation of a large number of the best practical men of the day, not as a logical argument, but simply as adding tone to the trumpet blast with which he thinks it "pardonable and even necessary" to proclaim the downfall of those timid and vacillating rules of conduct which text-books have hitherto prescribed. For these he would substitute the following:

"If at the end of fourteen or twenty days for a child, or three weeks to a month for an adult, from the initial symptoms, the fluid does not show real signs of diminution, paracentesis should be performed; and this rule is absolute, both for primary and secondary pleurisies, except where the case is hopeless on other grounds."

By "Hydrothorax" Dr. Anstie means a passive non-inflammatory effusion of serum, due either to mechanical obstruction of circulation or to blood poisoning. By this etiology he designedly draws as clear a line as possible between it and pleurisy, for a reason which cannot but appear to everybody most practical. As he says, "The tendency of the best modern practice in regard to hydrothorax may be said to be nearly the reverse of that with regard to pleurisy." Reference is intended to the uselessness of paracentesis, except in very few instances, to such cases, and on the other hand to the beneficial effects of diuretics and hydragogue purgatives. The singular number would have been suitable to the latter word, for he says, "I only recommend one, viz. elaterium, which is incomparably superior to all others." That sentence is very characteristic; it seems to protest against that vagueness of prescription which we have already denounced in this article, bidding the student more powerfully than in words to have but few drugs, but to take good care that those few are efficient and well proved. The Thucydidean heedlessness of grammar which in the two sentences quoted almost provokes a smile, gives a quaintness to the diction which makes it all the more forcible.

While on the subject of diction we may be allowed to suggest that the use of the conventional term "effusion" is somewhat unsuitable to the correct pathology which Dr. Anstie has evidently adopted, and makes less clear the therapeutics thereupon grounded. "Collection" would be an improvement, for in fact the amount of serum continuously effused is not increased, but its reabsorption is arrested, and to the recovery of this reabsorption the treatment is directed.

The four pages on "Pneumothorax" say shortly what has often been said before at greater and melancholy length.

II.—The Dynamics of Nerve and Muscle.[1]

More than twenty years ago, as he tells us in his preface, Dr. Radcliffe was not a little puzzled by seeing what happened to a rabbit after death by strychnia. The animal at death was propped up against the side of a box, touching the ground only by the tips of its toes, with its legs rigidly stretched out, and with its neck and body arched backwards until the head almost touched the tail, and so it remained until the putrefactive unstiffening of the muscles caused it to fall down. The contraction, which had fixed the body in this position before death, had not been relaxed by death; the spasm before death and the rigidity after death seemed to be confounded one with the other. Dr. Radcliffe upon this began to ask himself whether the interpretation of the spasm might not be found on the side of death rather than on the side of life; whether the spasm might not be in very deed a transition step towards *rigor mortis*, and result from the abstraction of something from the muscle, and not from something imparted to it or awakened in it; whether, in fact, it might not be a physical rather than a vital phenomenon.

This was a bold speculation twenty years ago. Now that the doctrine of the conservation of energy and the correlation of forces has penetrated into physiology, and educated medical men are familiar with the deductive calculations of Meyer and the experimental investigations of Joule, by which heat is weighed in the balance, and are competent to that scientific exercise of the imagination by which molecular motion and motion in mass are realized as mutually convertible, the one into the other, it is not difficult to conceive that "vital force" may be correlated with the cosmic forces generally, and to admit that vital motion may be simply another mode of motion; but twenty years ago the case was different, and the question was calculated to startle those to whom it was addressed by Dr. Radcliffe after he had answered it for himself. Whether we agree or not with Dr. Radcliffe in his latest conclusions on this subject, as expressed in the book before us, this credit at any

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[1] *The Dynamics of Nerve and Muscle.* By Dr. C. B. Radcliffe.

rate is due to him of having formed a daring conception, and of having perseveringly laboured to demonstrate its truth; he has, moreover, in the course of his work largely contributed to the establishment of correct ideas as to the real nature of pain and spasm, and of more rational and successful methods of relieving these conditions.

Dr. Radcliffe's theory of muscular action was originally this. In some way or other the natural electricity present in muscle during rest produces this state of rest or relaxation by keeping the muscular molecules in a state of repulsion, and contraction results from the discharge of this electricity, allowing the attraction of the molecules to come into play. In other words, muscle is inherently elastic, and tends to contract; during rest the mutual attraction of its molecules is antagonised by the natural electricity, much in the same way as cohesive attraction in bodies generally is by heat, and its contraction is determined by the removal of this antagonistic force. Dr. Radcliffe's earlier idea as to the form of electricity present in muscle was derived from Du Bois-Raymond's demonstration of a galvanic current from end to side of the muscular fibre, and he supposed it to be current or galvanic electricity; later, he was led to consider the condition of muscle at rest to be one of charge, the electricity being tensional, this modification in his opinion being gradually brought about by consideration of the fact discovered by Matteucci, that muscular contraction is accompanied by an electrical discharge analogous to that of the torpedo. Investigating this new hypothesis experimentally, he came upon the fact that, not one, but two kinds of charge were present in muscle, positive and negative electricity, a state of things which appeared to him to be inconsistent with the mutual repulsion of the molecules in virtue of charge with similar electricity. This does not appear to us quite in the same light; we do not well see how a charge of one kind only could be expected to sustain any degree of tension without being equilibrated by an opposite charge; and one only of the charges being within the fibre, might, from the author's old point of view, have induced the required molecular repulsion. Our difficulty would have been that tensional electricity is a phenomenon of surface rather than of substance. The great facts, however, remained—that muscular fibres in a state of rest were in a state of high electric tension; that the act of contraction was coincident with torpedo-like discharge, and the condition of contraction associated with a lowered electrical tension. Dr. Radcliffe held to the idea that this proved relation between the relaxed or contracted condition of muscle and the high or low electrical tension in the fibres was something more than a mere accident;

that in one way or another they stood in the relation of cause and effect, and the new theory presented for our acceptance is the result. This is, that muscular fibre may be compared to a Leyden jar; the sheaths are non-conductors; a charge of one kind of electricity generated on the outside of the sheath by the reaction of the blood there circulating *induces* a charge of the opposite kind on the inside; the fibres are kept in a state of relaxation or elongation by compression of the sheaths, arising from the mutual attraction between the two opposite charges, one on the outside, the other on the inside, as in a Leyden jar; contraction results when this compression is withdrawn by the act of discharge. It is remarkable in how many points this theory is consistent with facts, and affords explanation of them. In the first place, the muscular sheath is a non-conductor in a high degree. Then all the tensional electric phenomena and all the current phenomena of muscular fibre are explained by it; and not only so, but they can be reproduced upon a model of muscular fibre, of which the core is wood, the sheath gutta percha, coated inside and out by tinfoil. The Leyden jar condition, moreover, explains the difficulty of detecting the charge in muscular fibre, and the retention of the charge by the fibre, though uninsulated. Finally, the elongation and contraction of the fibre can be in some degree imitated in a narrow band of india rubber covered on its two surfaces by a thin metallic coating; when this band is charged like a Leyden jar it can be seen to elongate, presumably under the pressure caused by the mutual attraction of its two surfaces, and to contract when discharge occurs and the pressure is removed. Not only in these respects has the theory the support of fact, but the phenomena of *electrotonus* are explained by it; the increased contraction referred to "exalted irritability" is apparently simply an elastic return of the fibres from an increased elongation, due to increased charge. If we hesitate to accept Dr. Radcliffe's theory, it is certainly not because he has failed to establish any one of these points. So far he has made out his case, and his experiments and conclusions must constitute a valuable contribution to our knowledge of the phenomena of muscular action, and must be taken into account in any attempt to explain it. We shall follow briefly his arguments as they are employed in the book before us.

The history of animal electricity need not detain us, interesting as it is, nor will it be necessary to enter in detail upon the electrical phenomena manifested by nerve and muscle while at rest, and when passing into a state of action. It is familiarly known that when the two poles of a galvanometer are applied, one to the side the other to the end of a band of living muscular or nerve-fibre, a current passes through the instrument. By the new quadrant electrometer, now for the first time applied by Dr. Radcliffe to the investigation of the tensional phenomena of muscle and nerve, it is similarly shown that while at rest living nerve and muscle furnish supplies of free electricity, the sides exhibiting positive charge, the ends negative charge ; the tension, like the current, increasing with the distance from the line of junction of the two surfaces.

When a muscle or nerve passes from a state of rest to a state of action we have, on the one hand, almost complete disappearance of the muscle or nerve current, as shown long ago by Du Bois-Raymond, and an almost complete disappearance of all tensional signs of electricity, as ascertained by Dr. Radcliffe; and on the other hand, electrical discharge, as seen from the following long-known and familiar experiment:—If the nerve of a prepared frog's leg be placed across the muscles of another prepared frog's leg, or across the nerve, or if the connection between the two be only a piece of cotton wick moistened with salt and water, when the nerve of leg No. 2 is pinched, not only are the muscles of this leg thrown into action, but those also of leg No. 1. The force transmitted from the contracting muscles to the nerve lying across them, as shown by the contraction of the muscles to which it is distributed, can scarcely be anything but electricity, and we have thus simultaneously with muscular action disappearance of electricity from its fibres and discharge from its surface, and the same may be said of nerve. This is scarcely capable of explanation on any other supposition than that the condition of muscle and nerve during rest is one of charge.

If, now, we suppose the sheath of the muscular fibre to be a non-conductor, and the contained sarcous material to have decidedly better conducting properties, which is a not improbable inference from the relative conductivity of muscle and tendon, a charge of positive electricity generated on the outside of the fibre by the reactions between the blood and tissue will induce a charge of negative electricity on the inside of the sheath, as in a Leyden jar, and this condition will explain all the electrical phenomena just enumerated. The negative electricity of the contents of the muscular sheath will be conducted by the sarcous matter to the extremity of the fibres, and there manifested on the application of the electrometer; along the line of junction of the ends and sides the two opposite charges will neutralise each other, and the tension will be nil, the further away from this line the higher the tension, positive or negative. The current obtained when the galvanometer is employed instead of the electrometer must be taken as due to the continued generation of free electricity, which will go on while the muscle is living. We have here to allow a certain number of things to be taken for granted; as, for example, that the shortness of the circuit consequent upon the minuteness of the muscular fibre, may make it difficult to obtain evidence of tension and of discharge without rendering tension impossible, but this we do advisedly, and the more readily because several points are capable of illustration, and it may be said of demonstration, by means of the model of a muscular fibre constructed in accordance with this view of its different parts, as already mentioned. Unless some fatal difficulty presents itself, it is a good thing to take an hypothesis and try it. If a certain place is paved with good intentions, we may say that science is built up of overthrown hypotheses, each one of which has, in its day, been useful and valuable, and in its fall has served as a stepping-stone to something higher.

Now, admitting simply the fact of discharge, there are many reasons for considering the discharge from muscle as it enters upon contraction to be closely analogous to the discharge from the electric organs of the torpedo. The nerves to these organs are from the motor tract. When the nerves are divided the organs are paralysed; when the distal end of the divided nerve is pinched they are excited, but they are thrown into violent action by strychnia. The elaborate structure of the electric prisms and their free supply of blood show that the electricity is generated on the spot, and not communicated from the nerve-centres; they are exhausted by prolonged exercise, and obviously they are during repose in a state of charge, and the action of the nerves is to effect discharge. If we go farther, and admit the comparison between muscular fibre and the Leyden jar, we bring the electrical shock of the torpedo out of the category of exceptional phenomena, and we can even see, not only an analogy between this and the muscular discharge, but an analogy in point of structure between the electrical organ and muscle.

Up to this point, then, theory and facts go fairly well together; but there are other phenomena by which further to test the theory. When the two hind legs of a frog, separated from the body, but remaining united by all the structures of the part, or only by the exposed and isolated lumbar nerves, are made to form part of a galvanic circuit by placing the positive pole on one foot, the negative on the other, the current will necessarily pass up one leg and down the other—against the course of motor impulses through the nerve in the former, with it in the latter; it will be "inverse" in the one, "direct" in the other. In this experiment it is found (1) that contraction of the muscles occurs at the moment of closing and opening the circuit, or at one only of these moments; (2) that the muscles remain relaxed while the current is passing through them; (3) that the contractions last longer in the limb in which the current is upward or "inverse" than in the limb in which it is downward or "direct." Certain differences are observed when the nerves are exposed, and when the two legs are united by all the structures, namely, that in the course of the experiment, when contractions no longer occur, both at the closing and at the opening of the circuit, but at one only of these events, the two legs will contract together at the closing of the circuit when the nerves are *not* exposed, whereas when they are isolated one leg will contract at the closing of the circuit only, namely, that in which the current is direct or downward, the other in which the current is inverse or upward at the opening only. The explanation of this difference is that nerve, being a worse conductor than muscle, is traversed by the current only when it is isolated; when, therefore, the two legs remain united the current will traverse the muscles in preference to the nerves, and muscular fibres contract most readily and energetically when the impulse is strongest, irrespective of the direction of the current, and this will be when the circuit is closed. When the limbs are united only by the isolated nerves, the electricity has no other channel, and nerve is called into action most readily when the current coincides with the habitual course of impulses in it, which will happen differently in the two legs. This is beautifully demonstrated by making a metallic bridge from the muscles of one leg to those of the other when the nerves are isolated, providing a passage for the current, which allows it to avoid the badly conducting nerves. The contractions now occur just as in the legs connected by the structures. The way being now clear, we may follow Dr. Radcliffe in his explanation of the principal facts under consideration. First, then, it is shown to be probable that, as contractions occur only at the making and breaking of the circuit, and not while the current is passing, the contractions are due not to the primary current, but to the instantaneous *extra currents* of high tension discovered by Faraday to be developed when the circuit is closed or opened. Then it is demonstrated that the longer duration of contractions under the "inverse" current has no direct relation with the direction of the current, but with a charge of positive electricity associated with this current, and, *vice versâ*, the shorter duration of contractions under the "direct" current is due to an associated "negative" charge. This point is too interesting and important to be passed over without reference to the experiments by which it is established. The electrometer, then, is applied to different points along the two legs through which a current is passing from foot to foot, the legs being insulated; it is found that a positive charge is present in the limb to which the positive pole is applied, by which, therefore, the current enters, and in which, consequently, it is inverse or upward, and a negative charge in the other limb, the tension in each case being highest at the foot, *i. e.* near the pole, falling gradually to zero midway between the poles. Under these circumstances contractions can be elicited for 60′ in the muscles through which the current passes upwards, and in which the charge is positive, but for 15′ or 20′ only in the other limb, through which the current passes downwards, and in which the charge is negative. So far the difference might be due either to the direction of the current or the nature of the charge; but the direction of the current remaining the same, the kind of charge can be varied; if the positive pole be put in communication with the earth by a wire the positive electricity will run off to the ground, and both legs will be charged with negative electricity from the negative pole. In this case the contractions cease in both within 15′ or 20′. Conversely, if the negative electricity be run off, and a positive charge be allowed to spread itself over both limbs, the contractions in both will last an hour or more; that is to say, the direction of the current remaining the same, the duration of contractility, or, as Dr. Radcliffe prefers to call it, of impressibility alters according as the charge associated with it is positive or negative, and a reversal of the course of the current makes no difference in the duration of muscular impressibility, provided the kind of charge be made to remain the same. Can anything be more conclusive? And this positive charge, which is thus conservative of the muscular impressibility, is also to a certain extent restorative of it, and is the cause of the fact that contractions which have ceased under the direct current are removed under the inverse. We have only to go one step farther, and see how all this fits in with the theory that muscular fibres are like so many Leyden jars. The positive charge associated with the inverse current is only an exaggeration of the natural positive charge on the outside of the fibres, and so favours the continued activity of the muscle. The negative charge reverses the normal condition by substituting an external negative charge and an induced internal positive charge, and so is unfavorable to continuance of activity. Finally, it is easily understood how the extra currents of high tension accompanying the making and breaking of the voltaic circuit may discharge the natural electricity of the muscular fibres, and so cause the contraction, just as a Leyden jar is discharged by passage of a discharge across it.

We come now to the condition induced in a nerve by the passage of a voltaic current through a part of its length, called electrotonus. The facts to be considered are, that during the passage of the voltaic current the nerve current, as shown by a galvanometer, one pole of which is applied to the cut end, the other to the side of another part of the nerve in the usual way, is *apparently* increased when its direction coincides with the direction of the voltaic current (an-electrotonus), while the impressibility of the nerve is suspended; diminished when its direction is opposed to that of the voltaic current (cath-electrotonus), the nerve being more impressible. The word apparently is here necessary, for a source of error underlies the statements hitherto made both as to the nerve-current and as to the nerve impressibility in electrotonus. With respect to nerve-current, as the phenomena of electrotonus are reproducible with dead nerve and with a piece of wet string, it is clear that the current of living nerve is out of the question, and Dr. Radcliffe has shown that the currents manifested by the galvanometer at the two ends of a nerve, living or dead, or of a piece of moistened string, through an intermediate part of which a voltaic current is passing, are due to, and proportionate with, the resistance to this current, in consequence of which the electricity spreads from each pole outwards in the nerve or string, charging it with free electricity, the presence of which is rendered evident by the electrometer. That end of the nerve, then, which is to the outer side of the positive pole receives a charge of positive electricity, that which is beyond the negative pole receives a charge of negative electricity, and in this way arises the apparent reinforcement and diminution of the proper nerve-current. As to the impressibility of the nerve, Dr. Radcliffe has shown by conclusive experiments with two frog's legs joined together by the isolated nerves, in which an-electrotonus is set up in one leg and cath-electrotonus in the other by the same voltaic current, while the same induced currents simultaneously test the nerve impressibility in each, that, instead of the impressibility being suspended in an-electrotonus and exalted in cath-electrotonus, it is suspended in both, but in a greater degree by an-electrotonus than by cath-electrotonus, while it lasts much longer in an-electrotonus.

But there are conditions of muscle associated with electrotonus which have to be considered. In cath-electrotonus muscular contractions are greatly increased in force, and this has been referred to increased irritability. If, however, muscular relaxation is the result of pressure on the sheath by the mutual attractionof two opposite charges of electricity disposed on its inner and outer surfaces, and contraction the result of discharge allowing the natural elasticity to come into play, then an increased charge, such as has just been seen to accompany the condition of electrotonus, ought to give rise to increased elongation, and this increased elongation to a more energetic recoil. Now, Dr. Radcliffe demonstrates that electrotonus, an- and cath-electrotonus alike, *i. e.* whether the charge is positive or negative, does cause increased elongation of the muscular fibres as an antecedent condition to the more powerful contraction. It is to be remembered that Dr. Radcliffe also shows, by the elastic band coated with tinfoil, that the electric charge is capable of causing elongation, and discharge contraction; a fact explicable only on the supposition that the attraction between the two opposite kinds of electricity compresses the band transversely, and in so far antagonises its longitudinal elastic force.

The phenomena of electrotonus in muscle having thus been shown to be consistent with the theory of muscular action, and the phenomena of electrotonus in nerve agreeing with those of the "direct" and "inverse" current,—already seen to be in accordance with this theory, we have a remarkable mass of cumulative evidence in its favour. The author, therefore, is justified, after examining the behaviour of sensory and motor nerve under the action of different forms of electricity with similar results, in saying that the action of electricity in general, the voltaic, the franklinic, the faradaic, and that which is natural to nerve and muscle, would seem to be resolvable into that of a charge and discharge of free electricity; each form of charge, the negative as well as the positive, though not to the same extent, keeping up the state of rest and impressibility, the discharge bringing about the state of action.

So far our author challenges our unqualified admiration. We have not, of course, had the opportunity of verifying all his facts, but we have no hesitation in accepting them. They hang well together; they are so marshalled as to constitute one connected and cogent argument, and the theory on which they are strung, and which seems to bring them into a simple and harmonious relation, almost commands our assent. Our hesitation in accepting it arises from the circumstance that it ought to extend over a wider area of phenomena than it has yet been shown to cover. There are, moreover, difficulties suggested by the structure of some contractile elements. The non-conducting sheath is apparently absolutely essential to the Leyden-jar action of muscular fibre, and in the unstriped variety no sheath is known to exist. Then it is difficult to understand how the delicate sarcolemma of the striped fibres, which, *ex hypothesi*, is that constituent which elongates under the compression induced by the charge, and produces contraction by its elastic recoil, can really exert the power manifested by muscle. In some muscles it is so thin that its very existence is doubtful, as, for example, in the heart.

We go further, indeed; we persist on various grounds in our belief that the substance of the muscular fibre, and not the sarcolemma, is the contractile element. The behaviour of living fibre under the microscope, when the striæ can be seen to approximate at one part and not at another, on one side of the fibre, indeed, and not on the other—when the striped sarcous material can sometimes be seen to contract within the sheath and break, leaving the sheath empty and collapsed—is alone sufficient to demonstrate this. It is also obvious from the fact, that, in the healthy well nourished muscle of a vigorous young man or animal, the microscopic evidence of perfection of structural condition is found not in the sarcolemma but in the striated contents; moreover, it is in these again that the evidence of degeneration is seen.

We cannot help thinking that a better imitation of the conditions present in muscular fibre, for the purpose of illustrating on a large scale the elongation under the pressure by two mutually attracting charges, might have been found than that offered by the flat elastic band coated on the two surfaces with tinfoil. We might have had, perhaps, an elastic tube coated inside and out with metal, and enclosing an elastic core, and possibly the exercise of a little ingenuity and perseverance in the search for appropriate material for the sheath and core might have been rewarded by a much increased elongation and contraction. While accepting the experiment advanced as demonstrating change in length on charge and discharge, we remember that this was extremely small, requiring, even with a considerable length of band, a multiplying apparatus and long index to make it evident, whereas a muscular fibre will contract one third of its length. We do not, however, insist on this as a serious difficulty, for the proportion between the diameter and length of muscular fibre, and between the former and the intensity of the charge cannot be imitated, and may have an important bearing on the result.

Other objections of the same kind might be urged, but we will pass to another point. Marey has shown that the response of muscle to an electrical current is a single *secousse* or jerk, and that a sustained contraction resembling its physiological action results from the fusion, so to speak, of a rapid succession of jerks, the number given is thirty per second. Each secousse would imply the generation and discharge of free electricity, that is, the fibre would be charged and discharged thirty times per second. This is not easily understood. Coming now to nerve, it has been ascertained by Helmholtz, Marey and others that an impression is transmitted by nerve at the rate of 100 feet per second. This is very slow for anything in which electricity is concerned; and if discharge, as of a Leyden jar, is the concomitant of nerve-action, how is it that the discharge is not simultaneous from end to end of the nerve-tube?

We do not care to push these objections too far. The facts brought forward are too interesting and too valuable to be allowed to pass into oblivion with a shattered theory. Were it open to still graver objections, we think a theory which explains so much must have a basis of truth, and, if superseded, that it will be by some larger generalisation into which it will probably fit, and towards which it may lead the way. Most physiologists now look upon the various forms of energy evolved or set free in living creatures as the liberation of force stored up in the food consumed, as heat is liberated by combustion of coal, and the question between Dr. Radcliffe and them is not now whether muscular contraction is a manifestation of 'vital' force or a phenomenon capable of explanation on the principles of physical science, but of the steps by which the chemical force stored up in the food is made to take the form of mechanical motion. In the case of coal we have the clumsy and wasteful process of the steam-engine; in the muscular apparatus of animals the end is attained with marvellous economy, perhaps not altogether without what seems waste, since exertion evokes heat, which has to be got rid of by perspiration; but still with an economy which puts to shame the efforts of mechanicians. How is this effected? On the one hand, what constituent of blood or of tissue undergoes oxidation? What are the successive stages in this oxidation, since we know it not to be direct? On the other hand, does the chemical energy take some intermediate form of force in order to become motion? This being probable, is this intermediate force heat, as in the case of the steam-engine? Or have we this question answered by Dr. Radcliffe in the researches just detailed? If so, then science owes him a large debt.

Now we part company with our author. In combating the vitalistic notion he considers it important to show that muscular action is antagonised by arterial blood, and he adduces in evidence the facts, that in death by rapid hæmorrhage or sudden strangling there is convulsion, and further seizes upon the isolated experiment that strychnia and brucia, which induce convulsions, when mixed with blood drawn from the vessel, retard the reactions between it and air. In all these cases convulsion is coincident with deficiency of arterial blood in the vessel, or the equivalent of this, inasmuch as the blood has lost, through the action of the poison, its power of carrying oxygen; and it is concluded therefore that muscular action is in one way or other antagonised by arterial blood, and possibly by keeping up the state of charge in the fibres. Now, the fact is, that in the cases here instanced the muscles are simply played upon by the nervous system, and of themselves have nothing to do with the convulsions. If the nerve of any limb be divided before the animal is bled to death or strangled, or poisoned by strychnia, the muscles of that limb, though deprived of blood or supplied by poisoned blood equally with the rest of the body, take no part in the convulsions. The occurrence of contraction in a muscle has no direct connection whatever with the amount of blood circulating through it. This may be much or it may be little; the muscle will not act till, by the nerve or some other agencies known to have this power, a contraction is determined. Moreover, if muscular action is antagonised by arterial blood, how is it that during muscular exertion the circulation and aeration of the blood are so greatly increased? Nothing can be more certain than that muscle in action requires and receives a more abundant supply of blood and oxygen, from which the energy developed by it is evolved. If Dr. Radcliffe's theory necessitates a denial of this, then assuredly it is unsound; but we cannot see that it does; at the same time it is weakened by being encumbered with an error of this sort.

An error of a similar kind runs through the reasoning which is supposed to establish that muscular action is associated not with the presence but with the absence of nervous influence. It is taken for granted, that the amount of nervous influence developed in the great nerve centres is proportionate to the amount of blood circulating through them; and apparently, that this influence is continuously communicated to the muscles;—both of which positions we should dispute; this, however, by the way. Again, the occurrence of convulsions in death by hæmorrhage or strangling, and likewise when the great vessels of the head are compressed, is taken as demonstrating that the withdrawal of nervous influence is the cause of the muscular contractions. But if this were the explanation, it would make no difference whether the withdrawal of nervous influence were abrupt or gradual, whereas when the hæmorrhage is gradual or the suffocation slow, no convulsion takes place; the suddenness, therefore, of the change in the circulation counts for something, and opens the door to another interpretation. Surely, also, the most efficient way of withdrawing nerve influence is to divide the nerve, and this paralyses instead of tetanising the muscle.

Let us take again the case of strychnine poisoning, lest it be supposed that we only seek to shift the question from the muscles to the nervous centres. The explanation would then be, that the oxygenation or oxidation (two very different things) being prevented by the action of the poison, the blood carried to the nerve centres is no longer competent to generate nerve influence, for lack of which the muscles fall into convulsion. Now, this takes no note of two important facts—first, that in poisoning by strychnine the cerebral hemispheres, which are the first to be affected by want of oxygen, do not at all suffer in their function; and, secondly, that other substances, and notably carbonic oxide, interfere with the oxygenation of the blood far more decidedly than strychnine, and destroy life without ever inducing convulsion.

Our mission here is destruction, not construction, and we cannot stop to discuss this vague term, nerve influence, or try to establish a conception of the force evoked by the nervous centres which might bring it into possible correlation with other forms of energy.

When the question is not the simple excitation of muscular contraction, but the effects on the duration of contractility, on the energy of contraction and on the susceptibility to electric shock of varying intensity in muscles separated from the nerve centres, the considerations advanced are far more weighty. There is Claude Bernard's experiment of the *lapin à sang froid*, in which, after division of the cervical cord, the paralysed muscles are found after death to retain their contractility almost as long as if the animal had been a reptile. Again, the leg of a frog is thrown into action by a feebler electric shock after than before division of the spinal cord, and by a still more feeble shock if the lumbar nerve have been divided; and, farther, the force of the contraction is greater after than before the division of the cord. Of course, the conclusion from Dr. Radcliffe's point of view lies on the surface—muscular action is inversely related to nervous influence, and we will not now dispute it, though different interpretations have been put on the experiment.

Shall we go on to the consideration of the arguments which Dr. Radcliffe draws from the action of the heart and vessels, from the respiratory movement, &c.? If we do, it is in order that the theory may be divested of difficulties not inherent in it, and may stand upon its own merits. If we must accept with it the physiology of circulation and respiration tacked to it in this book, we reject the whole; but the two things are quite distinct. The rhythmic action of the heart is here accounted for in this way. The diastole is caused by the injection of the walls of the ventricles with arterial blood driven by the recoil of the aorta into the coronary arteries, the blood simultaneously antagonising the contraction of the muscular fibres directly and indirectly by generating nerve influence in the ganglia, which has the same effect. The systole is due to the change of the blood from arterial to venous, when it no longer has the property of resisting contraction, and so on. How can this rationale be accepted when we know that the heart may go on contracting for some time after its removal from the body, whether exposed to air or immersed in water; that when it is cut into pieces rhythmic movements may be observed in the fragments from the base? The physiology of respiration is even less comprehensible. The inspiratory movements are said to be due to impressions made upon the periphery of the afferent nerves of respiration by the oxygen of the air, the expiratory collapse to the cessation of the impression in consequence of the consumption of the oxygen and generation of carbonic acid. Now, the afferent impression in ordinary respiration is known to start from the air passages, and we are consequently, at the end of respiration, in this position. In order to excite inspiration oxygen is wanted for the purpose of making an impression on the afferent nerves of the air passages, the oxygen of the contained air has been consumed, and no fresh air can enter except by inspiration; that is to say, an inspiration must take place in order to produce the impression necessary to excite the act of inspiration, which is absurd, as our 'Euclid' used to say.

Here we stop. The author goes on to summarise his conclusions, and to support them by the comparison of the behaviour of a magnetised bar of iron, which suddenly elongates without change of volume when charged with magnetism, and contracts when the current by which this is effected is stopped. He deals also with various objections of more or less weight, but nothing which appears to us important is added. We do not propose to follow him at all into the domain of Pathology. His views as to the character and causation of epilepsy, tremor, tetanus, and other deviations from normal muscular action, have long been before the profession, and have been frequently discussed. It is not too much to say, that they have had an important influence in the production of the change of opinion respecting these states which has taken place. They have not undergone any such radical modification as the theory out of which they originally sprung, and consequently do not require any new examination. We are the less inclined to this task also, since it would send us far back among fundamental questions to which we could here not do justice when they were raised, and would involve long discussions, out of which would come disproportionately small results.

The task we proposed to ourselves was to endeavour to form a correct appreciation of Dr. Radcliffe's new theory of muscular action. We find that, in his efforts to establish it, Dr. Radcliffe, —unless his experiments, and not his arguments merely, can be impugned, has at any rate established the existence of a new relation between electricity and muscular action, the association of relaxation with charge, and of contraction with discharge; he has thereby reduced to beautiful order and made comprehensible the confused and perplexing phenonema of electrotonus and of the direct and inverse current. This is a service to science of the highest value, and justifies and rewards his prolonged devotion to the question of muscular and nervous action, and if we cannot think that he has established his view of the part electricity takes in the process, and of the mode in which electricity operates, we believe he has laid the foundation of a true theory yet to be evolved.

III.—Dr. Porter Smith on Chinese Materia Medica.[1]

THIS volume, which must doubtless have taken up a large amount of the author's time and attention, is arranged alphabetically in the fashion of our old dispensatories, and is mainly based on the native books of the Chinese 'Materia Medica.'

Such an arrangement excludes the possibility of our reviewing it in the ordinary sense of the word; and the best method of giving our readers an idea of its value is by numerous abstracts and short quotations from the most important or curious headings.

Of *antelope horn* we are told that

"The horn of a kind of chamois, usually set down as the antelope gutturosa, is mainly brought from Lung-ngan fu in Sech'uen. A country called *O-tan-kwoh* is said to have a sort with a continuation of the dewlap passing along the under surface of the belly to the tail. The character representing this animal consisted of the two characters for 'deer' and 'spiritual' combined. It is said to hang itself up in trees by its horns. It is described as a sheep with coarse hair, which renders its skin valuable for making coverings for beds or seats. A kind of 'wild ass' or *shan-lu* is described in connection with it. A kind of unicorn belonging to this species of antelope is said to be met with on a mountain in Annam. The specimens sold in Hankow are about five inches long, of a dirty white translucent colour, with several partial rings marking the base of the horn, which is about the size of a man's thumb, tapering off gradually to a point, with a single spiral twist. The horn is given in coarse powder, or after being partially calcined as a remedy in convulsive apopleptic, cerebral, and rheumatic affections. It is said to hasten the pains of labour when given in wine. Most of these properties are mere

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[1] *Contributions towards the Materia Medica and Natural History of China. For the Use of Medical Missionaries and Native Medical Students.* By FREDERICK PORTER SMITH, M.B. Lond., Medical Missionary in Central China. 1871, pp. 237. Shanghai and London.

conceits, but women are very fond of taking this medicine in sundry diseases of the pregnant and puerperal states" (p. 18).

Of *sublimed arsenic*, we read that this preparation is used to cure ague, according to the *Pen Ts'au*,[1] but very few of the practitioners of the present day venture to prescribe it. Caustic, emetic, anthelmintic, and alterative properties are set down as the effects of this powerful drug.

"The water of the *Phaseolus angulatus* is said to be antidotal of this poison. Bottles exposed to arsenical fumes are said to preserve wine kept in them for a long time. Chinese crackers are said to be much louder when containing a portion of this sublimate. Asthma is said to be relieved by small doses of this drug, but it is distinctly forbidden in all sorts of eruptions and sores. It is recommended in chronic dysentery along with massicot, and in sundry pains and aches of a neuralgic nature."

In the next paragraph on *white arsenic* we are glad to find that none of these arsenical preparations are sold in shops without evidence and witness to the propriety of the sale. The punishment of death by decapitation is inflicted upon both the seller and the buyer if fatal effects result. If not fatal, they are both strangled. (The alternative is a pleasant variety!) If the druggist ignorantly or carelessly sells the poison, he receives eighty blows.

The following history of *Artemisia moxa* is curious. This herb is hung up with the *Acorus calamus* over the doors of every Chinese house on the fifth day of the fifth month. Although its principal use is as a counter-irritant, this plant is in general repute as a charm, or remedy in internal diseases. The downy leaves are collected, dried and rolled into a small ball, which is ignited upon the skin, in order to cauterize the part.

"The heat of the sun's rays collected by a mirror or glass is said to be the proper way of igniting the moxa. This form of cautery, called *Ngái-ho*, was formerly applied indiscriminately in all cases of disease. In Hupeh, at least, the moxa has fallen into disuse, as it is employed by the buddhist priests in initiating their neophytes. The *Teng-ho*, or lamp cautery, and the bloody cupping-vessel, called *Pa-ho-kwan*, have replaced the moxa to a very great extent. The plant itself is used as a carminative, stimulant, stomachic, astringent, alterative, and resolvent remedy. The supply comes from Chin-ting fu in Pehchihli, and a reddish variety called *Tsz'ngái*, comes from Fung-yang fu, in Nganhwui. The best, called *Ki-ngái*, comes from

[1] The *Pen-ts'au-kang-muh* is a "synopsis of ancient herbals," compiled by *Li Shi-chin*, a district magistrate, born in the Province of Hupeh. Its preparation occupied him for some forty years, and it contains no less than 11,896 formulæ. The substances of which it treats are arranged in sixty-two great classes, under the sixteen orders of water, fire, earths, minerals and metals, herbs, grain and pulse, vegetables, fruits, trees, garments, and utensils, insects, scaly animals, mailed and shelly creatures, birds, beasts, and man. It was published about 1597. This encyclopædic work is more often referred to by our author than any other authority.

K-'i-chau, in Hwang-chau fu (Hupeh). A solid substance used as a febrifuge is mentioned by Dr. Williams in his 'Chinese Commercial Guide,' as a kind of camphor, extracted from the leaves of artemisia. The crystals are limpid and brittle, and present a brilliant fracture. It is probably identical with a solid volatile stearoptene called *Ngái-lah-hiang*, formerly brought as a tribute from some foreign state. It is said to be disinfectant, cooling, astringent, and anthelmintic" (p. 25).

Under the deceptive title of *asses' glue*, or *O-Kiau*, we meet with a drug in flat rectangular cakes, two inches by one and one third of an inch in size, and three or four lines in thickness.

"It is reddish and translucent, with all the properties of common glue. The cakes are wrapped in rouge-red paper, as is usual with all expensive drugs. The name asses' glue is an incorrect and ridiculous translation of the Chinese name; for the genuine drug is properly the extract prepared by boiling down the waters of a celebrated well, at a place sixty *lí* to the north-east of the district city of Yang-kuh, in Kwan-chau fu (Shantung). This town was anciently called *O-yih* or *A-yih*, and the well was named after it. The well, as large over as a wheel, and sixty or seventy (Chinese) feet deep, contains a water probably like that of Barèges, in France, which has a gelatinous principle in it, conferring peculiar properties on the water. As hartshorn, cowhide, and the skin of a black ass are said to be often used to make this valuable article, which is used by artists and by others than carpenters to join articles together, the power of this water to produce such a substance will be perhaps doubted. . . . Tonic, astringent, tussic, emmenagogue, arthritic, sedative, and diuretic properties, among others, are attributed to this "perfect medicine," as Li Shi-chin calls it" (p. 28).

The *bat* is a very common animal in China, "being a frequent visitor of foreign houses in quest of mosquitoes, which it devours most satisfactorily. As it is supposed to feed upon the stalactites which are frequently met with in the caves which it is wont to hybernate in, its medicinal properties are rated at considerable value by the Chinese. From its asserted longevity and its excellent sight, this curious creature is credited by the Chinese with the power of conveying these desirable qualities to those who consume the disgusting preparations made from all parts of its body."

But unpleasant as a bat must be, *bat's dung*, which is sold as a dark brown, coarse powder, looking something like teadust, and consisting of *débris* of the Mylabris insect, dirt, bat's dung, and other extraneous substances, must be still more decidedly objectionable.

"As bats fly by night, the Chinese name this composition, which according to the *Pen Ts'au* was formerly much better made, 'night-bright sand,' and apply it to the eyes as a powder or as a wash, in tinea tarsi, opacities of the cornea, &c. They profess to detect the eyes of the mosquitoes, on which the creature feeds, in this excrement, which is given internally in ophthalmic affections, otorrhœa, ague, cough, infantile dyspepsia, tabes, offensive perspirations, &c." (p. 34).

Under the heading "Bones of Tiger," we find a notice of several curious superstitions.

"This animal is said by Chinese writers to be the king of beasts and to have very intelligent ways. It is said to eat its victims according to the calendar, and to have the power of planning out the country round its lair, to be visited according to a fixed system. If it leaps up three times at its prey, and fails, it withdraws. Its victims become devils after digestion, but the flesh of the dog is said to intoxicate this cat-like creature. Bad smells, such as burnt horn, are said to scare it away, and the hedge-hog, or tenrec, is said to be able to get the better of it. It is believed to become grey at the close of the first 500 years of its life. An animal, spoken of as the *Ts'iu-rh*, is said to be much larger than the ordinary tiger, having a white body with black stripes, and a tail as long as the body. The tibiæ and skull bones are esteemed the best for making the tincture of the much vaunted drug, which is much used in Hankow in rheumatic affections of the joints, diseases of the bones, ague, and general debility" (p. 41).

Carrot seeds are recommended in chronic diarrhœa; we may add, carrots are eaten by the natives with wild ducks.

Turning to *cassia bark*, we find it stated that

"The compound powder of cinnamon is an excellent remedy in the water brash of Chinese dyspeptic patients, and with a small quantity of opium it is one of the best things that can be given to an opium-smoker endeavouring to give up the habit. Some action is ascribed in Chinese works to cinnamon as affecting the uterus, a property which is usefully turned to account in the treatment of menorrhagia, a very common disease in China."

The writer seems not to be aware that cinnamon has been successfully employed in this country for the last named affection.

On reading the article on the "Castor Oil Plant," we find that it

"Is grown in Hupeh as a shelter from the sun. This fact lends considerable probability to the belief that the gourd of Jonah was this plant, which attains a considerable height, and is self-sown in tropical climates. It has been, or is known, in all parts of the world. The Saxons were acquainted with it, and in their translation of the 'Herbarium of Apuleius,' a favorite book with them, it is said that this 'wort smootheth every tempest.' It grows to the height of more than ten feet, and forms a woody stem, which never survives the winter of Central China."

The crushed seeds are used in Chinese medicine as an outward application in a large number of diseases, combined with the oil of the seeds, or the pulp is taken internally as a remedy, whose effects must be nearly identical with those of the oil.

"The pulp is rubbed into the temples in headache, into the palms of the hands in palsy, and is introduced into the meatus of the urethra in stricture. The pulp is rubbed into the soles of parturient women to hasten the birth of the child, or the expulsion of the placenta. It is stuffed into deaf ears, rubbed over the top of the head in cases of prolapsus uteri, and is applied with the oil to burns and scalds" (p. 55).

Chloroform is introduced into the author's list of native remedies, although of course it was unknown until introduced at the mission hospitals. He observes—

"That it has a most excellent effect on the Chinese, but should always be given with much caution to confirmed opium smokers, and generally with more care during the very hot weather. The repetition of the drug, towards the close of the operation, when consciousness has been already restored, is a dangerous experiment in the author's experience."

The term *Ma-yoh*, which the natives have given to chloroform, appears to be more generally applied to local anæsthetics than to those which act through and all over the system. The name seems to have been taken from the word applied to the painless eruptions of leprosy, which is called *Ta-ma-fung*.

"The flowers of a species of cannabis, *Ho-ma*, and of the Datura, *Man-tóu-lo*, were formerly infused in wine, and drunk as a stupefying medicine preparatory to acupuncture, the opening of abscesses, and the use of the actual cautery. A solanaceous plant, called *Yah-puh-lu*, probably identical with the *Atropa mandragora* of botanists, is said to be capable of causing a trance of three days' duration. Hwa-To, a celebrated surgeon of the Han period, the Machaon of Chinese historical romance, used this latter plant. Aconite root, the tubers of *Pinellia tuberifera*, long pepper, the root of *Heterotropa asaroides*, the flowers of hyoscyamus, azalea, andromeda, and rhododendron, the tubers of *Arisæma* and *Arum pentaphyllum*, an unknown gum resin, called *Mwan-hiang*, and the fat extracted from the head of the toad, are substances which are reputed to have anæsthetic properties, generally applied locally. These substances, and other imaginary or superstitious formulæ, are said to be employed by kidnappers of children, who manufacture *Yoh-p-ing* or 'medical confectionary,' containing these drugs. On this account such drugs, called *Mi-yoh*, are virtually forbidden to be sold or employed. Robbers are known to use a sort of pastille containing the *Mwan-hiang* and other quieting perfumes, by means of which they certainly seem to render the sleep of their victims very profound" (p. 61).

The *Dragon* contributes its *blood, bones, spittle,* and *teeth* to the Chinese 'Materia Medica.' Passing over the first, which is a common, well-known gum, we shall give our author's statements regarding the nature and the properties of the others.

Under the name of *dragons' bones* we read that

"Irregular pieces of fossil ivory, weighing a few ounces, are sold, as Hanbury remarks, in stamped packages. They have been examined microscopically by the latter observer, and proved to be, at least in some cases, fossil ivory. Fossil bones of the *Stegodon orientalis*, of Swinhoe, are brought from Ching-tu-fu and Ch'ung-king fu, in Sech'uen, in large broken masses, showing the cancellous structure of the large fossil bones of proboscidians. Portions of limestone matrix, bearing the impressions of these bones, are sold with these genuine fossils, which are also brought from I'-chau fu, in Shantung, Tsangchau, near Tientsin, and from Tai-yuen fu, in Shanse. They are powdered, levigated, and used in chorea, spermatorrhœa, ague, fevers, hæmorrhages, and fluxes."

The following is the description of *dragon's spittle* or *lung-sin*:

"A costly, odorous, light yellow, gummy substance, found floating on the sea, or procured from the belly of some large fish in the Indian Ocean, is described in such a particular way, under this name of *lung-sin*, as to leave no doubt that ambergris is meant. This is probably the origin of Dr. Williams' *Lung-sin hiang*, a name applied to a counterfeit ambergris, made by mixing together Borneo camphor and musk. The dragon is said to cough up this ambergris. A similar substance, called *Kih-tiau-chi*, brought from Canton and Foochow in former days, is said to be the egg of the dragon or a kind of sea-serpent, named *Kih-tiau*. This drug is of a greyish or yellowish colour, according to Chinese writers, and is asserted to have marvellous discutient, vulnerary, and healing properties."

Dragon's teeth or *Lung-ch'i* are

"Fossil teeth of the *Stegodon sinensis* (Owen) that have been found in the marly beds of the country round Shanghai by Lockhart, and of the *Stegodon orientalis* (Owen), by Swinhoe, near Ch'ung-king fu, in Sech'uen. These, with the horns of the *Chalicotherium sinense* (Swinhoe), the teeth of *Hyla sinensis*, from Ch'ung-king fu and from Ching-tu fu, in Sech'uen, and molars of mastodons, elephants, sheep, stags, teeth of the hippotherium, described by Hanbury, after Waterhouse, as coming from Shensi and Shansi, are sold under the name of Lung-ch'i. They are supposed to act on the liver, and to be of great service as cordial or sedative remedies."

The *dung of the common sparrow* is

"Mixed with pepper corns, powdered, and then mixed up by means of spirits of wine. This mess is used to diminish the pains of opening abscesses, the thick compound being first applied for some time to the skin. It is also applied to the wounds caused by arrow-heads or shot, to diminish the pain of extracting the foreign bodies" (p. 90).

The excrements of other birds, as the magpie and the white pigeon, are also used by the Chinese. Of the dung of the former we read that it is cordial, sedative, anti-periodic, astringent, anthelmintic, and vulnerary, with almost any other quality that could be enumerated. It is one of the remedies for leprosy, being applied to the benumbed parts in the form of an ointment. The brain of the magpie is eaten to increase the thinking power.

Under the heading "Fowl, Domestic," we find some curious statements; as, for example, that all the Chinese names for this bird refer to its crowing, which they say is regular all through the day, as well as at dawn. The Chinese having no means of reckoning time, pay special and superstitious regard to the crowing of cocks. The flesh of the male bird is said to be injurious, especially to those suffering from bad eyes, or from growths or sores of any kind. This objection is more likely to depend on the fact that the cock is used in oaths and sacrifices, and is not to be slain on ordinary occasions.

"Black-boned fowls are called *Yoh-ki*, being much prized for making soup for those suffering from lung diseases and debility after hæmorrhages. Many other distinctions are made between the colour and sex of birds as to their suitability, or otherwise, for particular classes of sick folk. Cordial, tonic, and many other fanciful properties are attributed by the Chinese to the albumen and yolk of the egg, which they compare to the sky and soil of the universe respectively. The white of the egg is applied like collodion to burnt sores, and eruptions on the head. Egg shells, *Ki-koh*, are burnt and pulverised, to be given in dysuria, and for use topically in scald head" (p. 48).

The article on "Ginseng" is, as might be expected, curious, but contains less information than we should have expected to find. The Manchurian ginseng is the best, the Manchus boasting that the weeds of their country are the choice drugs of the Chinese.

"The pieces, after careful trimming with a bamboo-knife, and drying in still air, are made to assume something of the form of the human body. They generally do resemble a miniature human hand, the larger pieces being the size of a man's little finger, with some two to four finger-like branching rootlets. They are yellowish, semi-transparent, firm, brittle to some extent, and of a sweet, mucilaginous taste, accompanied with a slight bitterness. The drug is usually prepared for use by steaming and finishing off, so as to approximate its appearance to the normal standard of clearness. Fabulous stories are told of the finding of special depôts of this root, which is associated with guiding voices and other good and peaceful omens. This drug is prepared as an extract, or as a decoction, in silver vessels as a rule. Its effects are apparently those of an emetic, tonic, stimulant, carminative, and demulcent nature. It is prescribed in almost every description of disease of a severe character, with few exceptions, but with many reservations as to the stage of the disease in which it may be administered with the greatest benefit and safety. All forms of debility, spermatorrhœa, the asthenic hæmorrhages, the various forms of severe dyspepsia, the persistent vomiting of pregnant women, malarious affections of a chronic character, the typhoid stage of fevers, especially of an epidemic character, are occasions in which the Chinese use this drug. Several cases in which life would seem to have been at least prolonged by the taking of doses of this drug, so as to allow of intelligent disposition of property, indicate that some positive efficacy of a sustaining character does really exist in this species of ivywort. The leaves are sold in bundles of the green, fragrant, excellently preserved foliage of the shrub. They are said to be emetic and expectorant in their effects" (p. 104).

Under the heading "Indigo" we read that indigo-dye is almost the only form of this material employed medicinally.

"Swellings, bruises, stings, strumous glands, and tumours in general, are treated with a daub of this remedy. The pages of the 'Pen Ts'au inform us that fevers, fluxes, worms, infantile disorders of all kinds, were treated by means of some form or other of this, perhaps, rarer remedy. Li Shi-chin properly remarks that the lime used in its manufacture must make its action not a little different from that of so much indigo juice. It is curious that the Chinese have anticipated us in the treatment of convulsive diseases by this agent, the action of which in such cases deserves some further trial. The domestic use of the blue-bag in England as a remedy for the stings of bees and wasps is daily carried out in China. Indigo extract was used for painting the eyebrows in olden times, as henna was employed by the Arabians" (p. 117).

Lapis hepaticus, or *Fuh-ling-kan*, is supposed by the author to be the calcined clay which forms the simple fireplace of the poor Chinese. It is mixed with pig's liver, and administered as a remedy in cross-births, puerperal, cutaneous, and many other disorders, and is probably of about the same value as the majority of the Chinese drugs.

The so-called *Lotus* is in reality the beautiful water-lily known as *Nelumbium speciosum*, and has scarcely a part that has not a special name and use amongst the Chinese.

"The flower, with its red-tipped, pinkish-white petals, is seldom gathered, plants being placed in large jars for ornamental purposes. The petals of a flower, with the horary characters of the person traced upon one of them, were formerly swallowed by women in the throes of difficult labour, as a certain relief. The carefully dried, beautifully yellow fragrant stamens of the flower are brought from Honan to Hankow; they are used as an astringent remedy, and as a cosmetic article; the seeds are brought, specially as a medicine and as an article of dessert, from Fuh-kien, Kwang-sin-fu, in Kiangsi, and from Kwang-p-'ing fu, in Pehchihli. The kernels are very starchy, and pleasant when boiled in soup or roasted; they are also eaten raw; they are supposed to be good for spermatorrhœa, and hæmatemesis. As sold in the streets, these nuts might be mistaken for acorns. The leaves are prescribed as remedies in fevers, fluxes, and hæmorrhages. Even to the leaf-stalks are assigned some therapeutical value in certain movements of the fœtus in the gravid uterus, which are frequently referred to in the 'Pen Ts'au as something very serious. The stalks of the curious receptacle in which the carpels are embedded, resembling the broad nozzle of a watering-pot, are a popular remedy for hæmoptysis, a frequent symptom with the Chinese" (p. 139).

Under the heading "Mutton" we read that the sheep is comparatively rare in China, and hence we need not be surprised to learn that "mutton-broth is advised as good for pulmonary diseases, abdominal obstructions, debility, and for parturient and suckling women."

To the *Mylabris chicorii*, a coleopterous insect with a very wide distribution (known as the Telini fly in India), the most remarkable properties are ascribed:

"It is reputed to be emetic, diuretic, and antidotal. It is taken internally in scrofula, syphilis, and rectal diseases, and is given to persons for purposes of abortion, contrary to the Tartar code. It is the grand remedy of the Chinese faculty for hydrophobia, a disease by no means common in China, in spite of the street-plague of ill-conditioned dogs met with all over the empire. The mad dog is supposed to have impregnated the bitten person, and the little dog, the product of this conception, is sought for in the urine, rendered bloody by a large dose of the powdered mylabris, digested in wine. Recovery is thereupon considered certain" (p. 154).

Glancing at the article on "Paper," we find that the ashes are given as an astringent, and the paper of an old book or letter, after cutting out the printed characters, held in such commendable veneration in China, is a remedy for barren women!

Pork is said to produce phlegm, and is considered to be bad for those suffering from healing wounds, abscesses, and strumous, or inflamed joints. Every part of the pig is assumed to have some special medicinal property. The liver and lungs are commonly employed to make soup for convalescents, who almost invariably make this terrible piece of extravagance the climax of their recovery. The blood of the pig is carefully collected, cooked, and hawked about the streets of Hankow at night, as a favourite supper of the million. Pig's feet make a gelatinous broth, much used as a wash for irritable carbuncular and other sores.

Soy being a fluid popularly regarded as mainly composed of black-beetles, it may be satisfactory to know how it is really prepared :

"The yellow beans of the Dolichos-soja are boiled very soft, and mixed with any cereal flour in varying proportions, and allowed to ferment and become mouldy. Salt and tea or boiling water are then added, and the mixture is then exposed to the dew and sun of the open air for three weeks or a month, care being taken to avoid rain. The liquid becomes much thicker, darker, and more uniform in consistence, and after constant stirring it is then strained and kept for use. Bran is sometimes used in making it. Large quantities are both sold from the shops and made at home by the Chinese. It is considered to provoke the appetite, and to correct any injurious quality of food. It is laxative, cooling, and antidotal, according to Chinese estimation. It is sometimes daubed upon burns, scalds, eczematous and leprous sores" (p. 203.)

The quotations and abstracts that we have given afford sufficient evidence that Dr. Porter Smith has produced a very interesting and remarkable volume, which deserves a place in every medical library.

IV.—The Medical Jurisprudence of Insanity.[1]

The difficulties that beset the subject of Insanity in its relations to law are very numerous. Law itself is no longer unvarying; the absolute *dicta* of a Solon, compelling compliance, would now no longer be accepted. Law "grows," as our author puts it. But, unfortunately, legal form still carries with it much of the old pride of the times when it was absolute. The necessity of this form being based as far as possible upon the results of scientific research is indeed acknowledged; but there is a tendency among lawyers to revert to general principles which are insufficiently established, and to deduce from them conclusions for particular instances. In no branch of law has this tendency more decidedly displayed itself than in that which forms the subject of this work. Such a maxim as that the law cannot take cognizance of gradations of mental disease,—one lying at the root of many of the mistakes made by lawyers in dealing with the subject of Insanity,—is unhesitatingly affirmed. Where a proposition is so unreasonable that no reason can be given in support of it, we suppose it is best to affirm it unhesitatingly.

There is a second difficulty, not so great as the preceding one, but still formidable, viz. that medico-psychology is not in so forward a state as to furnish a foundation for the construction

[1] *The Medical Jurisprudence of Insanity.* By J. H. Balfour Browne, Esq., of the Middle Temple, Barrister-at-Law. London, 1871.

of a scientific or rational code of legislation for the insane. In fact, a complete science of Insanity is still a very long way off, and recent advances, though great in comparison to former progress, are very small when measured by the length of road still to be traversed. But this difficulty is the less dangerous of the two; for when we compare the present state of scientific knowledge with the excessive haziness in which the followers of the law seem still to be, we have no unfit companion to the contrast between the perfect science of the future and the imperfect of the present.

In the third place, while it would be difficult for any man to write a work on the 'Jurisprudence of Insanity,' with a greater amount of ease and fluency in handling legal details than Mr. Browne displays, it is unmistakably difficult for him to understand the medical aspects of cases. If it be true, as a modern artist has said, that one must have sympathy with a stone in order to draw it, much more is it true that the physician must have sympathy with an invalid's case in order to understand and treat it. Every detail must be patiently acquired and considered; the growth and constitution of the person, the whole history of his previous health and disease, must be present to the mind of him who would influence for good the future course of the patient. And, since the law aspires to deal with cases of lunacy in the way most for their own good and for that of the community, its followers ought to consider the work of the physician as the necessary preliminary to their own, and even condescend sometimes to accept his leading. Instead of this, the practice is to set up (especially in criminal cases) an absolute standard of responsibility and irresponsibility, and to neglect entirely the fact that partial responsibility is much more commonly met with in asylum wards than either of the others. The physician's view is from the inductive side; he learns all he can about his patient before prescribing a treatment. The lawyer's is from the deductive side; he brings forward one or two general propositions, insufficiently proved, and treats the lunatic according to that which seems to fit him best.

Our author is wise in not often treading upon the field which more especially belongs to the medical man. When he does so, want of regard for that essential quality of the physician, called tact, is apparent. Thus, under the heading "Examination of the supposed Insane," we come on the following:

"However, let us suppose those obstacles overcome; the medical man is in the presence of his patient, and the question is, how should he proceed?

"1. He ought to pay considerable attention to the physiognomy of the individual before him."

To put this proceeding, important as it no doubt is, first in the list, is of questionable propriety, for when access is got to the patient, it would be extremely injudicious in many cases to put it directly into practice. In fact, it is often true that the less that is actually done towards examination in the first five minutes of the interview the better. But as for commencing with a scrutiny of the physiognomy, we had rather Mr. Browne than ourselves were exposed to the very probable consequences of so rash an act.

We repeat, there are very many difficulties in performing a task such as the present writer has taken on himself, and it requires a ripe knowledge of not only the law on the subject but of the many facts also which scientific observation of the insane has lately accumulated for us, and, further, of the spirit and tendency of the opinions held by the most advanced medical alienists at the present day, to enable an author to produce a work on this subject which shall prove thoroughly satisfactory to the large body of professional men interested.

We like Mr. Browne best in his expositions of the law of the present day, and in his lucid histories of cases. The latter are numerous, and serve both to throw light upon the general tendencies of evidence, and to illustrate that wonderful uncertainty of decision which, notwithstanding the bold asseveration of general principles by many ardent *à priori* jurists, still reigns in our courts of law, as prominent a feature, certainly, as the stupidity in medical evidence, of which these gentlemen make so much.

The first chapter of the work deals with "Lunacy and Limited Responsibility." We may say of it what holds true of other parts of the work, that without perhaps evolving profound principles on the subject, it gives a good idea of what is the present state of the law. The artificial lunatic of the lawyer, not the madman of nature, is, however, as might be expected, the being dealt with throughout. In regard to criminal cases especially, the illogical alternative, "Either a man must have all his mental powers or none of them," is continually appearing; and the solution which the author puts forward for the difficulties that arise, is unreal. Speaking of the case where the criminal agent, from want of understanding or from mental disease, is not in a position to choose freely, he says—

"It is the same principle that induces the law to exempt very young children from the criminal responsibility of their acts, and the same principle is to be found as the reason for the non-infliction of legal penalties, where the individual is, against his will, compelled to do a wrongful act, inasmuch as the dread of distantly future penalties cannot in reason be expected to prevail against the fear of present suffering. Were it more generally understood—were it more thoroughly appreciated—that it is really the same fundamental principle which induces the law to forego its penalties, even after proof of the criminal act done in these two classes of cases, less difficulty would undoubtedly arise in practice, as to what amount and what kind of insanity is sufficient to establish a claim to immunity from punishment. Were it once held that the proof of that amount of insanity would relieve from the consequences of a criminal act which deprives the individual, either by amentia, dementia, or mania, of that amount of free will or choice—of that power to balance and appreciate motives which is found in the ordinary ranks of mankind—were it held that the amount of insanity which deprives a man of this, as the amount of duress which deprives a sane man of of the same power, would relieve an individual of criminal responsibility, no doubt would, it seems, in any case arise."

This does not, we fear, go quite far enough. For who can always say, from an insane man's actions to what extent his insanity is compelling him to them? True, in some cases, as in acute mania, any one can answer the question, and it is just in such a case that there is never any difficulty. But when a chronic maniac, or a person morally insane, is before the court, who can any more declare that an act in dispute was connected or not with the disease of the mind, than he can assert that the attitudes and gesticulations of the barrister, as he arrives at the grand concluding stage of his peroration, are conscious or ideomotor? "The power to balance and appreciate motives" is one which is possessed by sane men, from the philosopher to the criminal, in the most various degrees; the "higher motives" themselves vary immensely in individual cases, in degree and in character; and it is utterly wrong to take other men's ideas of what their state of mind would have been under the circumstances, as a criterion by which to judge whether insanity was compelling the mind of the accused person. When will jurists learn that mental disease means decay, not of one part, but of the whole; that the individual delusion or impulse is only like the first branch that withers on a rotting tree, that it is in reality the symptom of a weakness pervading the entire organism?

The second chapter deals with the causes of insanity—a subject which it is hardly in the power of any one at the present stage to grasp, and concerning which Mr. Browne contents himself chiefly with attacking some circumstances that are commonly enough and, perhaps, unjustly set up, such as that civilisation is one of these causes. The next chapter is headed "Unsoundness of Mind;" it contains two classifications of the forms of insanity—Ray's, and one of the attempted physical classifications; the former is adopted by the author. The first paragraph of this chapter would be worth quoting had we space; it gives, within small compass, a very clear and useful account of the commoner attributes of insanity, those by which people in general might define the latter.

Amentia is divided, in this work, according to Ray's plan, into idiocy and imbecility. The legal relations of the latter form a very important item in such a book, and we are glad to find them well discussed here. The reasonable nature of the following remarks on the famous Wyndham case is apparent:

"The test of imbecility is undoubtedly conduct, and that conduct which has become fixed and permanent, namely, disposition; and the objection raised by a writer in the 'Solicitors' Journal,' that unless the system of lunacy inquiry be materially changed, every commission may amplify itself into a biographical inquisition, has evidently been made in entire ignorance of the subject. The only reasonable method of coming to a right conclusion in any case of imbecility is from a careful consideration of all the facts of conduct during a long series of years."

We do not think Mr. Browne so happy in his dissertation on the criminal responsibility of imbeciles. It is rather disappointing to be referred to the "questions which arise in regard to general and partial moral mania," for further elucidation of this subject, because one cannot acknowledge that the principles which should guide the treatment of two so different classes of lunatics ought to be identical. Mr. Browne gives two notorious cases, viz. that of Cuthbert Rodham Carr and that of "the Alton murderer;" and although the conclusion that he comes to, viz. that both of these men should have been considered responsible, seems just, we should be far from allowing a parity of reasoning in the case of a moral maniac. The brain of the imbecile is like a house not wholly fitted up, that of the moral maniac is a house shaken to its foundations.

We pass over the "Pathology and Symptoms of Mania," the former of which, with its (alas!) wide scope for fancy, meets with fair enough treatment here, and also the chapter on "Intellectual Mania." That on "Moral Mania" possesses no little interest, since lawyers have, in times past, comported themselves, in regard to this subject, in various extraordinary ways.

The present writer, after stating the current sense of the words "moral mania," remarks upon the error of those who assume the absence of a "moral sense" in cases falling under this head. Since there is no proof of the existence of an independent moral sense, such a faculty ought not to be assumed, either as present or absent, to explain facts. He next defines his idea of the sort of moral sense that is really wanting in moral maniacs, viz. the capacity for realising punishment as the direct unpleasant consequence of a bad act, and he sums up as follows:

"It will, therefore, be understood that repeated convictions on account of the same crime would naturally lead to a suspicion of an amount of incapacity which would justify the law in exempting an individual from criminal consequences; and while such an amount of incapacity is proved in reference to acts occurring in the life of the individual, other than those which have come under the cognizance of courts of law, the presumption is strengthened; and, further, if in conjunction with these circumstances it is found upon inquiry and examination that there is an inherited tendency to insanity, or malformation of the skull—if the history of the case is such as to lead a physician to suppose that it is not impossible that the mind may be diseased—in such a case it seems to us that the law would do well to admit the existence of moral mania, and exempt the individual from the legal consequences of criminal acts."

How the writer can reconcile these sentences with his "opinion that only on very rare occasions should moral insanity stand between the individual and the consequences of his criminal acts," and his assertion that laws enact punishments, "not because Government wishes to punish crime that has been committed, but because the invariable connection between an act and a serious disadvantage to the actor is likely to lead to the discontinuance of the act," we do not care to inquire, for the admissions of the paragraph just quoted are liberal ones for a lawyer to make, and we are well content with them. Nor does Mr. Browne neglect a fact of science which goes far to explain many cases of moral insanity, viz. that motive and consequent action, by habit (or, what Mr. Browne only partly expresses by that characteristic feature of insanity, weakness of the inhibitory or controlling power of the mind), get separated from mind and imprinted on the bodily machinery, so that when the sensations that used to be motive are experienced, the act goes on without the mind being stirred;

"For use can almost change the stamp of nature,"

to borrow a quotation used by the author.

The succeeding chapter on partial moral mania is one of the longest in the book, and treats of the following six varieties of this disease:—Kleptomania, Erotomania, Oinomania, Pyromania, Suicidal and Homicidal mania. Two exceptions have struck us to the otherwise satisfactory consideration of these subjects. The first of these is the author's reference of moral mania to a bending aside of the whole disposition into one channel. This explanation savours too much of theory, not to say of fancy. The other is that he frequently affirms the fallacious proposition which is such a favorite in his profession, that a person mentally unsound in one particular can be perfectly sound in every other respect. Those who are best acquainted with the course of cerebral disease know and have frequently asserted that a general derangement precedes the advent of the particular lesion, and leaves behind it a general weakness.

Following on this are a number of chapters treating of the legal relations of the varieties of mental disease. It is satisfactory to read the clear expositions of existing law given by Mr. Browne in these chapters; though the reasons he adduces in defence of them are not always so pleasing. That upon the legal relations of epilepsy is especially interesting, and shows both that the subject is beginning to attract the attention of jurists, and that they are ignorant, to no small extent, of the work which has already been done by medical men in this field. We ought also to add that Drunkenness, Somnambulism, and Aphasia, are treated at considerable length, and that the views advanced on these subjects are novel and interesting. The work concludes with a consideration of prognosis in insanity, and with directions for examination of the insane.

Much as we should desire to be able to say that a complete treatise on the important and difficult subject of the Medical Jurisprudence of Insanity had been added to professional literature, it seems, we confess, impossible that such should be predicated of any work until a full and clear statement of the views of medical and scientific men, both as regards the legal aspect of mental diseases, and as regards the treatment which, in their opinion, lunatics ought to receive at the hands of the law, has been made. We do indeed strongly recommend medical men to pay attention to these utterances of a lawyer, but this is in the hope that the like attention will in the future be reciprocated on the part of the lawyers. Did these latter study a little more the true nature of mental diseases, less scope would be found for the expression of such supreme contempt of medical opinions which is too often heard in courts of law. In short, results would in all points be vastly more satisfactory were more consideration shown on both sides; and every one will hail the day when some author does full justice to the opinions and feelings of both the lawyer and the physician. Mr. Browne has not been altogether successful in accomplishing this; nevertheless, allowance being made for the unfortunate professional bias displayed in it, this work may be pronounced both readable and worthy of being read.

V.—On Protoplasm.[1]

No one can, we think, mistake the tendency of modern biology; whenever it is possible, and sometimes when it is hardly possible as yet, rule and measure are called in to give exactness of expression to our knowledge of the subject, and exact data are formulated in terms of mathematical precision. Improved chemical processes are gradually enabling us to resolve those waste heaps or dust bins labelled "extractive matters" into their components; and many phenomena which, not understanding, we used to term vital, have been found after all to be readily intelligible on the ordinary principles of chemistry or physics. So many things heretofore supposed to depend directly on the vital principle have been thus resolved that some have gone so far as to call in question the existence of any force different from the physical forces we see at work around us. Undoubtedly the discovery of the correlation of these forces, the fact that one is transmutable into another without diminution and without increase, has had a most important bearing on physiological doctrines. If in the physical world we see motion transformed into heat, this into light and chemical action, chemical force into electricity and magnetism, and so on, we are tempted to make another step in advance. In vegetable life we know that light and heat are the forces whereby carbon is fixed and oxygen set free, whereby ammonia is converted into complex organic compounds, and an infinite variety of other changes is effected. In animals it has been found that digestion is a chemical process, absorption to a great extent a physical one; respiration is partly physical, partly chemical; not a muscular movement can be performed without the consumption of fuel in some shape or other; in short, in all these so called vital acts the physical forces are equally manifest as if there were no complex organic medium through which to exert their influence. The question therefore arises, Suppose we take the simplest

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[1] 1. *The Fortnightly Review* (February 1*st*, 1869). "*On the Physical Basis of Life.* By Professor HUXLEY. London.

2. *Protoplasm or Life, Force and Matter.* By LIONEL S. BEALE, M.B., F.R.S., Fellow of the Royal College of Physicians, Physician to King's College Hospital. Second Edition. London.

3. *As regards Protoplasm in relation to Professor Huxley's Essay on the Physical Basis of Life.* By JAMES HUTCHISON STIRLING, F.R.C.S., LL.D. Edin. Edinburgh.

4. *Essays on Physiological Subjects.* By GILBERT W. CHILD, M.A., F.L.S., &c., Lecturer on Botany at St. George's Hospital. London.

5. *The Cell Doctrine, its History and Present State, for the Use of Students in Medicine and Dentistry.* By JAMES TYSON, M.D., Lecturer on Microscopy in the University of Pennsylvania, &c. Philadelphia.

organism known, and consider well its functions, is there anything in these not explicable by physical force; shall we be forced to call in some mysterious entity of which we know nothing, to explain the terms of its being? If we let fall a drop of white of egg yet unboiled, into water, this drop retaining its individuality and mixing not with the water, represents the simplest organised, or rather we might say unorganised, being. Haeckel calls it *monera,* but herein consists the difference: this monera having no other structure than a drop of albumen, can move from place to place of its own accord, can change its shape in a thousand ways, can take in and make part and parcel of itself other and larger portions of albuminous matter, and can reproduce itself by throwing off a portion of its own substance, and all this it does by virtue of something we call life.

But it has been roundly asserted that this property is inherent in the kind of matter with which we are now dealing; where it exists life exists, and where it does not exist no life is, insomuch that it has been called the *physical basis of life;* let us call it *protoplasm.*

The questions in dispute seem to formulate themselves thus:—Does the existence of *protoplasm,* using the word in a strict sense, imply life; may life and matter co-exist whilst the latter assumes no distinctive form; can life begin *de novo* and without the agency of pre-existing life?

But to the preliminary inquiry of what is protoplasm? using the word in the strict sense, we obtain no adequate reply. We are told that all forms of it contain carbon, hydrogen, nitrogen, and oxygen in certain proportions, but what these are we know not. Protoplasm, in a strict sense, is not known chemically. If we seek for an example of protoplasm, we are referred to the well-known experiment with frog's blood. A drop is drawn from a blood-vessel of the animal and deposited on a piece of thin glass; this is placed under the microscope, and kept at a temperature of about 100° F., when by-and-by the white corpuscles will be seen to approach the edge of the blood drop, exhibiting distinct movements of the kind called amœboid. Similar bodies are found in connective tissues, some wandering, some fixed, but apparently otherwise identical. To these we are referred as samples of protoplasm, but we are ignorant as ever of its chemical composition; indeed, this would seem to be far from simple even in leucocytes; in fact the type of protoplasmic bodies would appear to have at all times during life a certain proportion of fat intimately combined with its albuminous matter, the separation of these two constituents being the first certain sign of the death of the body. Professor Huxley tells us that all the forms of protoplasm yet examined behave similarly towards several reagents; if this be true of protoplasm, it is certainly not true of the type of such bodies, albumen itself; for this sometimes will coagulate with acid, and sometimes will not; sometimes too it will coagulate with heat, and sometimes it will not.

Syntonin, globulin, and peptone are all varieties of albumen; of all may different characters be predicated, but of none can it be said that it is the matter of life or protoplasm. Nay, if we fall back on what we are told is the type of protoplasmic structure, a white blood-corpuscle or an amœba, we find in its body certain portions apparently constituted differently from the rest, inasmuch as by a reagent such as acetic acid we can demonstrate the existence of particles called nuclei, which behave differently from the rest of the structure. The same is the case if the body be killed by an electric shock, without any chemical reaction. It is, however, perfectly true that in monera there is no nucleus, which shows that this is not quite essential to the performance of all the acts of life, but it is also quite true that in those bodies possessing a nucleus, changes in it generally precede changes in the mass of the being itself. If possible, this nucleus shows a more vigorous vitality; is it protoplasm? If so, it reacts differently from the rest of the mass.

If, therefore, we mean by protoplasm merely the simplest form of *living* matter, that is, if we make vitality the test of its existence, we can understand the meaning of the word. But it is plain that protoplasm is not a simple substance in all instances chemically identical; in this sense it is a varying admixture of more than one body of albuminoid constitution, reacting differently with different reagents. Its presence is not the test of life, but life of its presence. Professor Huxley himself hints at these differences of properties when he speaks of the difficult digestibility of lobster protoplasms as compared with that of sheep; in other words, these two do not behave exactly in the same way towards the reagent we call gastric juice.

There is, however, one sense in which we may make use of the word protoplasm with advantage; to illustrate this we must fall back on matters of history. When Schleiden had shown that the plant was built up of a series of elements, as a house is of bricks, he investigated the structure of these elements, and found that in every instance they seemed to possess a cellular wall, slimy contents, and a nucleus, besides other things, as starch, chlorophyll, and such like. Schwann adapted these principles to the human body, and applied the term *cell,* introduced by Schleiden, to designate these ultimate vegetable elements, to designate also those of the human frame. Furthermore, a similar constitution was predicated of them; that they consisted of cell walls, slimy contents, and nucleus; in short, that they were in every way analogous to the vegetable cells. But in 1835 Dujardin, then concerned in his inquiries into the Rhizopoda, discovered that they consisted of a substance irritable and contractile, having nothing of a cellular character; it was called *sarcode*. Researches went on, and new discoveries were made, especially that the slimy substance of many animal cells was also contractile, until at last Max Schultze was able to show that the contents of animal cells were analogous to sarcode, and that the cell walls were of comparatively little consequence compared with their contents. In the days when this universal character was predicated of all animal structures, blood corpuscles, red and white, were a standing difficulty; by no means could the red globules be reduced to the highest or nucleated cell type, and by many they were held to be free nuclei. White corpuscles were more easily managed, for by means of reagents something like a cell wall and multiple nuclei could be made out. But when these last named bodies had been better studied, and their curious movements, exactly resembling those of a *protist*, became known, their analogy to a mass of sarcode was recognised, and it was admitted that a wall was no longer to be held as an essential portion of an animal cell. This new advance may be said to have been made simultaneously by Max Schultze and Lionel Beale. These two observers, however, looked at the matter from different points of view. Schultze considered chiefly the irritability of the cell contents, to which he gave the name of protoplasm, whilst Beale, looking rather at one definite chemical reaction, considered all material readily tinged by carmine solution as germinal matter, and that not so affected as dead or formed material. The selection of these terms seems to have been peculiarly unfortunate; they have never been generally accepted, nor, perhaps, even understood, and their use, in a fashion to many unintelligible, has done much to deprive Beale of that reputation most justly his due, and to cause to be assigned to German discovery that which had long before been done among ourselves. At all events, the results of Schultze's investigations entailed the rejection of the cell wall as an essential portion of the animal cell, which he considered to be made up of a particle of protoplasm and a central nucleus, sometimes having the outer portion of the protoplasm condensed so as to constitute a kind of membranous wall, or even modified into a substance not protoplasm, and constituting a true wall. "But," said he, "in fact, a cell with a membrane chemically different from protoplasm is comparable to an encysted infusory." Nor did matters halt here. Brücke went further, and denied the necessity of a nucleus in the ideal animal cell; he based his objections mainly on the cells of certain cryptogams in which no nucleus was known. "And," he argued, "as there is nothing known either of the constant presence of the nucleus or of its functions when present, why should we include it in our conception of what is essential to a living portion of animal matter, commonly called an animal cell?"

This notion as to the non-essential character of the nucleus was speedily confirmed by the discovery in the Adriatic of an amœba (*A. porrecta*), by Max Schultze, devoid of nucleus; this again was followed by the discovery in the Mediterranean of *Protogenes primordialis*, a larger form, also non-nuclear, by Haeckel. Other kindred organisms were found by Cienkowsky, and, finally, Haeckel found in the Canary Islands that structureless *protist* he has so well described in his monograph as Monera. This irritable and contractile substance then, the sarcode of Dujardin, the protoplasm of Max Schultze, the germinal matter or bioplasm of Beale, so constituted as to be able under appropriate circumstances to maintain an independent existence, yet corresponding with the active material inside true cells, may be spoken of and considered histologically as protoplasm. Chemically, it agrees with a whole group of protein or albuminoid bodies. If, therefore, we use the word protoplasm chemically as synonymous with protein or albuminoid substance, it is nothing new, and we are certain that it is not, in the meantime, capable of a more exact definition by chemistry.

Of all chemical substances, perhaps protagon most closely approximates to what is called protoplasm in character, but neither of that, even although it tends to assume shapes resembling cells, can it be said that it is protoplasm.

But though incapable of this close and exact definition, the term is nevertheless a very useful one. It expresses the idea which it is so desirable to bring out, that between animal and vegetable matter there is no great gulf fixed. It is known that certain forms of life are at one time similar to vegetable, and at another period of life history more like unto animal organisms; it is well then that there should be a common title for the substance, which may thus be impressed with either an animal or a vegetable character, but which in essentials remains the same.

There is yet another way in which the term is valuable. It is plain the term 'cell' as applied to a body possessing the characters of a leucocyte, whether wandering or fixed, is a misnomer, and yet a very considerable number of what are now frequently called *elements* are constructed on this type. For these it is desirable that the term cell should be abandoned, and though the substitution of *protoplasmic mass* for *cell* may be the substitution of a long term for a short one, it has the merit of conveying no false impression.

As already pointed out, the characters of such a protoplasmic mass as monera or a white blood-corpuscle are irritability, contractility implying the power of changing shape and station, nutrition and reproduction.

But apart from these characters, protoplasm may be active, resting, or dead.

By irritability we merely imply the possibility of the powers of a protoplasmic mass being called into play by some agent external to itself, and of these the simplest and most efficient is heat. If we take a portion of the leaf of an *Anacharis alsinastrum*, now so common, or of a Vallisneria, surround it with a drop of water, and cover it lightly with a thin piece of glass, we may, if the temperature be rather low, see nothing save the cellulose compartments dividing the cells one from another, and the cell contents containing abundance of chlorophyll granules; but if we use a lamp for our illuminating agent, its rays of heat are reflected on to the leaf as well as its light-giving rays, so that presently we see active motion of the cell contents, as indicated by motion of the chlorophyll granules, set up in every direction. Here is the stimulus of heat setting up motion in the previously resting protoplasm.

This is still better exemplified by the behaviour of the leucocytes contained in the circulating blood of a triton or salamander. The experiment is easily made by any one who can command the means of warming the stage of a microscope, and for this no elaborate apparatus is needed. Care must also be taken that the drop of blood does not dry, so it must be placed in a cell and surrounded by vapour. When withdrawn from the veins of the animal, the white corpuscles are seen as rounded, well defined masses, presenting a marked contrast to the elliptical corpuscles of the red kind. This is the resting condition when no particular stimulus is acting upon them. For as the observer watches these rounded bodies change their shape, they probably become elongated and more transparent; presently their edges become jagged, and a series of active changes of shape ensue. These changes become more active up to a temperature, a few degrees above a hundred, but if this limit be transgressed their motion ceases, and in no wise can it be made to begin again; the leucocyte is dead. But apparently intermediate between the conditions of active life and motion, and irretrievable death there is the condition of rest, whether of repose or tension is not quite clear. At all events, this can be done; leucocytes of the blood can, by electric stimuli, be made to gather together into a clump, which after a time, if the stimulus has not been too strong, gradually resumes the mobile condition. But with the protoplasm of a cartilage-corpuscle this is not possible; contraction of the protoplasmic mass on its nucleus may be easily brought about, but no further change results; it is dead. Nevertheless, we have no reason to believe that the electric stimulus has in any way altered the chemical properties of the substance; it will imbibe carmine as readily as ever. Yet though the protoplasm is there, life has departed. The existence of protoplasm cannot be said, therefore, to predicate even the potential presence of life.

Nay, more, protoplasm as studied in the living body presents different degrees of vitality. The connective-tissue corpuscles, of which one has heard so much, seem to be of two kinds, some fixed like the corneal corpuscles, which give out processes in various directions, some like the cartilage-corpuscles, resembling in outline a resting leucocyte, but not possessing, as pointed out above, the same degree of inherent vitality. Others again are found in various tissues, emigrants from the blood vessels, though now in a condition of rest, whilst the most active members of the class seem to be the leucocytes of blood and lymph.

These, as we have seen, readily respond to stimuli; they also grow, as we infer from their presence, in very various sizes in the interior of the vessels, and they are capable of reproduction by fission, in all probability within the vessels and glands, certainly outside them, when they have wandered into the surrounding tissues. It is very strange that doubts as to the truth of the phenomena we have referred to should still exist in the minds of many when they are so readily capable of proof. The changes in the corneal corpuscles of the frog after irritation as by nitrate of silver, are easily studied, especially by the method of staining with chloride of gold, now in vogue. The migration of white blood-corpuscles may now be studied by any one who can command a tolerable microscope, and can get hold of a frog. By all these phenomena we are impressed with the truth of that view which teaches that it is the living matter of the so-called cell which is of importance; its outward configuration or its envelope matters little. It is absolutely necessary to have a name for this irritable and contractile substance, and we know of none better than protoplasm; but protoplasm may be alive or it may be dead; in the latter state it is neither irritable nor contractile.

It is clear, therefore, that the presence of life implies no specific form, neither does the existence of protoplasm imply the presence of life (its pre-existence is another question). The final question arises, May life begin *de novo* without the agency of pre-existing life? To this, after due consideration, we are bound to reply, No, not as far as our knowledge goes. It would be beside the purpose to argue the question whether dead protoplasm can come to life again under the influence of the simple forces heat, light, &c., because until we can manufacture protoplasm it is not possible to say that we do really begin *de novo*. And yet this is the only form of the question which admits of argument. We know of no chemical means whereby the simple elements already alluded to oxygen, hydrogen, nitrogen, and carbon, with sulphur and phosphorus, can be combined so as to resemble living matter, or the matter of living beings. And the idea that living beings can be formed from matter which does not contain all, or at all events four of these elements, may be surely put on one side, so that any admixture of compounds of these not containing all of them cannot by any possibility be converted into a living being. The argument as to whether dead protoplasm can come to life again is a vain one until both parties can agree upon a distinct boundary line between life and death, and on an unfailing test for matter in either state; an agreement which we need hardly say does not exist at the present day.

To our minds reasoning from analogy has always seemed a powerful weapon if used aright, and in the present inquiry it cannot be denied that the analogy is entirely on one side. If we trace the history of the simplest protoplasmic masses, they begin as exceedingly small offshoots from a parent like themselves. As we see them in the animal body, they are surrounded by food, and rapidly grow. Yea, leucocytes as they exist in living animals can be fed on special substances. In the frog white corpuscles often devour portions of red ones. If aniline blue is introduced into a lymph sac the leucocytes ingest the blue particles, which may be seen in their interior as chlorophyll is seen in the protoplasm of a vegetable cell. The same occurs if they are fed with carmine, and so these corpuscles may be traced throughout the body. In inflammation such marked bodies are seen rapidly dividing and rapidly growing. Even in the living body we have every reason to believe that the doctrine *omnis cellula e cellula* is true when *cellula* means a mass of protoplasm. Outside the body the rule is the same; when animals are unicellular, their history is like that of a leucocyte inside an animal. The more plentiful their food, the greater the heat within the limits already pointed out, the more do they multiply and increase, and if any one reflects on the rapidity with which an abscess forms within the organism he can readily understand that a similar rapidity may be observed outside it. But this, again, is to be borne in mind that a high temperature is not absolute, but relative, as in the well known instance of the ova of certain fishes (*ex. gr.* the trout), which proliferate rapidly at a temperature which would kill many forms of animal life, and which would themselves be destroyed by a greater heat. *Omne vivum e vivo* is, as far as we know, the rule. That there seem to be exceptions every one admits; that these exceptions are real and not apparent only is another thing.

In dealing with the history of protoplasmic masses we found that though it was easy to tell the active from the resting form of a leucocyte, it was by no means easy to tell the resting form from the dead condition save by the separation of fat and gradually advancing decay. Experience with regard to infusories teaches that in them the distinction is, if possible, harder, and by the distribution of such protoplasmic masses in a resting state doubtless many of the instances of so-called heterogeny may be explained. With experiments like those of Pasteur, on the one hand, and those of Pouchet, Bastian, and others, on the other, a hasty judgment would be the acme of rashness. Nevertheless, the one view has the analogy of all animated life to support it, the other merely the care of the experimenters or their reputation for care; and on them, too, lies the *onus probandi*. We can only hope that in due time their care will be rewarded, and truth, the object of them all, attained.

Finally, the active and the resting conditions of protoplasm are, to the medical world, of the very utmost importance. The two conditions explain much with regard to the spread of epidemic and contagious disease, without calling in the aid of any mysterious bodies, germs or otherwise. They encourage the use of antiseptics or bodies capable of unmistakably killing protoplasmic particles, be they what they may. And as the size of a particle of protoplasm which may rest for a time, and again, with favourable conditions resume activity is unknown, but is in all probability infinitely small, they suggest the utmost care in the destruction of these.

VI.—Holmes's System of Surgery.[1]

The success of a great work like that before us must be considered undoubted when a second edition is required within ten years of the publication of the first volume of the original issue. Mr. Holmes is an indefatigable writer and editor, and manages

[1] *A System of Surgery, Theoretical and Practical, in Treatises by various authors.* Edited by T. Holmes, M.A. Cantab., Surgeon and Lecturer on Surgery at St. George's Hospital. Second edition in five volumes, with illustrations. London, 1869, 1870, and 1871.

to keep his team of authors in hand as regards punctuality, in a manner which must make the publishers and subscribers of other compound works which wearily drag their slow length along, ready to gnash their teeth in despairing envy. We could have wished, however, that Mr. Holmes had exercised a little more autocratic power over some of his authors in preparing a second edition, so as to have led to a more uniform perfectness of work. For, high as is the standard of excellence of the work as a whole, some of the essays fall very short of perfection, and, curiously, those which in the first edition were most behind-hand, occupy the same position in the second, their authors being apparently incapable of effecting any improvement in their respective productions. Under these melancholy circumstances it must become a question whether the subjects should not, when another edition is called for, be placed in other hands.

The expansion of the 'System' into five volumes allows of a somewhat clearer type, and the introduction of a considerable number of woodcuts. The first volume also contains some chromo-lithographs, of no great merit, to elucidate the subject of syphilis; and a series of beautiful lithographic plates from drawings by Mr. Henry Arnott, illustrating the microscopic structure of tumours, which, together with the descriptive letter-press, form a most valuable addition to the essays on "Tumours" and "Cancer."

We propose now to pass the several essays briefly in review, with the object of indicating, so far as may be, the differences in the new edition as compared with its predecessor.

Mr. Simon's essay on "Inflammation" is practically unaltered, save by the omission of the description of the process of inflammation, and of the pathological summary of the first edition. These are supplied, together with the most recent researches bearing on the subject of inflammation, by the article in the fifth volume, "On the Process of Inflammation," by Dr. Burdon-Sanderson. This most valuable summary of modern research gives the effects of injurious irritation of the tissues under the following headings: (1) disorder of the circulation; (2) transudation of the constituents of the blood; and (3) altered mode of growth of the elements of the inflamed texture. Under the first heading Dr. Sanderson shows that the effect of irritation of a vascular membrane is immediate and continuous dilatation and lengthening of the vessels, without that antecedent stage of contraction formerly described. The rate of circulation of the blood is at the same time increased temporarily, but this soon subsides; and by the time the arteries are fully dilated the circulation is much slower than it was originally. The results of investigations on the vaso-motor function of the cerebro-spinal system show that section of a vascular nerve produces congestion of all the tissues to which it is distributed; that excitation of a nerve produces constriction of the vessels, and consequent anæmia of the part it supplies; that excitation of a sensory nerve produces increased activity of the capillary circulation in the part in which the nerve originates; and that all arteries manifest alternating states of contraction and dilatation, independently of the heart and lungs, but controlled by the nervous supply. In the second part the exudation of liquor sanguinis and the migration of leucocytes through the capillary walls are fully described and investigated. Justice is done to the researches of Williams, Addison, and especially of Waller, who so long ago as 1846 placed on record most of the facts to which attention has been recently drawn by Cohnheim and others. Without giving all the details of the experiments, it will be sufficient to note here that it is now generally conceded that the white blood-corpuscles find their way through the walls of the capillaries, and are identical with pus-cells, though it is by no means proved that the cells of the tissues do not, in addition, form pus-cells in the later stages of inflammation.

Under the head of "Stasis" it is shown that the accumulation of blood-corpuscles, so familiar to all observers, is not due to alterations in the blood itself, but in the walls of the vessels, or in the surrounding tissues; and it has further been shown that, contrary to received notions, the walls of capillaries during inflammation undergo a process of fatty degeneration tending, doubtless, to permit the passage of corpuscles through their walls.

Dr. Sanderson next considers the changes which take place in the tissues in inflammation, premising that he believes the multiplication of cells, &c., to be due solely to the soaking with liquor sanguinis, and in no respect dependent upon the influence attributed by some pathologists to "an unknown nervous centre, supposed to preside over nutrition." The results of inflammation upon non-vascular structures, such as cornea, cartilage, and tendon, and of vascular structures, such as connective tissne and muscle, are then given in detail, together with the structural changes which occur in the epithelial and glandular tissues in inflammation. In part 3 the influence exercised by the form and mode of action of the injurious agent on the character of the resulting textural changes is discussed, and the relation between inflammation and the reparative process is described. Suppuration is shown to depend upon the tendency of leucocytes after escaping from the vessels to collect together in groups, so as to form foci of suppuration; but Dr.

Sanderson confesses himself unable to explain why one inflammation should be suppurative and another not so. An interesting and ingenious experiment of Stricker's, on the corneæ of frogs, is given in detail, in order to show that the structural changes occurring in inflammation of that tissue cannot be dependent on any influence exercised by the nervous system, or by transmission of the irritative effect from one structural element to another.

We have thus epitomised the leading points in Dr. Burdon-Sanderson's most valuable and interesting article, and we proceed to give his conclusions in full, since they cannot fail to be of service to all engaged in pathological research; they are as follows:

"1. In every inflammation which attains its full development the changes which manifest themselves in the inflamed part are of three kinds, distinguished from each other according to the organs which are concerned in their production. They are either (1) effects of disorder of the vascular nerves and centre, (2) effects of alteration of the properties of the living walls of the capillaries, or (3) effects of the stimulation of the living cells by transudation of liquor sanguinis.

"2. Of these three orders of phenomena the second only can be regarded as absolutely essential to the existence of inflammation, which may, therefore, in the strictest sense, be said to have its seat in and about the veins and capillaries, it being there that the earliest and most constant effects of irritation or injury manifest themselves.

"3. The nervous and vascular effects of local irritation cannot be directly described as successive stages of one process, for the determination of blood to the seat of injury, which is the sole result and, if I may so speak, purpose of the vaso-motor disturbance, has no relation to the local vascular changes, excepting in so far as it tends to make the exudation more abundant. Exudation of liquor sanguinis, although favoured by increased arterial afflux, may occur without it, and, as a rule, continues after the afflux has ceased. The vascular and textural changes, on the contrary, may be regarded as successive stages of one process, for they are connected by a casual relation—the exudation of liquor sanguinis, in which the former ends, being the determining cause of the latter.

"4. The mode in which an injury changes the living substance of the vascular walls so as to make them permeable to the blood is unknown, the only clue which we have to its character being that afforded by the structural alterations to which it leads in certain organs, and particularly by those which are observed when the process of reparation, attended with the formation of new capillaries, is commencing. From these appearances we are led to infer that the primary change consists in the transition of the material from the formed to the plastic condition; from a state in which it is resistant, because inactive, to one in which it is more living, and, therefore, more liable.

"In all living tissues the effect of inflammation manifests itself in a modification of the action and properties of individual cells. In cells which form part of permanent structures the protoplasm increases in quantity, and becomes more or less contractile. Subsequently it is converted entirely or partly into young cells, either by cleavage or by endogenous germination."

Returning to the essays in the first volume, we come to that on "Abscess," by Mr. Holmes Coote; and as that gentleman apparently favours Virchow's views as to the development of pus from the connective tissue, and only casually alludes to the transudation of blood-corpuscles, it is but fair to remind our readers that the article was in type some two years before Dr. Sanderson's article, to which we have just been referring. The author refers very briefly to the proposals to limit suppurative inflammation by ligature of the main artery, as practised in America, and by Mr. Maunder in this country, or by digital compression, as suggested by Vanzetti and Nélaton—a method which appears to have been scarcely tried in this country. No opinion on either subject is, however, expressed. The same thing may be said respecting the antiseptic treatment of abscesses by Lister, which is shortly described. The same author's article on "Gangrene" includes now the subject of phagedæna, which is regarded as "an insensible transition from ulceration to mortification." Mr. Coote believes phagedæna to depend upon a state of septicæmia, and that it is *not* contagious, in which, probably, many surgeons will not agree with him. The articles by Sir James Paget on "Sinus" and on "Ulcers;" by Mr. De Morgan on "Erysipelas," and by Mr. Callender on "Pyæmia," have undergone little if any alteration requiring notice. Mr. Croft's article on "Hectic and Traumatic Fever, and on the Treatment of Cases after Operation," is properly enough brought out of the appendix, and placed after that on pyæmia, and is followed by Mr. Poland on "Tetanus," Dr. Barclay on "Delirium Tremens," and Mr. Savory on "Scrofula" and "Hysteria." We observe that Mr. Croft has added some typical thermographic sheets; that Mr. Poland discusses the diagnosis between tetanus and strychnia-poisoning, particularly in relation to Cook's case, and gives a lithographic plate, showing the microscopic appearances found in a tetanic spinal cord; and that Mr. Savory summarises the experiments on inoculation of tubercle, and finds that they "afford no support to the doctrine that tubercle is a specific disease."

Mr. Henry Lee has entirely rewritten his article on "Syphilis," which is now one of the best compendiums of information on this important subject we are acquainted with. He broadly draws the distinction between the suppurating non-infecting sore and the true infecting chancre, of which he describes three modifications, and remarks that two of these, the peeling off of the cuticle, and the formation of an indurated tubercle, are often not recognised as sources of syphilitic infection. The question of auto-inoculability is very clearly put, and it is shown, in direct opposition to the teaching of a few years back, that the infecting sore is not auto-inoculable, while the non-infecting sore usually is so. Cauterization or excision of a true chancre are found by Mr. Lee to be perfectly useless, and he pins his faith on mercury as the only real antidote to syphilis. The effect of previous disease on syphilitic inoculation is shown by Mr. Lee to be very marked.

We are glad to find a chapter devoted to the transmission of secondary syphilis, since many members of the profession still doubt its possibility. The evidence Mr. Lee quotes from his own observations, and from those of Dr. Marston, shows conclusively that secondary syphilis can be transmitted and produce constitutional infection. Mr. Lee quotes a remarkable case of a woman who nursed a syphilitic child with one breast (which became infected), and her own healthy child with the opposite breast. The mother developed a secondary eruption, but her child remained healthy, and he regards this as conclusive that the secretions of a diseased body do not convey infection. This is a somewhat dangerous conclusion, it appears to us, and one which we cannot regard as proven when we find well-authenticated cases of transmission of syphilis to a hand cut against the tooth of a syphilitic patient.

In the treatment of syphilis, in its primary and secondary forms, Mr. Lee is, as we have said, a firm upholder of mercury, and he relies particularly on the mercurial bath as the safest and readiest means of administering the drug. He also recommends local mercurial fumigations to parts affected with primary sores.

Sir James Paget's article on "Tumours" has been edited by the late Mr. Charles Moore, whose article on "Cancer" follows. Mr. Moore has written an introduction to the entire subject, in which he supports the primary local character of cancers, and shows that the distinction between them and simple growths is less and less marked the more the subject is investigated. He has added to Sir J. Paget's article short descriptions of the tumours described by Virchow as myxoma or mucous tumour, psammoma or tumour containing sand, and glioma, the fibro-cellular tumour found in connection with the retina. Mr. Moore's article on "Cancer" contains the result of his enlarged experience, but does not require especial notice.

Following appropriately on these articles are twenty-four most beautiful microscopic drawings of the structure of tumours and cancer by Mr. Henry Arnott. A short but clear description accompanies each drawing, and the series form a most valuable addition to the work, and will be of great service to microscopists as standards of comparison.

The articles on "Contusions" and "Wounds" by Sir J. Paget, on "Animal Poisons" by Mr. Poland, on "Wounds of Vessels" by Mr. Moore, and on "Collapse" by Mr. Savory, conclude the first volume.

Local injuries are brought together in the second volume, and comprise the essays on "Burns and Scalds," by Mr. Holmes; the "General Pathology of Fractures," by Mr. Hornidge; the "General Pathology of Dislocations," by Mr. Holmes; "Gunshot Wounds," by Mr. Longmore, in which the qualities, kinds, and forms of projectiles rcceive especial notice on the present occasion; and "Injuries of the Head," by Mr. Prescott Hewett, in which depression of the brain following contusion, and inflammation after contusion, are discussed for the first time.

Mr. Shaw's essay on "Injuries of the Back" has been considerably expanded by a lengthened reference to sprains of the back—a subject of increasing interest in connection with railway injuries. Mr. Shaw's experience of severe sprains of the back is, that they do well if there are no symptoms of paralysis immediately produced. Paralysis may be due wholly to extravasation of blood within the theca vertebralis; and in these cases the patient often slowly recovers. Inflammation of the joints of the spine may result from sprain, and so also suppuration in the spinal canal, with consequent paralysis; of which latter affection two interesting cases are quoted from Sir Charles Bell and Mr. Simon. Concussion of the spinal cord is discussed at length by Mr. Shaw, who does not, however, approve of the term, and believes that, as has been shown by Mr. Hewett in the analogous case of concussion of the brain, there are frequently actual pathological changes which account for the symptoms put down to mere concussion.

The progressive paraplegia due to chronic degeneration of the cord resulting from injury of the spine is illustrated by reference to one of the few recorded post-mortem examinations, which has already been related by Mr. Erichsen and by Mr. Le Gros Clark. Mr. Shaw draws attention to the disproportion between the slight injury sustained and the magnitude of the results, and asks, Was the shock in the railway accident a cause or merely a coincidence?

The late Mr. Henry Gray's article on "Injuries of the Neck" has been entrusted to Mr. A. Durham, who has considerably added to and improved it. The portion relating to foreign bodies in the air-passages and tracheotomy is now especially full and good, and is illustrated by several drawings of instruments. The operation of tracheotomy is well described, and though recommending the fixation of the trachea with a hook before opening it in most cases, Mr. Durham describes also and speaks well of the practice of fixing the trachea with the finger and thumb, and cutting rapidly upon it in cases of urgency. Mr. Marsh and Mr. Holmes are quoted to show that the calibre of the cricoid cartilage is considerably less than that of the trachea, and that this again is smaller than that of the fully expanded glottis; and it is concluded, therefore, that moderate-sized tracheotomy tubes are sufficient for all respiratory purposes. With the view of readily adapting a tube to the depth of the neck, and also of obviating ulceration of the trachea from pressure, Mr. Durham has devised an ingenious modification of the ordinary tube, the only drawback to which is its price. The question of gastrotomy is discussed in connection with injuries of the œsophagus, and two tables are given, one of nine cases, in which the stomach was opened on account of stricture of the œsophagus, all of which were fatal, the latest on the thirteenth day; the other of seven cases of the operation for removal of foreign bodies, all of which recovered!

Mr. Poland has added to his article on "Injuries of the Chest" a description of the dislocations of the ribs, and elaborate tables of recorded examples of these injuries. The subject of wounds of the heart has also received more attention than in the former edition, and a table of 452 cases of the kind is appended. Mr. Pollock's article on "Injuries of the Abdomen," and Mr. Birkett's on "Injuries of the Pelvis," present no alteration requiring remark. Mr. Flower's essay on "Injuries of the Upper Extremity" has been edited by Mr. Hulke, and both this and the essay on "Injuries of the Lower Extremity" by Mr. Holthouse are much improved by the addition of woodcuts. At the same time we must protest against the outlines of the head of the femur, shown in the dislocations figured at page 898, since they give a most erroneous idea of the position of the bone; and we may note that Mr. Holthouse's attention does not appear to have been called to Bigelow's recent work on the hip, and the anatomy of its dislocations, in time for him to modify the old received methods of treating these injuries.

The third volume opens with Mr. Dixon's elaborate treatise on "Diseases and Injuries of the Eye," which extends to 256 pages, and is divided into thirteen chapters. The order of these varies somewhat from that of the former edition, the first two being devoted to optical defects and to the examination of the eye. The subjects of myopia, hypermetropia, astigmatism, &c., are well explained, and the theory and use of the ophthalmoscope are clearly described and illustrated by lithographic plates. The diseases of the several structures of the organ are then described, with the treatment of each, and the essay forms altogether one of the most readable and reliable treatises on the subject with which we are acquainted. Mr. James Hinton has made a few additions to his article on "Diseases of the Ear," including the subjects of relaxation of the membrana tympani: Politzer's method of inflating the Eustachian tube, which is one of the few modern improvements in aural surgery, and the diagnosis of affections of the tympanum with the aid of the tuning-fork. The article on "Aneurism" by Mr. Holmes is one of the most important in the work, and gives, we believe, one of the best reviews of the whole subject to be found in the English language. The author has evidently taken great pains to work up the literature of the subject, and has added much interesting matter to the former edition. The subjects of acupressure and temporary ligature, of treatment by rapid compression under chloroform, of direct pressure, and of treatment by wire introduced into the sac—a plan once followed by the late Mr. Moore, but not likely to be undertaken again—are all fully described. The subject of the distal ligature by Brasdor's or Wardrop's operation has received particular notice; and Mr. Fearn's and Mr. Heath's well-known cases of double ligature for aneurism of the root of the neck are specially referred to. A valuable table of cases of distal ligatures is appended, and an instructive case by M. Broca of innominate aneurism treated by ligature of the subclavian is published for the first time. At the end of the essay is also an interesting account of the ligature of the first part of the *left* subclavian artery by Dr. Kearney Rodgers, of New York.

Mr. Tatum's essay on "Affections of the Muscles" has been in part rewritten by Dr. Lockhart Clarke, who has contributed a valuable addition on muscular atrophy, including granular, fatty, and waxy degeneration, upon which the author's well-known and elaborate researches on the nervous system are shown to have a direct bearing. Dr. Little's article on "Orthopædic Surgery" is reproduced with but slight modifications, but with several additional woodcuts illustrating various deformities. Mr. Holmes' essay on "Diseases of the Bones," with which the third volume concludes, has also been much improved by the addition of numerous woodcuts from specimens in the museums of the College of Surgeons and St. George's Hospital. We note that the author has somewhat modified his opinion as to the existence of osteo-aneurism, of which a doubt was expressed in the former edition. Cases recorded by Dr. Mapother and Mr. Bickersteth seem to prove conclusively the existence of pulsation in innocent tumours on long bones displaying no other recognisable structure than enlarged blood-vessels.

The fourth volume opens with Mr. Athol Johnstone's article on "Diseases of the Joints," which, like the preceding essay, has been freely illustrated from specimens in St. George's Hospital Museum. It is one of the drawbacks of the arrangement of a work of this kind that the important question of excision in cases of diseased joints is not considered in connexion with the diseases themselves, but by a different author in another volume. Mr. Shaw's essay on "Diseases of the Spine" has no material alteration, and is followed by a valuable paper on "Nerve Lesions and their more Immediate Effects," by Dr. Lockhart Clarke. He believes idiopathic neuritis, though less common than traumatic, to be more common than generally supposed, and to depend upon exposure to cold, excessive bodily exertion, &c. Several cases are quoted in which the large nerves of the body were found red and swollen, and had produced excessive pain during life. Neuroma (for which the author gives no cure but the knife), tubercle, and cancer of nerves are considered, and then the effects of various injuries. Perhaps the most valuable and interesting part of the essay is that referring to the nutritive changes resulting from injuries and diseases of the nerves. These effects have been described by various authors; but the most complete account is that given by Drs. Mitchell, Morehouse, and Keen, of the United States, who had abundant opportunities of studying the effects of division of nerves by gunshot injuries, after the American civil war. In entire division of a main nerve there is of course palsy of the limb, which becomes œdematous. The skin thickens and dries, the epithelium hangs in patches, and the nails become curved. In partial division the skin is red or mottled, the surface glossy and free from hair, and there is always pain, generally of a burning character. Dr. Duchenne has shown also that complete division of a nerve leads to slow atrophy of the muscles, but that partial division only affects the muscles whose supply-fibres are severed, but the atrophy is much more rapid. Dr. Clarke confirms Dr. Duchenne's statement, that electro-muscular contractility is not necessary for the exercise of *voluntary* contraction; but "whenever this contractility is lost, or even impaired, the prognosis in reference to the return of *voluntary* control indicates that the affected muscles will suffer in their nutrition and become atrophied." It must be always borne in mind, that "in paralysis resulting from *cerebral* disease, the electro-muscular contractility remains unaffected, and therefore Faradisation is of great importance in the differential diagnosis of paralysis which follows injuries of nerves or affections of the spinal cord." The whole essay is well worthy of study, and is followed by Dr. Brown-Séquard's article on the "Remoter Consequences of Nerve-Lesions," which appeared in the first edition; this includes the diseases of the nervous centres caused by injury to or disease of a nerve, and includes references to epilepsy, tetanus, catalepsy, chorea, &c., and is especially valuable for its numerous references to published accounts of cases bearing on the question.

An article of six pages only on "Locomotor Ataxy," by Dr. Lockhart Clark, is introduced, with the following quasi-apology, which, however, is quite unneeded: "Although locomotor ataxy comes more frequently under the care of the physician than the surgeon, yet in the early part of its course the symptoms are so equivocal, and so liable to be mistaken for those which belong to certain surgical diseases, that, for the sake of the differential diagnosis alone, a short description of this malady should have a place in every system of surgery."

The diseases of the organs of digestion and respiration are now placed consecutively, an order which was not possible before, commencing with Mr. Coote's article on "Diseases of the Tongue." The late Mr. Alexander Ure's article on "Diseases of the Nose" has been rewritten by Mr. Durham, who has added to it considerably, and has especially called attention to modern methods of research and treatment. Among these may be especially mentioned the treatment of ozæna by washing out the nostrils with a continuous stream passed from one nostril to the other behind the septum. This is so simple that a patient can be readily taught it, and no syringing is half so effective. It is not generally, we believe, known how completely the posterior nares are under the control of the forefinger of the surgeon, both for examination and treatment, and we could have wished that more attention had been directed to this method, which is only mentioned incidentally.

Mr. James Salter's valuable essay on "Surgical Diseases connected with the Teeth" has been expanded by reference to the class of Odontomes under which Broca has proposed to classify tumours connected with the teeth. Under this heading Mr. Salter now includes (*a*) enamel nodules; (*b*) exostosis; (*c*) hypertrophied dilated fangs; (*d*) dentine excrescence, and (*e*) warty teeth, and these are illustrated by numerous engravings. Mr. Pollock's original article on Disease of the Mouth and Alimentary Canal has been subdivided into two, the first essay treating of "Diseases of the Mouth, Pharynx, and Œsophagus," and the other of "Diseases of the Intestines." Between these are placed Dr. Barclay's article on "Diphtheria and Croup," and Mr. Durham's article on "Diseases of the Larynx." This last, which is a combination of the author's brief article on the "Laryngoscope," and of the late Mr. H. Gray's article on "Diseases of the Larynx," in the first edition, has been considerably added to and improved, and contains a very good account of the various forms of laryngitis, and of the treatment of growths in the vocal passages. Mr. Durham speaks highly of the practice of dividing the thyroid cartilage in order to clear the larynx of extensive growths, especially if of an epitheliomatous character.

Mr. Birkett's elaborate article on "Hernia," Mr. Henry Smith's on "Diseases of the Rectum," Sir Henry Thompson's exhaustive essay on "Diseases of the Urinary Organs," Mr. Poland's article on "Urinary Calculi and Lithotomy," and Mr. Charles Hawkins on "Lithotrity," complete the fourth volume. These essays appear to have required but little emendation at their respective authors' hands, and all have the advantage of additional illustrations.

The Diseases of the Generative System are grouped together in the fifth volume, beginning with Mr. Jonathan Hutchinson's article on "Surgical Diseases of Women." The author thus sums up his matured views on the treatment of the pedicle in ovariotomy.

"On the whole, I think that the operator will do well to determine to use the clamp in all cases in which the peduncle is long enough; in cases in which it is too short it will be better when practicable to employ ligatures, and to let the peduncle sink into the wound so that the seat of ligature is level with the peritoneum; and lastly, in cases of extreme shortness he should use the actual cautery. In spite of recorded successes I cannot but feel a strong prejudice against cutting ligatures and peduncles off short and dropping the stump into the abdomen. This incurs the risk both of hæmorrhage and peritonitis."

We may remark in passing that this is not the experience of those who have most frequently practised the method. Dr. Humphrey's valuable article on "Diseases of the Male Organs of Generation" is reproduced with a few illustrations, and is followed by the essay on "Gonorrhœa," for which Mr. H. Lee is now alone responsible. Mr. Birkett appears to have found little alteration required in his essay on "Diseases of the

Breast," though his views on the pathology of that organ have undergone some modification since the publication of his original work on the subject. Mr. Nayler has replaced the late Dr. Hillier as Sir William Jenner's colleague in the article on "General or Constitutional Affections of the Skin," and expresses an opinion—

"That the microscopic characters hitherto noted of the various parasitic fungi, while amply sufficient to enable the one plant to be distinguished from the other, are not sufficient to prove their essential non-identity, *i. e.* to prove that they are not different stages of development of the same fungus."

Mr. Thomas Smith describes the "Local or Surgical Affections of the Skin," and again contributes the article on "Minor Surgery," which is followed by that on "Plastic Surgery," by Mr. Holmes Coote, in which numerous illustrations are introduced. Mr. Lister has revised his articles on "Anæsthetics" and "Amputation." In the former he simply upholds the views formerly expressed as to the simplicity required in the administration, and the danger resulting from the falling back of the tongue. The production of anæsthesia by sulphuric ether, nitrous-oxide gas, and cold is also briefly noticed. In the latter, Mr. Lister devotes, as might be anticipated, a considerable space to the antiseptic treatment of wounds. This is not the place to express any opinion on the method, which is still *sub judice*, but we understand that since the article was published the hitherto all-important carbolic acid has been superseded in Edinburgh itself by a more simple and less offensive material. Mr. Holmes's article on "Excision of Bones and Joints" has been very considerably expanded and improved. The author has had a very considerable experience upon the subject, particularly in young persons, and has illustrated his paper with some drawings showing the amount of movement following excision of the larger joints, in a manner not hitherto attempted.

The Appendix contains as before the articles on "Surgical Diseases of Childhood," by Messrs. Holmes, Brodhurst, and Shaw; on "Apnœa," by Dr. Harley; on "Parasitic and Venomous Insects," by Mr. Busk; on "Surgical Diagnosis and Regional Surgery," by Mr. Holmes; on "Hospitals," by Sir Ranald Martin, and on "Surgical Instruments and Apparatus," by Mr. Holmes Coote. This last essay is illustrated from the catalogues of some of the principal metropolitan instrument-makers, but does not give a very satisfactory account of the *armamentarium chirurgicum*.

We have thus endeavoured to sketch the alterations in the second edition of the 'System of Surgery,' which, take it for all in all, is by far the best compendium of British surgery extant. The expansion to five instead of four volumes has the demerit of enhancing the price, but the advantage of allowing more scope to the authors, and of doing away with a quantity of small type. The addition of illustrations (which now number 472) is a great advantage to many of the essays, and renders them much more complete. The indefatigable editor is to be complimented on the regularity of the issue of his volumes, and we only wish he could exercise a little more despotic influence over some of his staff so as to bring all of them up to the same standard of excellence. Most of the articles are good, some are excellent, and a very few inferior. If authors have neither power nor time to bring their contributions fairly up to the level of the knowledge of the day, we think that in justice to their *collaborateurs* they ought to be treated as defunct authors, and their labours entrusted to other hands.

VII.—Hospitals and their Critics.[1]

THAT the world increases in wisdom with its increasing years may or may not be true; at times those who uphold the sentiment appear to have reason on their side, but at others, alas, its opponents may claim to have the advantage. If reformers succeed in carrying their point, it by no means always follows that they have been right, but, the change having come, it behoves all to make the best of it; and hence very often a change, though not in itself the cause, becomes the opportunity for improvement. When the artist thankfully acknowledged the practised eye of the shoemaker he by no means justified the subsequent criticism of the man of the last.

Without denying the benefit that has already taken place through agitation on the subject of which we treat, we depre-

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[1] *Some Propositions on Hospitalism.* By the late Sir J. Y. SIMPSON, Bart. 'Lancet,' 1870.

2. *A System of Surgery, Theoretical and Practical, in Treatises by various authors.* Edited by T. HOLMES, M.A. Cantab., Surgeon and Lecturer at St. George's Hospital. Second edition, in five volumes. London, 1870-71.

3. *St. George's Hospital Reports.* Vol. v.

4. *Mortality of Childbed and Maternity Hospitals.* By J. M. DUNCAN, A.M., M.D., &c. Edinburgh, 1870.

5. *Introductory Notes on Lying-in Institutions, &c.* By FLORENCE NIGHTINGALE. London, 1871.

6. *The Medical Times and Gazette*, 1871, pp. 787, and *The British Medical Journal*, 1871, pp. 736, containing a notice of the 37th Annual Report of the Glasgow Maternity Hospital.

7. *Report of the Rotunda Lying-in Hospital.* Read before the Obstetrical Society of Ireland, January, 1872.

cate many of the changes contemplated by those who have striven for sweeping alterations, assured, partly by actual experience and partly by reasoning, that injury, not benefit, would arise by carrying out the same.

The late Sir James Simpson starts in the paper first in our list—which we believe was the last he wrote on the subject—with a general assertion of the insalubrity of the older hospitals now existing. Mr. Tait very fairly says that this assertion has been disputed by some most competent authorities, and whilst he is prepared to admit some measure of evil and need of change, he doubts the extent to be as great as indicated by Sir James Simpson. The proposal of this distinguished writer to revolutionise the old hospitals by making the staircases, corridors, &c., all open, is evidently not suited to the British climate. The same objection also applies to the proposition that all the doors and entrances of the wards opening on these staircases and corridors should be built up, and that an entrance into each ward should be made from without. The plan here proposed is, indeed, not new, having been pretty largely tried in the model houses in London for many years past, and been found objectionable and unpopular. The same principle that induces our kinsmen in Canada and the inhabitants of Northern Europe to fit their dwellings with double windows, prompts us to secure ventilation in winter free from the frequent dampness and coldness of the outer air; and this end is gained by the modern staircase, which, when in itself well lighted and ventilated, becomes a valuable assistance in ventilation at all times, especially when the severity of the weather allows but little direct ventilation by means of the windows. When an outside staircase is adopted the doors and windows of a ward are alone available for the purposes of ventilation, and experience has repeatedly shown the disadvantage thus arising, as just adverted to, by prolonged exposure to the inclemency of the weather incurred by those using them. If the system of outside stairs were adopted, pollution of the air of the wards would nevertheless follow, as exemplified by the history of the so-called model lodging houses; for practically people will exclude cold and damp, which would mean, with the arrangement in question, exclusion also of ventilation. If the atmosphere of the wards, staircases, corridors and galleries is allowed to become polluted it is the fault of the management of the institution; for, unless in extraordinarily exceptional cases, such default cannot be laid to the construction.

At the same time, galleries and corridors are in our opinion objectionable, and the system of construction that we think should always be pursued is that of pavilion hospitals; by these we mean buildings of two or three stories, with a central staircase of ample size well lighted and ventilated by large windows, the basement story to be rather above the level of the ground, or if necessary, as in damp localities, elevated to some height above it; the wards to extend from the staircase in two opposite directions, with ample windows placed opposite to each other, with abundant facilities for opening from above as well as from below upwards; each pavilion or building to contain not more than from 80 to 100 or 120 patients, and so placed as to be completely open to the wind and to the sun's power on all sides (we are speaking of the British climate, some modification by the shelter of verandahs, &c., might be admissible in a tropical country).

The late Sir J. Simpson spoke of corridors and galleries, &c., as if normally *retaining and imprisoning the internal befouled hospital air.* We do not know any establishment that does not attempt at all events by ventilation and cleanliness to obviate any tendency to such an atmosphere. Reformers in other days have tried to increase the utility of certain hospitals by adding to their accommodation, forgetful of the original design and arrangements of the buildings. We know cases of the kind where material impediments have so been thrown in the way of those who would otherwise have had comparatively little difficulty in maintaining a salubrious atmosphere in a well designed and well built edifice. The old, many-storied, palatial Continental hospitals, which are regarded with so much disfavour by some, are thus spoken of by Mr. Ewart in his valuable remarks on the German field hospitals in the late war:[1] "The smallest French town is endowed with a capacious hospital or Hôtel-Dieu, with barracks, and with large communal schools. The first was a real boon, whilst the latter contained all the elements which were strictly necessary, and could easily be shaped into comfortable ambulances."

In reference to the employment of wooden huts and canvas tents, Professor Gurlt points out the liability of canvas and wood, as well as other materials, to become the recipients of infection in proportion to the number and nature of the cases placed in them, and the length of time they are in use.

Tents, too, are not exempt from defects, and serious ones also; for, besides those to which we have alluded above, may be noted the very imperfect protection they offer against the changes of weather, and, apart from the difficulty, the danger of warming, as instanced in the melancholy accident at Wakefield, where four persons were burned to death in an hospital of this frail tent structure.

[1] 'St. George's Hospital Reports,' vol. v, 1870, p. 366.

As to the value of iron as the material for hospital construction, so much insisted on by Sir J. Simpson, some experience we have had of iron ships in temperate as well as in warmer latitudes, did not so favorably impress us as to enable us to recommend it for general use in hospital construction.

The hobby which some recent writers have so well ridden, that a cottage-hospital must necessarily be healthy, borders on the absurd; for cottages, like all other edifices, have their defects, which in connection with structural qualities are, there is little question, greater in proportion to their accommodation than those of large institutions, and are, in short, so obvious and numerous that it appears quite unnecessary to enlarge upon them.

We would not have it thought that we object to cottage-hospitals when required for a small community; on the contrary, we believe them especially useful, and often capable of saving life which might be endangered by a journey to a more distant hospital. What we object to is, the recommendation that when hospitals are required to accommodate a hundred, or possibly many more cases, cottages should be constructed instead of larger edifices. Every requisite, combined with economy of construction, supervision and attendance, and all sanitary requirements, can be secured by pavilion buildings, such as we have sketched, whereas if the cottage system, for any considerable number of patients, should be carried out it would be at the sacrifice of many advantages.

In 1862 (Miss Nightingale writes) the Committee of the Nightingale Fund, with a view to extend the advantages of their training institution, entered into an arrangement with the authorities of St. John's House, under which wards were fitted up in the new part of King's College Hospital, opening out of the great staircase, and shut up within their own doors, for the reception of midwifery cases. The wards were under the charge of the (then) lady superintendent. Arrangements were made for medical attendance, a skilled midwife was engaged, a certain number of pupil nurses were admitted for training; and hopes were entertained that this new branch of our training school would confer a great benefit on the poor, especially in country districts, where trained midwifery nurses are needed.

Every precaution had apparently been taken to render the midwifery department perfectly safe; and it was not until the school had been upwards of five years in existence that the attention of the Nightingale Committee was called to the fact that death from puerperal diseases had taken place in each of the preceding years.

During the period of nearly six years that the wards were in use the records show that 780 women had been delivered in the institution, and that out of this number twenty-six (exclusive of a poor woman, who, delivered in a cab, had died in the hospital of *post-partum* hæmorrhage) had died—a mortality of 33·3 per 1000. Of the fatal cases seventeen were due to puerperal fever, three to puerperal peritonitis, two to pyæmia, and one to metritis. The wards were closed as soon as possible. We confess we are not surprised at the fatality, when we read at page 41 that the smell from the post-mortem theatre could be distinctly detected in the wards, and find other objectionable points in connection with the position and arrangement of the department.

Miss Nightingale is not disposed to doubt the value of the returns compiled by the Registrar-General, which are very favorable to delivery at the patients' homes.[1] The death-rate of 1 in 120, in the hands of "educated accoucheurs" in private practice, should in Miss Nightingale's opinion be inquired into. On this matter, we may observe that neither are the cases delivered in hospitals nor yet in private practice among the wealthier classes likely to be the most natural and otherwise devoid of danger. But apart from this, we should be heartily rejoiced to feel that we could depend on the statistical returns commonly published, although we would not imply want of confidence in the integrity of their compilers. But statistics generally, alas! are largely open to errors both in collecting and compiling. Of the accuracy of the information given to registrars by the masses of our people, we are sceptical of in no small degree; and in the precision of the certified causes of death we likewise cannot indulge the fullest confidence. An extended experience, in short, with the circumstances attending the collection of statistics relative to the causes of death as well as many other matters shakes our confidence even in the returns published with the brand of authority upon them. Isolated or patchwork returns of small institutions, as well as those furnished by private practitioners who trust to memory, are very untrustworthy.

To return, however, to hospital economies. It is a good and laudable service to draw attention to the miscarriage and the serious results which have in so many instances followed want of prudence in the arrangements of hospitals, and especially of maternities. When we say this much we by no means acquiesce in the reasoning of those who consider the miserable tenements

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[1] Perhaps we are wrong in using the word patient, as the author of 'Notes on Lying-in Institutions' informs her readers that parturition is not a disease at all, but custom must be our excuse.

of the masses of our poor people as the only safe place for lying-in women. Attention has been directed to this matter in Glasgow, where the lying-in hospital proved truly a haven of safety, as is sufficiently shown by the thirty-seventh annual report of the hospital. Of 313 women delivered in that hospital but one died of puerperal fever; whereas of 698 who were delivered at their own homes that disease carried off five. The report adds that for years past complete segregation of cases of puerperal fever, perfect cleanliness, thorough ventilation, fumigation, &c., have been diligently practised with very satisfactory results.

The report of the Dublin Lying-in Hospital for the year ending 30th of October, 1871, shows a considerable mortality; the temporary sojourn of the cases in hospital being characterised by the same disease which proved destructive to the population that furnished the lying-in women. The season was characterised by an unusual prevalence of typhoid fever, of scarlatina, of smallpox and of erysipelas, whilst bronchitic pneumonia and other evidences of a depressed condition of the vital powers marked the period in the Irish metropolis as elsewhere.

Eleven hundred and sixty-one cases were delivered in the Rotunda in the year, and of these 33 died. Of the fatal cases, 13 acknowledged that they were unmarried. Four others were in mental distress; in one case the woman had been deserted by her husband, in another the husband was in America, and in the other two the husbands were separated from them. In all such cases it has been found that the mental condition predisposes most seriously to fatal diseaes. The depressing influence of the season was forcibly demonstrated by the fact that the fatal result was attributed by Dr. Johnston to sloughing of the vagina with hectic in two cases, in other two cases to sloughing with peritonitis, and in yet two other cases to sloughing with pyæmia. One of the cases fatal from sloughing with peritonitis, was that of an unmarried woman, who, although but twenty years of age, had daily drunk from a pint to a quart of whiskey for the seven months preceding her confinement. Our space and our object do not permit us going into details of each fatal case, nor into the history of cases that recovered from accidents, operations, or illness; for such our readers must refer to the report itself, which will amply repay perusal. That the hospital serves as a refuge for the friendless, the destitute, and the desponding, is abundantly seen in the details of the report. In not a few cases the poor women had suffered the greatest privations, some were but partially recovered from fever, others were actually suffering from illness, diarrhœa, dysentery, bronchitis, phthisis, &c., and others again from the results of previous beatings. To this category we must add a death from being run over, one from premature delivery induced by smallpox, one from anæmia on admission—placenta prævia—and one of sudden death forty-one hours after use of forceps.

To compare the mortality among patients, such as those just detailed, with that obtaining among most of the higher or middle classes, and to denounce an institution of such manifest public benefit, because of the necessarily high rate of fatality attending the cases treated in it, is to say the least unjust. Well may Dr. Guy say that the great leading cause which determines the mortality of hospitals is the *selection of patients*. Others have also justly remarked that a low death-rate too often denotes but hospital inefficiency. We will give Dr. Johnston's own words in reference to the condition of cases when received into the hospital—

"There were five cases of what I may call malpraxis, viz. in two instances the forceps had been tried several times ineffectually before the women had been brought into hospital; two were under the care of women for twenty-four and thirty-three hours respectively. In the fifth case we were not able to ascertain the length of time, but the arm was protruding through the vulva, and the waters had escaped eight hours previously. In seven cases the child was born for some hours before admission, and, therefore, the mothers were exposed to all the dangers of hæmorrhage, inflammation, &c. Forty-seven were in very delicate health, suffering from great debility, either from severe sickness of stomach during pregnancy, recovering from fever, labouring under anasarca, extreme emaciation, fretting with great anxiety of mind from seduction, ill treatment, deserted by or death of husband, &c.; 8 cases of exhaustion from accidental hæmorrhage, 1 from unavoidable hæmorrhage, 36 from bronchitis, 2 from pleuritis, 3 from phthisis, 1 pneumonia, 2 from laryngitis, 1 tonsillitis, admitted from another hospital; 3 inflamed varicose veins, 1 erysipelas of the head, 3 injuries by beating, 1 influenza, 1 hæmoptysis with peritonitis, 1 jaundice, 1 epilepsy, 3 condylomata, 1 secondary eruption, 3 purulent vaginitis, 1 variola, 1 hysteric convulsions with hæmorrhage (three months), 1 typhus."

The report mentions several other instances of illness some of which were fatal. In three it appears death was sudden; they are thus enumerated:

"1 of clot in the left ventricle, 1 of rupture of a varicocele in left spermatic vein, 1 of embolism."

It appears cases of the zymotic class did not spread in a single instance in the hospital.

We cannot but concede to the master of the Rotunda the privilege to lay before the public the unavoidable liability to fatal results in an institution which is open to the admission of "all seeking admission, even the most abandoned," who "may freely enter without any previous examination or inquiry into their circumstances, the only recommendation being that they stand in need of assistance," &c. We must bear in mind, too, that the institution founded by the energy, ability, and self-denial of Bartholomew Moss, cannot refuse any application even though coming from infected houses and belonging to a family suffering with contagious diseases. Thus one poor woman had it appears lost four of her children by scarlatina before her admission to the Rotunda.

Among the military hospitals a mortality of 10·1 per 1000 occurred at Aldershot. At Chatham there were three deaths in 342 deliveries, and at Woolwich eight among 751 deliveries, and five of these from puerperal diseases. It would, of course, be idle to compare the healthy and comparatively well-to-do wives of soldiers with poor and outcast women of a city population, such as those admitted into the Rotunda hospital. Nevertheless, this course has been pursued by some who have busied themselves writing against such institutions.

It is amusing to find the author of 'Notes on Nursing' gravely discussing the suitability of this and that military hospital for lying-in purposes, and of the admission of medical students to lying-in hospital practice, at the same time that she proposes to hand over the whole responsibility of midwifery to practised females, who are to be medical students too, and if so must dissect, as so many of the fair sex now insist on doing: an interpretation of women's rights we are by no means inclined to accept.

We should be rejoiced if the rule laid down by Miss Nightingale could be carried out, namely, that parturient women *should be perfectly well in health.* Indeed, as we have before remarked, this lady goes further. She writes, "Since lying-in is not an illness, and lying-in cases are not *sick* cases, it would be well, as already said, to get rid of the word 'hospital' altogether," &c.; and further on, "on the contrary, a death in childbed is almost a subject for an inquest." But really this is too much; we must leave our very clever authoress with *Socrates' Mother's Shade,* to whom she has dedicated her book.

Individuals with every mental agency of a depressing nature at work, and affected, but too often, with diseases of various kinds are received in the established maternities of Vienna, Dublin, and other cities, and if we look for further explanation of a high death rate in such establishments it may be found in the very large proportion of primiparæ. Thus, at the great Dublin hospital there were 403 such out of the 1161 interne labour cases during the past year. Now, Dr. M. Duncan has clearly shown that the mortality of first labours is about twice that of all subsequent labours taken together, and further that the mortality from puerperal fever following first labours is about twice the mortality from this cause following all subsequent labours taken together. In reference to this point the works of Collins, Johnston and Sinclair, Hardy and M'Clintock, and the registers of Edinburgh and Glasgow have been had recourse to by Dr. Duncan, and we may give full credence to his conclusions.

To bring forward the mortality of the Dublin Rotunda Hospital during the fearful period of the famine-fever, and the cholera years 1847—54—(for no less than two visitations of cholera as well as the sad period of the Irish famine and fever were included in those years)—is manifestly unfair, yet we find Le Fort cites those years as specimens of the mortality of the Dublin hospital, and Miss Nightingale adopts his figures, classing them for these years with the returns for certain periods in some Continental hospitals. Looking to the period of Dr. Collins's mastership of the Dublin Hospital, the mortality in a total of 16,414 women delivered in seven years, be it observed, was 164, or in the proportion of 1 in 100. Again, of 6634 women delivered, as recorded by Drs. M'Clintock and Hardy, 65 died, or 1 in 102; and in the seven years recorded by Drs. Sinclair and Johnston, 13,748 were delivered and 163 died, or 1 in 84.

Prussia conducts her records probably as she does her other public affairs, with a full regard to realities, yet she records a mortality of 1 in 108 deliveries out of a total of 7,654,021.

Dr. Duncan followed up Dr. M'Clintock's inquiries as to the deaths omitted in the registration returns of deaths of childbed with the result,—among the married women alone, of showing the deaths to be 153 instead of 118 in Edinburgh and Glasgow in 1855. Now, we conceive Miss Nightingale, and those who think with her, can take their choice of the comparatively small figures in relation to this matter in the case of the joint numbers in Edinburgh and Glasgow and the large numbers seen in the grand total of Prussian returns, in each case strangely at variance with the position attempted to be taken by those of the anti-maternity school, if we may be allowed to coin the expression.

The question of so-called hospitalism has very much hinged, in all past discussions, both oral and written, upon the experience gained in those hospitals used for the relief of lying-in women and therefore we have in the present paper confined our remarks almost, if not entirely, to those establishments, nor does our space permit us now to enter into the consideration of institutions intended for other classes of cases.

Dr. Johnston in his recent address answers the question, as to the practicability of maintaining large maternities as safe asylums, in the affirmative, and he shows that, as we have before noticed, individuals were received not only from the midst of their families, other members of which were actually suffering from typhus, typhoid, erysipelas, scarlatina, or smallpox, as the case may have been, and in many instances the parturient was herself suffering from one or other such disease, yet in no one instance did the malady extend beyond the individual so affected.

He does not presume to maintain that hospitals can be kept inviolate from epidemics any more than the mansions of the rich or the hovels of the poor, but he contends that it is within the bounds of possibility to render even large hospitals as safe as private dwellings, and certainly more so than the hovels of the poor.

What have been the means pursued in the Rotunda for the purpose? The report enumerates the following; namely,—perfect purity of the atmosphere, secured by not overcrowding patients together, by allowing ample cubic space for each individual, with thorough ventilation; strict cleanliness; not allowing labour to run on too long, *i.e.*, to the verge of inflammation, and giving a generous but judicious diet from the commencement according to the circumstances of the case.

To complete the notice of the report of a large maternity it is perhaps right to give an idea of the obstetric features of the cases treated in the Dublin Lying-in Hospital, or, as it is commonly called, the "Rotunda." Of the 1161 cases 403 were primiparæ and 758 pluriparæ; of the foregoing 19 were tedious, but did not require artificial aid, the delay being in the first stage; 24 were abortions; 56 were preternatural cases, 8 of which presented with the shoulder, elbow, or hand; 4 were breech or lower extremity presentations; 64 were forceps cases. In 2 cases the crotchet had to be used, and in 4 the cephalotribe; of these there was distinct evidence that the child was dead in 5, and this point was doubtful in one instance. In 39 cases the labour was complicated, viz., 16 had twins, 8 were cases of accidental hæmorrhage, 1 of placenta prævia, 5 of post-partum hæmorrhage; in 9 cases the placenta was morbidly adherent; in 8 there was prolapse of the funis, in 1 rupture of the uterus, in 2 convulsions, and in 1 epilepsy; mania occurred in 12 instances. In 55 chloroform was used.

Of the diseases remarked there were 25 cases of peritonitis, 1 of puerperal fever, 1 of metritis, 5 of phlebitis, 4 of pyæmia, 4 of sloughing of vagina and perinæum, 1 of gangrene of the uterus, 5 of scarlatina, 1 of variola, 3 of erysipelas, 2 of typhoid and 1 of typhus fever; and, lastly, 36 of bronchitis, and one of pneumonia.

We have in the foregoing remarks and quotations devoted some space especially to enable our readers the better to comprehend the differences obtaining between cases that seek the aid and shelter of a public maternity hospital, and the average run of cases met with in healthy rural districts, and in the well-found and well-circumstanced military hospitals; and, in conclusion, we would suggest to book makers and critics the necessity of comparing like with like.

VIII.—Watson's Practice of Physic.[1]

SINCE the first public announcement that Sir Thomas Watson was engaged in preparing a new edition of his lectures, the question has often been asked, and more than once we have heard it discussed, whether he did well to undertake a task so difficult and laborious as that of attempting to incorporate into his celebrated work the available results of medical research during the fourteen years which have elapsed since the publication of the last edition. The question was raised by those who held Sir Thomas Watson in the highest respect and esteem, and who naturally feared lest his reputation as an exact, trustworthy, and eloquent teacher should suffer through any faulty performance of the difficult task which he had undertaken. The work has now been for some months in the hands of the profession; it has been examined and criticised publicly and privately, and the result is a general concurrence of opinion that while, as in every human production, it is not difficult to point to faults and omissions, it is yet easier and far more agreeable to indicate in nearly every lecture of the series the results of a most careful and conscientious revision, and these, too, expounded by a pen which has evidently lost none of its marvellous skill. The traces of retouching by the hand of a master are evident at the very commencement of the work. The first part of the well-known introductory lecture, with its eloquent and touching peroration, has been rewritten. In this newly written part the student is earnestly warned not to lose or neglect the precious, but short and fleeting opportunity which is afforded him by his hospital attendance. Reference is then made to the study of anatomy and physiology, in a passage which we cannot resist the temptation to quote:

"Do not think that I am wandering from my proper subject when I bid you remember how profoundly interesting, how almost awful is this study in itself, and for its own sake, revealing, as it surely

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[1] *Lectures on the Principles and Practice of Physic.* By Sir THOMAS WATSON, Bart., M.D., F.R.S. Fifth edition. London, 1871.

does, the inimitable workmanship of a hand that is divine. Do not lose or disregard that grand and astonishing lesson. Do not listen to those who may tell you not to look for the evidence of purpose in this field of study; that the visible mechanism of that intricate, but marvellously perfect and harmonious work, the animal body—the numberless examples it contains of means suited to ends, of fitness for a use, of even prospective arrangements to meet future needs, of direct provisions for happiness and enjoyment—that all these have no force at all, in true philosophy, as evidences of design. For my own part I declare that I can no more avoid perceiving, with my mental vision, the evidential marks of purpose in the structure of the body than I can help seeing with my own eyes in broad daylight the objects that stand before my face. There are, however, minds, very powerful and cultivated minds, too, that cannot or will not, or at least do not, recognise or acknowledge these teachings of anatomy, but denounce as unscientific and unsound *all* reference to final causes in nature. To me, believing in their honesty, this is intelligible only on the hypothesis suggested by an eminent living philosopher and anatomist, that the minds in question labour under some defect analogous to that which renders certain eyes imperfect and untrustworthy, and which has received the name of *colour-blindness*."

We believe that we shall best succeed in our endeavour to give a fair and accurate statement of the extent and thoroughness of the revision to which these lectures have been subjected by referring first to some of the leading scientific questions which are discussed, and subsequently to the important subject of the treatment of disease.

Very early in the first volume we discover that Sir Thomas Watson keeps himself *au courant* with the progress of physiology. He distinctly recognises the fact that the function of the minute muscular arteries is to regulate the blood-supply in accordance with the requirements of the various tissues and organs; and he applies this modern but now generally accepted physiological doctrine in explanation of a variety of pathological phenomena in a manner which we will now endeavour briefly to explain. It has long been known that, in cases of death from apnœa, the circulation through the lungs is in some way impeded and finally arrested. It has been very generally assumed that the blood is brought to a stand by some influence exerted upon it by the pulmonary capillaries; but Sir Thomas Watson clearly shows that, in cases of acute apnœa, the capillaries are empty of blood, that therefore the blood must have been arrested before it arrived at the eapillaries, and the only conceivable cause of this arrest is the contraction of the muscular walls of the minute arteries under the influence of the vaso-motor nerves.

"It was reserved," he says, "for Dr. George Johnson to suggest, that when the conversion of venous into arterial blood in the pulmonary capillaries is suspended, the stream of dark blood is arrested by what he happily calls the stop-cock action of the ultimate pulmonary arteries." If now we turn to the lecture on acute laryngitis, we find the same physiological principle applied in a very interesting way to explain the state of lung and of pulmonary artery gradually induced by obstruction in the larynx, a state which renders intelligible the occasional inadequacy of tracheotomy to save life. The explanation is, that the obstruction in the larynx limits the supply of air to the lungs; the blood in the pulmonary capillaries is imperfectly aërated, and some partially aërated blood passes on into the systemic arteries. At the same time the minute pulmonary arteries by their contraction lessen the supply of blood to the pulmonary capillaries in proportion to the limited access of air. The blood accumulates therefore in the trunk and larger branches of the pulmonary artery, in the right cavities of the heart and in the systemic veins. The distension of the superficial veins renders the lips and the skin more or less livid; while the retrograde engorgement of the bronchial veins and capillaries, which belong to the systemic venous system, results in a serous effusion into the bronchial tubes. This serous exudation gravitates towards the bases of the lungs, filling the air-cells and smaller bronchi, and thus still further impeding respiration. Meanwhile the slowly moving, partially stagnating blood in the pulmonary artery becomes more and more viscid, and at length partially coagulates. Hence, on post-mortem examination, fibrinous coagula, which had evidently been in process of formation for several hours before death, are often found in the pulmonary artery. Secondary causes of apnœa are thus established, which do not cease when the primary cause is removed by the operation of tracheotomy. Obviously, then, the practical lesson is, in suitable cases, to resort to that operation before this unfavorable state of lung and of pulmonary artery has been induced by long-continued partial apnœa.

The next disease upon whose pathology the recognition of the true function of the minute muscular arteries has shed a new light is epilepsy. Sir Thomas refers to the opinion which formerly prevailed, that an overfull condition of the cerebral vessels is the proximate cause of that terrible malady; and he points out that the phenomena of the disease are inconsistent with this theory. He then goes on to state that the very opposite condition of the cerebral circulation, extreme emptiness of the blood-vessels of the brain, is now generally believed to determine the fit. Clearly, as he says, the cause of the fit must be some condition which may be both sudden and transitory; and such a condition an abrupt and brief privation of arterial blood would furnish. Reference is then made to the experiments of Kussmaul and Tenner, who in numberless instances threw rabbits into epileptiform convulsions by ligaturing their carotids and subclavians, and thus suddenly arresting the circulation through the brain. Again, it is shown that convulsions frequently result from a profuse and rapid hæmorrhage. If, then, as appears probable from a consideration of these facts, sudden anæmia of the brain be the immediate cause of an epileptic paroxysm, a rapid and energetic contraction of the minute cerebral arteries affords a sufficient explanation of the abrupt arrest of the circulation through the brain; and this theory of the proximate pathology of epilepsy is now generally accepted.

Then, turning to a very different but a scarcely less terrible disease, the impeded circulation through the lungs, now believed to be the essential cause of the collapse stage of cholera, is, for reasons which we shall presently state at some length, ascribed to the contraction of the identical minute pulmonary arteries, which, as we have seen, are the immediate cause of the arrested circulation in cases of death from apnœa.

The arterial contraction which is now admitted to form so important an element in the pathology of the diseases hitherto referred to is of a more or less transient character—sudden and paroxysmal in the cerebral vessels of an epileptic patient, more persistent in the pulmonary vessels during apnœa and cholera collapse. There is, however, one class of cases in which this arterial contraction is so vigorous and so continuous as to result in hypertrophy of the muscular walls of the arteries concerned.

Sir Thomas Watson, in his lecture on Bright's disease, refers to the fact which was first made known by Dr. Bright, that in a large proportion of cases of chronic Bright's disease, the left ventricle of the heart is found hypertrophied, even when there is no disease of the valves or of the large arteries to explain the hypertrophy. Sir Thomas goes on to say,

"Dr. Bright suggested that the altered qualities of the blood might so affect the minute and capillary circulation as to render greater action necessary to force the blood through the distant subdivisions of the vascular system. But to Dr. Johnson must be given the merit of having discovered the true and full cause of this hypertrophy of the left ventricle, in the fact that the muscular walls of the small arteries, not only in the kidneys, but also in most or all of the tissues of the body, in these cases of chronic Bright's disease are greatly hypertrophied. The fact itself is demonstrable, and beyond question. This excessive growth of muscular tissue implies long continued over-action; and since the tonic contraction of the small arteries is known to oppose the passage of blood, the hypertrophy of the left ventricle is presumably due to the excessive resistance offered to the circulation, by the excessive contraction of the minute arteries, this excessive contraction being the consequence of the irritant action of the deteriorated blood."

Once more, in the lecture on apoplexy, Sir Thomas Watson applies this doctrine in explanation of some cases of cerebral hæmorrhage occurring, as it often does, in association with chronic Bright's disease:

"The arterial stop-cocks resist the passage of the unpurified blood into the capillaries. The strong left ventricle strives to force on the blood. The resulting distension of the systemic arteries is indicated by the full and hard radial pulse, and by the existence of increased arterial tension afforded by the sphygmograph. There is thus excessive pressure on the whole of the arterial pipes between the stop-cocks and the forcing-pumps; and in the struggle between the two contending forces a minute artery in the brain may be broken, and so cerebral hæmorrhage may occur."

With reference to hypertrophy of the muscular walls of the minute arteries—a tissue change which may be taken as a true index of self-registered continuous over action of the arterial stop-cocks—we venture to suggest that if this interesting discovery, instead of being of home production, had been imported from the Continent, it would have been more heartily welcomed and more loudly heralded than it has hitherto been. It has always appeared to us that amongst the most striking characteristics of Sir Thomas Watson have been his aptitude to learn and to unlearn, his readiness in availing himself of the results of recent research, and his generous appreciation of other men's labours. It appears to us that he has done good service to the cause of rational and scientific medicine by so interweaving into the texture of his discourses these modern discoveries and doctrines relating to the circulation, as to show in the clearest and most convincing manner not only their high scientific interest, but also that the light which they throw upon some very intricate morbid phenomena affords material and valuable aid in the treatment of disease.

Plugging of the vessels by embolism and by thrombosis is referred to and discussed in connection with the various phenomena to which it gives rise. Thus, in the interesting lecture on the different modes of dying, allusion is made to the not uncommon case of a clot of fibrine either brought from a distant vein, or formed within the right chambers of the heart, getting into the current of blood, and plugging the pulmonary artery. If the obstruction be complete death may be instantaneous; if the vessel be not perfectly sealed, extreme dyspnœa, with pallor and faintness, comes on at once, and the patient dies within a period ranging from a few minutes to several hours. Then attention is directed to the fact that an impediment to the supply of blood to the lungs through the pulmonary artery causes dyspnœa as urgently as an impediment to the supply of air to the lungs through the air-passages; and it is shown that physiologically this is explicable by the want, common in both cases, of aërated blood by the system, expressing itself in the chemical cry from the famishing tissues for the indispensable oxygen. What is peculiar to this mode of dying is that the circulation is stopped, yet not by asthenia, and the function of respiration is suspended, yet not by apnœa. Death does really in this case begin in the lungs, and the mode of dying may with literal accuracy be called death by *pulmonary asphyxia*, or pulselessness in the pulmonary artery.

Embolism and thrombosis are further discussed in the lectures on apoplexy, and are there shown to be a not infrequent cause of hemiplegia and white softening. Again, in the lecture on chorea, which has been in great part rewritten, reference is made to the frequent association of chorea with endocarditis, and the views of Dr. Kirkes, Dr. Hughlings Jackson, Dr. Tuckwell, and others, are discussed. It is pointed out that the one-sided character of the disease has an important bearing on its pathology, for it shows at least this, that the same region of the brain is concerned, directly or indirectly, in that disease, in hemiplegia, and in epileptic hemispasm; the region, namely, which includes the corpus striatum, the optic thalamus, and the structures adjacent. Occasionally hemichorea runs into hemiplegia, and conversely hemiplegia is occasionally accompanied by choreiform movements, and it even *improves* sometimes into chorea, paralysis being succeeded by a return of motor power, but of an incomplete and disorderly character. Again, the same muscles are affected in hemispasm as in hemichorea, and if, as has been appositely said, chorea be "next door to palsy," it is as near a neighbour to epilepsy also, and not unfrequently traceable to similar exciting causes.

Sir Thomas Watson accepts the conclusion that chorea "is often, not always, the ultimate result of the injection of molecular particles of fibrine into some of the minutest arteries or capillaries of the nervous tissue;" and he alludes to the probability that the chorea which results from fright, or other emotional disturbance, may be due to a temporary interruption of the circulation through a limited portion of the brain, caused by spasm of the minute cerebral arteries; so that "molecular embolism and spasm of the minute arteries would have, common to them both, the effect of depriving a portion of the brain mass of its due supply of nutritive blood."

Much care and labour have evidently been bestowed upon the revision of the lectures on diseases of the nervous system. Amongst the subjects which have attracted especial attention since the publication of the last edition of these lectures one of the most interesting and intricate is that of *aphasia;* and we would refer to Sir Thomas Watson's treatment of this subject as an admirable example of methodical and lucid statement. The facts are set forth in orderly array, the clinical history of the affection is given with remarkable completeness, and the various speculations to which it has given rise receive due consideration, reference being made to the observations of Drs. Bateman, Bastian, Broadbent, Hughlings Jackson, Maudsley, Moxon, Ogle, Sanders, Wilks, &c. Two diseases are here introduced for the first time: these are locomotor ataxy and progressive muscular atrophy. We can understand that some readers would have been better pleased to find a longer and more elaborate history of these formidable maladies, but the narrative as we have it is a model of clear and condensed statement.

In the discussion of the subject of *Croup* we find much that is new and interesting. It is shown that in this country the term has been applied to three distinct forms of disease: thus we have, 1, *Spasmodic Croup,* or laryngismus stridulus, a purely nervous affection, unattended by structural change of any kind in the larynx; 2, *Inflammatory Croup,* or infantile laryngitis, a catarrhal affection of the larynx, not contagious, and not resulting in the exudation of false membrane within the larynx; 3, *Diphtheritic Croup,* or membranous laryngitis, the exudation of false membrane within the larynx being a result of the specific general and contagious disorder diphtheria.

It has long been the custom with French writers to limit the term *croup* to this last class of cases, namely, those in which a diphtheritic exudation extends into the larynx; with them croup forms a part of the history of diphtheria. The name is of little importance so long as we have a definite notion of what it is intended to designate, but Sir Thomas Watson shows that ever since the publication of Dr. Home's pamphlet on "Croup," in 1765, English writers on this subject have confounded under this one name two totally distinct diseases. In describing croup they give the history, the symptoms, and the treatment of catarrhal laryngitis, but with this they combine the morbid anatomy of diphtheria. After a careful consideration of the subject our author arrives at the conclusion that croup accompanied by false membranes in the larynx is always diphtheritic, whether in the child or in the adult, and that simple laryngitis or inflammatory croup is never associated with the exudation of false membrane. We are grateful to Sir Thomas Watson for having placed this important matter in so clear a light. Etymologically we believe that the word croup signifies a noise in the throat such as is made by crows or frogs; the term might, therefore, with equal propriety be applied to all the three forms of disease, the spasmodic, the inflammatory, and the diphtheritic. It might, perhaps, be better to use it as a generic term, with a prefix designating the specific form of croup referred to. It is obviously of vital importance that two diseases so distinct in their pathological history as diphtheria and catarrhal laryngitis should not be confounded under one name. One result of this confusion in past times has been that in numberless instances children suffering from inflammatory croup have been injuriously medicated by mercury, with a view to prevent a dreaded exudation of lymph within the larynx; yet an exact knowledge of the nature of the disease would have shown that no such exudation ever occurs as a result of catarrhal laryngitis; while, on the other hand, if the case were one of laryngeal diphtheria mercury is powerless to prevent the exudation. The obvious inference is that mercury, except occasionally as a purgative, is not an appropriate remedy for any form of croup.

In discussing the subject of tubercle and phthisis the results of all modern researches and speculations are passed in review. Reference is made to the experiments of Villemin, of Simon, Sanderson, and Wilson Fox; and accepting Dr. Andrew Clark's definition of phthisis pulmonalis as "comprehending all progressive consolidations and circumscribed suppurative degenerations of the lung," the disease is shown to be one of many forms and aspects. The chief of these are discriminated and described.

The lecture which in the present edition has undergone the greatest amount of change—a change so great as to constitute a complete reversal of its author's former doctrines—is that on cholera. We observe that some critics have expressed regret that Sir Thomas Watson should have unreservedly accepted Dr. George Johnson's views as to the nature and treatment of cholera, yet they make no attempt to point out wherein consists the error of the teaching which they profess to dislike.

No one who reads Sir Thomas Watson's rewritten account of the pathology and treatment of cholera can fail to see that he has thoroughly reconsidered and studied the whole subject, and that the arguments which he adduces in opposition to the older doctrines and in favour of the new are such as it would be very difficult to refute or to gainsay. He shows that the theory of the collapse of cholera being a result of the drain of liquid through the alimentary canal is inconsistent with the fact, that in the worst cases there is rather an inverse than a direct relation between collapse and discharges; that the symptoms of collapse are not such as exhausting discharges would give rise to; that they are not beneficially influenced by the remedies—alcoholic stimulants, for instance—which have a telling effect in cases of mere exhaustion; that the state of collapse passes off while the discharges continue, and that recovery from collapse is often so rapid as to be inconsistent with the theory of exhaustion by profuse discharges. Then, on the other hand, it is shown that all the facts of the disease are consistent with the theory that the essential cause of choleraic collapse is an impeded flow of blood through the lungs, and consequently through the whole system; the immediate cause of this impediment being the contraction of the minute pulmonary arteries upon the poisoned blood.

The morbid anatomy of the disease is in entire harmony with this theory. The left side of the heart being empty or nearly so, while the right cavities, the large systemic veins, and the pulmonary artery to its minutest ramifications are distended with black blood. The extreme anæmia of the pulmonary capillaries is explained by the fact that the stop-cock action of the minute arteries has arrested the blood before it could reach the capillaries. This abrupt stoppage of the blood explains on the one hand the emptiness of the systemic arteries, and on the other the fulness of the systemic veins and the consequent lividity of the surface. The scantiness of the oxygen-bearing blood-stream in the arteries explains the lowering of the temperature, and the suppression of those secretions, bile and urine in particular, which are products of oxidation; while it is interesting to observe that when a nursing mother is passing through the stage of collapse, the mammary secretion continues apparently unchecked, and the breasts become painfully distended. The most reasonable explanation of this being the fact that the milk constituents, curd, sugar, oil, and water, may be obtained from unoxidised blood.

The discharges from the stomach and bowels are looked upon as the means by which the morbid poison and its products are eliminated. It is a notorious fact, and confirmatory of this view, that recovery from collapse is always associated with a continuance of the discharges, while, on the other hand, an abrupt and complete arrest of the discharges during the state of collapse is a sign of fatal import. Surely, then, there is reason to believe that these discharges are an essential part of that process by which a natural cure is effected. If this be so, then, as Sir Thomas Watson says:

"It must be wrong to dam the choleraic poison and its products within the body. Even when those products have in one sense been separated from the system, they may produce highly noxious effects if they remain shut up in the stomach or bowels, there to ferment and decompose. Admitting, as we must, that a minute quantity of the morbid excretions swallowed with water may suffice to produce the disease, a large quantity retained through weakness of the expulsive powers or otherwise can scarcely be harmless. Rather may we expect that its expulsion will tend to liberate the patient from danger and discomfort."

Reference is made to the success of the evacuant or cleansing treatment in all stages of the disease, as shown by the results of a large number of cases treated in the Liverpool parish infirmary, and recorded by Drs. M'Cloy and Robertson, in the 50th volume of the 'Medico-Chirurgical Transactions.' In our endeavour to present a brief sketch of this most important lecture, we have done but scant justice to the subject; we trust, however, that we have said enough to show that no practitioner of medicine can with safety or credit to himself remain in ignorance of the arguments here set forth. We venture also to suggest that, unless the statements can be shown to be erroneous or the reasoning inconclusive, the inevitable practical conclusion will have to be accepted; as we believe it has been by a large and rapidly increasing number of the profession, and we may add, too, of the intelligent and reading portion of the public.

In the lecture on mechanical occlusion of the intestinal tube Sir Thomas Watson refers to and accepts as probably true the late Dr. Brinton's theory of fæcal vomiting.

Dr. Brinton maintained that there is no such thing as a reversed peristaltic action of the intestine. He believed the reflux of the contents of the gut to be caused by the direct and forward propulsive action of the impeded bowel; part of the force being expended against the sides of the bowel, which yield and become stretched, part in producing a backward current along the axis of the bowel. We confess that we find a difficulty in accepting this theory. Sir Thomas admits two facts in contravention of the absoluteness of the rule, namely, the regurgitation of food from the cardiac end of the stomach into the mouth, and of bile from the duodenum into the stomach.

A reversed action of the œsophagus and of the duodenum being proved, there is an *à priori* probability that the same reversal of the peristaltic movement may occur in other parts of the canal; and we see no other way of accounting for the fact that whereas in cases of obstruction at the lower part of the ileum the distended and dilated portion of bowel extends only to a distance of a few yards above the seat of obstruction, a portion of the contents of the distended bowel finds its way backwards to the stomach and is vomited. Now, it seems clear that when the regurgitating contents of the obstructed bowel have passed upwards to the portion of the small intestine which is not filled and distended by liquid, the only way in which it can reach the stomach is by a reversed peristaltic action of the bowel. In short, Dr. Brinton's theory requires that the distension and dilatation of the bowel should be continued up to the pyloric end of the stomach. In fact, however, when fæcal vomiting has occurred, it has often been found after death that many yards of empty and collapsed intestine have intervened between the stomach above and the obstructed and dilated bowel below. We therefore retain our belief that a reversed peristalsis in the bowel is an essential part of the process of fæcal vomiting; and further, it is highly probable that in some instances obstruction of the bowel is due to a disorderly peristaltic action, from which it may result that instead of that regular wave-like movement of the bowel by which the contents are continually driven in an onward direction, one portion of bowel, by a disorderly reversed action, antagonises another. The effect of such a conflict of forces would be a painful and perilous obstruction, and the remedies for it are warm baths, fomentations, and anodynes. These are some of the cases in which drastic purgatives may fatally prolong constipation, while narcotics are the most effective laxatives and aperients.

In the 77th lecture the theory of contagious fevers is discussed at some length, and with the author's characteristic caution and sagacity. Reference is there made to the humoral pathology of Hippocrates and his followers, and Sir Thomas remarks that—

"It is most curious to see that these very doctrines which had sunk into universal discredit and contempt, are now again assuming their places as scientific truths—a wonderful example of the sagacity of the older physicians, of the despised wisdom of our forefathers."

Reference is made to Liebig's theory of fermentation, as illustrating analogically the possible influence of a fever poison upon some of the blood constituents. Attention is also directed to Mr. Simon's hypothesis that some material pre-existing in the blood, but not forming an essential or vital part of it, combines somehow with the exciting virus from without to cause the febrile commotion, and in consequence of the exhaustion of that material, the subsequent immunity of the individual from the same disorder. We are then reminded that Sir James Paget looks at this curious subject in a somewhat different light. The *maintenance of morbid structures* is, he says, so familiar a fact that not only its wonder but its significance seems to be too much overlooked. What we see in scars and thickening of parts appears to be only an example of a very large class of cases; for this exactness by which the formative process in a part maintains the change once produced by disease, offers a reasonable explanation of the fact that certain diseases usually occur only once in the same body. The poison of smallpox or of scarlet fever being, for example, once inserted, soon by multiplication or otherwise affects the whole of the blood; alters its whole composition; the disease in a definite form and order pursues its course, and, finally, the blood recovers to all appearance its former state. Yet it is not as it was, for now the same material, the same variolous poison will not produce the same effect upon it, and the alteration thus made on the blood or the tissues is made once for all; for commonly through all after life the formative process assimilates and never deviates from the altered type, but reproduces materials exactly like those altered by the disease; the new ones, therefore, like the old, are incapable of alteration by the same poison, and the individual is safe from the danger of infection.

The occasional recurrence of the disease is, on this hypothesis, an example of the operation of that law that, after a part has been changed by disease, it *tends* naturally to regain a perfect state; most often the complete return is not effected, but sometimes it is, and the part at length becomes what it would have been if disease had never changed it.

In further illustration of the same subject, Sir Thomas Watson says that, to his own mind, the whole train of events has always seemed analogous rather to a cycle in the progression of vegetable life.

"We have the visible and tangible seed, the manifest sowing, the hidden germination; then, the outgrowth and efflorescence, the ripening, the mature seed-time, the reproduction manifold of the original specific germ—every stage in the process of development occupying a definite period of time. Lastly, for here the analogy, though weaker, does not wholly fail, we have the total or the partial, the final or the temporary exhaustion of the soil, even by a single crop for that particular substance. Sometimes (to continue the metaphor) the soil slowly regains the power to grow the same disorder; we see this in the waning protective influence of distant bygone vaccination."

The same view is suggested in the writings of Dr. William Budd, and Professor Tyndall wrote to Sir Thomas Watson in the following terms:

"A tree or a grain crop requires for its existence an infinitesimal amount of mineral matter, without which, however rich the soil, it cannot grow. It is perfectly conceivable that a soil may contain this matter in such minute quantity that a single crop may exhaust it, and this without prejudice to the capacity of the soil as regards other crops. Now, may there not, prior to the sowing of the virus, be something analogous in the human system, which a single crop of pustules entirely removes? Some such change is certainly wrought, and I would rather express it in terms of matter than in terms of force. If after one attack of smallpox the system ever becomes receptive of a second, this would be equivalent to the restoration of the requisite mineral matter to the soil."

Sir Thomas Watson discusses at considerable length the exciting causes of fevers. Upon this subject he says, there has been and there still is a confusing and pernicious contrariety of opinion amongst medical men, and he remarks that—

"The two great questions about which we have to make up our minds are these—1. Are the three diseases, typhus fever, typhoid fever, and relapsing fever, *contagious* diseases; communicable, I mean, from one who has the disease to one who has it not? 2. If contagious do they ever arise except from contagion? My own conviction respecting every one of the three is, that the first of these questions must be answered in the affirmative, and the second in the negative."

In the advocacy of the doctrine of the essentially specific exciting cause of all these fevers, he sides with Dr. William Budd against Dr. Murchison, who, while admitting the contagious nature of typhus, typhoid, and relapsing fevers, maintains that the first may, in exceptional cases, be generated spontaneously by filth and over-crowding, the second by *fæcal* fermentation, and the third by destitution. Upon this disputed question, after setting forth many facts and arguments in support of his own view, Sir Thomas Watson says:

"Mind, I neither deny nor doubt that filth, foul air, and the gaseous products of animal and vegetable decomposition, are things hurtful to health; or that they are capable, especially when abundant and concentrated, of causing serious disease and even death; what I do doubt and deny is, that of themselves they ever produce a contagious fever. I agree with Dr. Guy—whose interesting report upon the health of nightmen, scavengers, and dustmen, is well worth your perusal in connection with this subject—I agree with him in believing that filth is rather the *nurse* than the *parent* of fever, but I am not persuaded of the correctness of his final conclusion, that 'in extreme cases fever may be bred of filth.' In old countries the *seminium* of each of these diseases is, doubtless, always dormant somewhere, as that of smallpox must be, ready to rouse into wide-spread mischief upon the first return of the mysterious influences which awaken or renew its epidemic power."

This question of the possible spontaneous origin of some cases of contagious fevers merges into that of the spontaneous generation of some of the lower forms of organic life. Upon this wide and difficult subject we have neither space nor inclination now to enter.

We have hitherto referred only incidentally to the *treatment* of disease; in what remains of our space we shall limit our remarks chiefly to this important subject. Those who are familiar with the former editions of these lectures will remember that, although in each successive edition the author had endeavoured, with more or less complete success, to keep pace with the progress of pathology, he had made but little change in his therapeutics. The directions given for the treatment of disease in the fourth edition, issued fourteen years ago, did not differ materially from those which appeared in the original edition. In the present edition, however, there are manifest signs that the subject of treatment has been most carefully revised, and we do not hesitate to express our conviction that, as a safe therapeutical guide for the student and young practitioner, these lectures are unrivalled either in our own or in any other language.

Foremost amongst the methods of treatment respecting which there has been, and still is, a conflict of opinion, stands that of bloodletting. Sir Thomas Watson, in his thirteenth lecture, discusses this important subject, examines and weighs the arguments on either side, and delivers his calm judgment with an authority which can scarcely fail to convince an unprejudiced reader. It is notorious that for some years past, while there has been a wide-spread disposition to push the administration of stimulants to a mischievous excess, there has been an unreasonable shrinking from the practice of bloodletting in any form. It may be that this excessive dread of abstracting blood is in part to be explained by a natural though not wise reaction from the undiscriminating employment of depletory measures by a past generation. One melancholy consequence of blind submission to authority, or of the senseless influence of what is called fashion, is the almost total want of experience about bloodletting among the current generation of medical men. Now, there is abundant evidence that this great dread of extracting blood is a mere bugbear; men and women bear often large losses of blood with impunity, from wounds on the battle-field, in hundreds of accidents, in floodings from the uterus, and this not only during health, but also when the strength has been already reduced by disease. Sir Thomas Watson maintains that—

"The reasoning of the out-and-out decriers of bleeding is faulty and fallacious. 'Disease signifies always,' they say, 'a subtraction from the general welfare and vital powers of the body.' Granted. 'Your aim as physicians is to obviate this lowering.' This also, as physicians, we may to a certain extent admit. 'But the abstraction of blood *must* increase and promote it.' To this we demur. The relief of pain—in itself a great subduer of vitality—the removal of a local and lowering morbid process, the restraining of the circulation throughout the frame of blood altered and made poisonous by passing through the area of the part inflamed, the setting free or restoring the oppressed and hampered functions of a great blood-purifying organ, such as the lung or the kidney, these are very conceivable ways in which the removal of blood may operate in preventing instead of furthering the depression of vital energy. But, after all, the great argument for the abstraction of blood in inflammation is its conspicuous and undeniable *success* in innumerable cases; though the force of the argument may not be so apparent to those who have scarcely ever, if ever, seen the remedy put fairly to the test."

He then proceeds to show that in many cases of inflammation, as of the pleura, the pericardium, or the kidney, *local bleeding* by leeches or by cupping is a powerful, a safe, and therefore a proper and eligible remedy, and that its beneficial operation consists in diminishing by direct withdrawal, or by diversion, the quantity of blood distributed to the part or organ inflamed. It is shown that in order to obtain relief by local bleeding it is not necessary that there should be a direct *capillary* communication between the inflamed part and the place where the bleeding is effected; on the contrary, all that is required is that there shall be a close *arterial* link of connection. The stream of blood passing through a branch or branches which enter and feed the suffering part will be lessened in proportion to the amount which is withdrawn from the contiguous branches of the same artery. Thus, suppose the case to be one of pericarditis. The internal mammary artery supplies the pericardium and the integuments over the heart. By the application of leeches over the heart, we abstract blood from the integumentary branches of the artery, and in the same proportion we divert blood from the deeper pericardial branches. And this operation of local bleeding is illustrated by the faint and feeble burning of a gas chandelier when the gas is largely diverted and the pressure lessened by a neighbouring street illumination.

With reference to *venesection,* Sir Thomas admits that in past times, when employed for the relief of local inflammation, great mistakes have been made, and a potent remedy has been misdirected. It is not denied that by abating the force of the heart's contraction, and by diminishing the amount of circulating blood, the local mischief may sometimes be lessened, but it is maintained that this relief is purchased at the cost of a needless expenditure of blood, and a consequent weakening of the reparative powers of the body. The mitigation of local inflammation is the proper object of topical bleeding, and this, in Sir Thomas Watson's present judgment, ought "almost never to be attempted by venesection." *Almost* never, he says, because there is at least one exception to this general rule. There are conditions in pneumonia—the very disease that has been the battle-field for the recent contentions about bloodletting—there are in that special disease conditions which warrant and require venesection, not, indeed, as a form of *general* bleeding, but as a derivative *topical* bleeding of the very part inflamed. It is peculiar to the lungs that all the blood of the body has to pass through them, and under their inflammation the portions of those organs that remain permeable by blood may be totally unable to transmit the requisite quantity; so that death may be the imminent consequence of that inadequacy. In this stress venesection may sometimes suffice when all other means would fail to avert the fatal issue. To tap a large vein, to draw off a portion of the circulating blood on its way back to the lungs, must tend indirectly to diminish the pulmonary engorgement, to divert blood from the imperilled organ. The condition here referred to as incidental to some cases of pneumonia is but one of several morbid states, for which prompt and free venesection is the appropriate and the only adequate remedy. In some cases relief by venesection has been shown in the emergence from deep coma, the subsidence of urgent dyspnœa, the departure of unspeakable pain and anguish while the blood is flowing. Sir Thomas remarks that—

"All these morbid states are now recognised as belonging to one category. The striking relief is always due to liberation from the effects of a mechanical obstacle or block in the circulation. Dr. Hughes Bennett, a strenuous denouncer of bloodletting except in these emergencies, Dr. Markham and Dr. George Johnson, who advocate its limited and rational use, all agree in this. As the source of the danger and difficulty Dr. Bennett assigns 'over-distension of the right side of the heart, and perhaps venous congestion and engorgement of the lungs.' Dr. Markham, 'some mechanical obstruction to the play of the thoracic organs, and a consequently distended, oppressed, and a partially paralysed heart.' Dr. Johnson, 'over-distension of the venous system including under that term all the vessels that contain black blood.'"

Then it is pointed out that this *accident* of disease—for such it is—may or may not be associated with inflammation, and it is to the accident, and not to the mere inflammation, when that is present, that the remedy is addressed and adapted. Dr. John Reid long ago showed by experiments how a distended and therefore motionless right ventricle may be set going again by opening the jugular vein of a dying animal, and suffering the blood to flow back again, as it will do, from the gorged right cavities of the heart. An instructive example of relief from opening the jugular in the human subject is cited, and then in brief terms it is stated that—

"The condition which cries out for and obtains relief so signal from phlebotomy may be described as that of great and often sudden engorgement of the vessels that carry black blood—of the systemic veins, of the pulmonary artery, and especially of the right chambers of the heart. In this embarrassed condition of the circulation, with so unequal a distribution of blood in the two different systems of vessels, it is the veins that require emptying, not the arteries. As the tension of the stretched and almost paralysed right ventricle is lessened, the hollow muscle again becomes capable of contracting upon and propelling its contents, the clogged lung is set free, the functions of the oppressed brain are eased and retrieved, and the balanced play of the heart and lungs is restored."

We have devoted so large a portion of our allotted space to the setting forth of Sir Thomas Watson's philosophical views on the operation and use of bloodletting, for the reason that in our judgment they cannot be too widely known. Surely there is ground to hope that in future this powerful means of controlling disease will be employed with scientific discrimination, and not, as in past times, either recklessly abused or timidly shunned as caprice or fashion may dictate.

With regard to the employment of mercury, we find that it is no longer recommended as having antiphlogistic powers in ordinary cases of inflammation. It is still, with reason, looked upon as specially curative, either alone or combined with iodide of potassium, in certain forms of syphilitic inflammation, and in combination with other drugs as a purgative and a diuretic in some cases of inflammation and of dropsy. Referring to the treatment of pericarditis, Sir Thomas says:

"The hope which I once cherished that the inflammation could be controlled by the constitutional influence of mercury has faded away. Pericarditis has been known not seldom to spring up while the patient was still under mercurial salivation. I am obliged, therefore, to recant the advice which I was formerly in the habit of giving in respect of mercury as a remedy for pericardial inflammation. I recommend you to abstain from giving it with the view of obtaining its peculiar effects upon the gums and the general system, but I should never scruple to prescribe it with other aims, and especially for its tendency to quicken and promote the action of some diuretic remedies."

In the recommendation of *paracentesis thoracis* for the removal of a pleuritic effusion Sir Thomas exercises, as we think, a wise and discriminating caution. He advises the operation when the effusion is so copious as to threaten suffocation. Again, when the patient, without suffering much dyspnœa while he lies quiet, is yet evidently losing ground, while other means fail to get rid of the effusion, and, lastly, when the effused liquid, no matter how we ascertain the fact, is known to be purulent. He, however, deprecates the early employment of the trochar in ordinary cases of pleuritic effusion.

Referring to the observations of Dr. Hughes and Dr. Hamilton Roe, who had expressly treated of this subject before Dr. Bowditch's researches were made known, he says:

"To those gentlemen the profession is much indebted for having shown with what facility, and with how little risk and pain, the operation may be performed. They have not convinced me of its frequent necessity."

For this display of caution we find that he has been blamed by one critic, who also says that Sir Thomas fails to recognise the fact "that it is now perfectly easy to tap with (practically) no admission of air." This statement, which curiously enough, has been repeated (shall we say copied?) by another critic, affords a good illustration of the fact that even a reviewer may fall into error, as will be seen from the following extract (vol. ii, p. 145):

"Should you desire to take away, as some advise, so much of the liquid, and no more, as the expansion of the lung and the elastic resiliency of the thoracic parietes suffice to press out, without admitting air, that object may be insured by adopting a simple contrivance of Professor Shuh's, of Vienna, which was shown to me by Mr. Spencer Wells. But a method which I have seen employed by the late Mr. Stanley seems to me more simple and equally certain. He used a trochar furnished with a stop-cock. To the trochar thus shut he adapted a long flexible tube, the open extremity of which was immersed in a vessel containing water."

It would be well if some modern critics would set themselves the task of imitating that which one of them professes to admire in these lectures, namely—

"That high-minded modesty to which exaggeration of all kinds appears a fault to be shunned as scrupulously as falsehood itself."

Now, with reference to the indications for the operation of paracentesis, there has of late been an attempt to show that tapping the chest is useful, and therefore desirable, in a large proportion of cases of simple acute pleurisy. We are quite convinced that this attempt will be unsuccessful, as in our opinion it deserves to be, and we will briefly state our reasons for this belief. In the first place, whatever doctors may say upon the subject, patients and their friends will always look upon the operation as somewhat formidable, and they will not submit to it unless fairly convinced of its necessity. Then, allowing, as we do, that with due precautions the liquid may be withdrawn without the risk of admitting air into the cavity of the pleura, we would ask what is the necessity for this proceeding when, as every physician of experience knows, in the great majority of cases serous effusion into the pleura is quickly and completely absorbed without surgical aid, and with little or no medical treatment? The absorption of the liquid is much impeded and may be entirely prevented when the pleura is covered by a thick unorganized layer of fibrine, which thus intervenes between the effused liquid and the vessels by which it should be absorbed. This exudation of fibrine takes place during the first intensity of the inflammation, and it may most surely and effectually be prevented by the prompt application of leeches, followed by linseed poultices, as recommended by Sir Thomas Watson. Now, it is not a little remarkable that one of the most strenuous of recent advocates for early tapping in cases of pleuritic effusion discourages the employment of leeches, and advises that the pain of pleurisy should be subdued by the hypodermic use of morphia. We highly approve of morphia in aid of and after topical bleeding, but to trust to morphia alone in the early stage of an acute and severe attack of pleurisy is, in our judgment, needlessly to incur the risk of so copious an effusion of lymph and serum as may render necessary at a later stage the operation of tapping. Acute pleurisy, painful as it is, is not a merely neurotic disease, and cannot be successfully combated by anodynes alone.

Before concluding our pleasant task, we feel bound to refer to a suggestion made by one reviewer and echoed by another, that, notwithstanding the labour which has been bestowed upon successive editions,

"It might have been well simply to have reprinted the original lectures, without any attempt to adapt them to the changed state of medical science."

If the labour of revision had been undertaken by some inferior workman, there would have been some reason to fear the result of unskilful attempts to patch an old garment with new cloth.

Happily, however, the gifted author survives, and, while in the full possession of his high faculties, he has, with great labour, subjected his lectures to a more thorough revision than was possible during the earlier and busier period of his life. The result is a work by one master-mind, uniform and perfect in style, from the opening sentences of the introductory lecture to the graceful and generous epilogue at the end—a work evincing in a degree rarely equalled calm judicial wisdom, a truly reverent spirit, and an ardent love of truth.

IX.—Contagious Diseases Acts.[1]

It is not our intention here to discuss the more immediate and obvious elements which connect the medical profession with the legislation involved in these Acts, present or future. There must be very few indeed of our readers who are not satisfied that their operation has within a limited sphere materially diminished the extension and virulence of syphilis, and who on sanitary grounds would hesitate to bid them God-speed. Neither will we linger long over the commercial aspect of the question set before the Privy Council by their medical adviser, who argues that we have no right to spend the money collected from chaste taxpayers in preserving the health of the unchaste. A true political economy teaches that every sick person in every sickness is a loss to the whole community, for his removal from the class of healthy workers deprives the country either of his labour or of money which would otherwise be distributed finally in paying labour. It is therefore financially just to use the consolidated fund for the necessary expenses of these Acts.

But it is to their politico-moral aspect that we think attention should now especially be directed; and medical men are not to excuse themselves from attending to this part of the matter on the plea of the physical needs of the population being their peculiar department. For medical men, as medical men and not merely as citizens, from their intimate relations to all classes at once, have opportunities for forming unbiassed opinions on such-like social questions, which surpass even those enjoyed by clergymen, policemen, and philanthropists specially devoted to the subject. And we will say at once that we consider the mental effect of the legislation now *sub judice* of infinite more importance than any physical result, and that we should hold it quite unjustifiable

[1] *Report of the Royal Commission upon the Administration and Operation of the Contagious Diseases Acts.*" Vol. ii, *Minutes of Evidence, &c.* London, 1871. Folio, pp 846.

to compass a sanitary benefit, however great, at the expense of a moral evil or degradation.

We will take *seriatim* all the objections raised by the opponents on moral grounds to the Acts; omitting, however, those which appear frivolous and the result of mere factious advocacy. And of these serious objections we will endeavour to estimate the value and point out their true bearings on the matter in hand.

First, then, it is urged that by diminishing the risk run by fornicators you remove a natural deterrent from vice. This applies to both sexes, but with greatest force to men; for to the other sex the deterring influence of risking possible disease is as nothing compared with the certainty of placing themselves in the ranks of an outcast, pariah caste, and probably has never frightened a woman from the brink of prostitution. But to men there is indubitably some safeguard in fear. We have heard a middle-aged surgeon of police remark, "Thank God, I had syphilis before I was twenty. It made me *think*—that's what it did; and I have never had illicit connection since. I should have been too frightened." This objection is, therefore, a very serious matter, and we should protest loudly against any legislation which ignores it. But that has not been the case in the administration of the Contagious Diseases Acts. When it was found that the printed notices to attend on a future day, which were given to women subjected to periodical examination, were shown as certificates of health, their issue was immediately discontinued; and every effort is used to make the needful attendance as private as possible, so that it may be not known who are and who are not diseased. Still it must be allowed that a good deal depends on the discretion and good faith of the police by which the Acts are carried out, and that the constant supervision of right-minded and observant persons will be required. When the intending fornicator has only the prostitute's word for the fact of her having been passed as healthy, we do not see that he has any protection in the individual instance. She would be just as likely to tell a lie in a protected district as in an unprotected. But the chief care should be taken not to administer the law in any way that could be made a recognition of brothels or brothel keepers. Not even special visitations of the houses should be made, for it may be inferred that a visit without penal results is in fact a bill of health. The great mistake made by continental governments in their control of prostitution is the direct or indirect licensing of brothels.

It may be remarked that the risk of the minor punishment of vice, gonorrhœa, is not appreciably lessened by the administration of these Acts, as appears from the statistics brought forward by several witnesses. By washing the mucous membrane shortly before inspection, a woman may easily escape the detection of this disease; and she suffers so little pain from it that she does not care to be cured, but lets the secretion get well in the course of nature. To the male urethra the consequences are much more serious, but still they do not involve innocent persons, nor sap the vital development of future generations. So that we may congratulate ourselves that we have left a substantial deterrent to the male debauchee, threatening a punishment, milder indeed, but much more difficult to escape than syphilis, which at the same time confines itself as much as any punishment can do to the guilty. It would be well if it were more clearly expressed in a preamble to future Acts extending to the civil population, that they are designed to protect the innocent, women and children especially, and coming generations, from the consequences of others' vice—in fact, that "contagious diseases" means "syphilis." Legislation about gonorrhœa would be strongly to be deprecated.

Another objection is, that to recognise the existence of prostitution, without aiming at punishment, is really to sanction it, and for special legislation to do that will corrupt the moral sense of the nation. In a country like England the spirit of the law is peculiarly important—more important even than her songs, in spite of the popular apothegm to the contrary—and we are not likely long to retain a higher standard of ethics than what a great statesman calls suggestively "our pædagogue."[1] It is not indeed a religion or fount of morality itself, but it guides thither our blundering steps. This is a very grave matter indeed, for it is incapable of being judged of by practical experience; the mischief, if done, could only be estimated after many years' working, and then would stand in the way of its own cure. For a nation whose ethical feelings were corrupted would become continuously less and less likely to amend the evil. And, no doubt, scrupulous care will be requisite, in watching the working of the details of this legislation, to secure the non-recognition of harlotry as a legitimate occupation. There must be no opposition between legality and morality. But such care we feel sure will be exercised; we see evidence of it cropping out in every portion of the Acts, and of a strong desire to enhance this principle. All who love England and long to see her humblest sons and daughters as pure and brave as their own, have shed bitter tears of disappointment at the historical failure of every attempt to deal penally with fornication. The stronger the panacea the more harm it does, by reason of the difficulty of limiting what constitutes crime.

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[1] ὁ νόμος παιδαγωγὸς ἡμῶν γέγονεν.—Galatians iii, 24.

Now in these acts it seems to us a judicious step is for the first time made towards employing the strong arm of authority in curbing vice. Instead of being protected and trusted as all other occupations are, harlotry as a trade is punished by subjection to suspicion and repugnant questioning. There is a difficulty, doubtless, in classifying and defining the degrees of iniquity exhibited in its pursuit; but a syphilized woman who habitually hires out her person for money may be justly selected as a typical specimen for punishment. And punished she is, by a loss of liberty till she gets well; that is, for a time closely proportioned to the period during which she has been going about with the disease neglected upon her. And the trade is further pointed at as criminal by its pursuers being made subject to punishment with less ceremony than the rest of the population. In other cases of summary convictions the limit of the magistrate's power is six months imprisonment; and in the great majority of cases, with one or two exceptions under the Metropolitan Police Act, three months' is the outside which a magistrate can give on summary conviction. But under these Acts a woman may be detained for nine months upon one certificate made by a doctor on his sole opinion. The very fact of being a prostitute puts her in the position of an imprisoned felon, subjects her to severe discipline, and deprives her of the safeguards provided by our criminal law and the checks which necessarily attend a public hearing. This is very clearly pointed out by Mr. W. Shaen, Solicitor to the Association for the Protection of Women (Answer 19,582). Mr. Stuart Mill also argues that the Acts intentionally take away the security of personal liberty from a particular class (Answer 19,994). These witnesses indeed consider the fact a ground of objection, but we are disposed to take an opposite view, and to agree with the Rev. C. Ellison, who would deal with prostitutes "as with any other criminal" (Answer 20,280), sending those who are diseased to a hospital to be cured while they are undergoing a punishment. What indeed are loose women but criminals, and enemies in open rebellion against the welfare of the State? Their livelihood is seduction, and almost every man who has left the paths of virtue has done so in consequence of the provocative exhibitions of these creatures. To have a son seduced by any person except an habitual harlot is an almost unknown experience. If the tempting to vice, sporadically and for pleasure, is a crime in a man; and if it is possible, as some think, to make it penal; then surely the regular trade of temptation may justly deprive its followers, when brought under the operation of law, of some of those rights of citizenship which the rest of their countrymen enjoy. These considerations will answer, we think, Mr. Mill's demurrer to special legislation for prostitutes as a class.

Strong objections are made by several witnesses to the periodical examination of the person, essential to the administration of any police control of syphilis. Professor Newman seems to aim at condensing all these objections when he designates it as "indecent, depraving, barbarous," "an instrumental rape," "an intrinsic wickedness." The language is somewhat vague, but seems to mean that it is, in the first place, painful and repugnant to the individual; and, secondly, lowering to the moral feelings. These are two quite different characteristics, and, to a certain extent, inconsistent. For the disagreeable nature of the operation, the more acutely it is felt, the more will it inspire disgust at the mode of life which necessitates it. Welcome pain, welcome shame, welcome any temporary evil, that makes sin hateful. We do not doubt that the introduction of a speculum into an ulcerated mucous surface *is* painful, and we hope that women do "loathe" (as the witnesses say) the letting the light of day into their vicious nastiness. We are glad to hear that some come to the examining surgeon in tears, and fall hysterical during the process, and we are not careful to enquire into the balance of evidence as to whether the women do or do not object on these grounds to submit to inspection—though a cynic might make himself merry on the conflict of the positive statements reported by the witnesses to be made regarding other persons' private sensations by a class who are scarce to be trusted in speaking of their own. But we are deeply interested by the opposite opinions expressed as to whether the effect may be demoralizing, degrading, hardening, and destructive of decency,[1] because such a result would render the machinery of the Act unjustifiable. We believe the fact to be, that those who have a taste for indecency use the occasion for enhancing it, and that those already hardened after a short time make a boast of their shame. It is impossible however to imagine that even their real moral nature can be lowered by an exposure which is not (like their habitual exposure) for the purpose of satisfying lust, but for a philanthropic object. But to a different and more hopeful class the periodical contact with a person of superior intelligence to their own, *ex officio* kind and considerate, cannot but be advantageous. They become more decent in their behaviour, more self-respectful and cleanly in their persons, and, in many instances, are restored to virtuous society through the

[1] See Index to Report. Examinations—demoralizing, 2 answers; degrading, 6 answers; hardening, 10 answers; not demoralizing, 1 answer; not degrading, 8 answers; not hardening, 10 answers; promote external decency, 3 answers.

agency of those whom they are introduced to by the periodical examination. While it is doubtful if any are made worse, it is certain that many are made better. The introduction to a new description of fellow-creature cannot fail to be humanizing. What can a common strumpet's notion of a man be? A vulgar animal, so stupid as to be easily seduced by the most glaring imposture, and to be amused by the stalest indecencies, paying money to be flattered by a show of love which barely veils disgust and hatred. And what is her experience of a woman? Such as she is herself. It must be a new revelation to her to be spoken to with Christian courtesy and charity.

We saw lately a proposition made in the Sequel to Dr. Chambers's Harveian Oration, that the ladies who have lately been fighting against so many difficulties in order to secure a medical education, would find an open path of employment in conducting these examinations. Their undertaking the duty would remove all pretext of indelicacy or outrage, while the shame of what is shameful would remain the same. It is true that the task is a disagreeable one; but if one of the opposite sex stands forward and says "Homo sum," she must add "humani nihil a me alienum puto;" the doing of disagreeable work is a test of earnestness in the pursuit of a profession.

There is alarm expressed, even by advocates of the legislation, lest it should effect "an increase of clandestine prostitution." Some state that clandestine prostitution has increased, others that it has diminished. The discrepancy in the statements of fact seems to us to be partly due to the phrase being a vague one; but mainly, and most importantly, to the witnesses' information respecting increase and decrease being derived from different sources. Those who speak to the increase cite the opinion of men who have contracted disease, and who attribute their misfortune to women that they believe not to be on the register, such as shop girls and the like; while those who speak to the decrease, and who are the more numerous, give as their authority observations made by the police and others on the women themselves. Now it seems extremely probable that women who formerly would have lived solely by prostitution have been driven by the operation of the Acts some to be really, and some to pretend to be, engaged in industrial occupations, and these would be very likely to be diseased and to conceal it also. We are sorry for their customers' sufferings, but still it is evident that the cause of morality has prospered; for the women have made the first step towards reform by assuming or pretending industry. If they can once get permanent employment they can always, when they like, re-enter the paths of respectability.

The *decrease* reported in the ranks of the unrecognized is traceable to the attention of the witnesses who speak of it being drawn to those young persons living at home with their friends who eke out their gains, or obtain funds for extravagance, by occasional prostitution. It appears that many of these are frightened at the exposure threatened by the new law, and give up their malpractices. If that effect continues it would alone justify the morality of the Acts.

It is urged by some that it is an injustice to apply a law to one sex which we do not apply to another, and that dissolute men ought also to be under supervision. This is a flaw doubtless in the legislation, but it is a flaw of omission, not of commission, and we shall be bound to aim at some means by which men who actively spread the infection of syphilis can be detected and punished. At present the only case that may be brought under the operation of law is that of a husband who infects his wife, which offence makes him guilty of legal cruelty, and liable to be divorced. Yet because human contrivances are confessedly imperfect, that is not a reason for refusing to adopt them. The difficulty in the present instance is that there is no class of men answering to prostitutes, no male trade whose whole business is seduction, so that we do not see what equivalent rule of inspection can be adopted. However, this is not a time for the periodical inspection of soldiers to be discontinued.

But we must unhesitatingly condemn attempts at a retaliatory warfare of one sex upon the other. We are informed by a witness before the Commission (Answer 13,030), that an association has been formed of persons who engage, first, never to refuse to aid a fallen woman, and, secondly, never even to receive within their doors a fallen man. This spirit of union is directly the converse of that of Christianity and patriotism, where "there is neither male nor female," and is a relapse into the antagonism between the sexes of barbarous times. We have not, however, seen the statement contradicted.

When these Acts were first tried in certain districts we were afraid that serious inconvenience would arise to virtuous, but imprudent, women in their administration. We have been agreeably surprised by the few cases in which this has been even alleged, and the still fewer, we think we might say none, in which the allegation has been justified. All who have been connected with hospitals or the police know how ready uneducated persons are to fancy every man's hand against them, and really without malice to tell groundless tales of ill usage; and it therefore speaks forcibly in favour of the administrators of the laws, and of their conciliatory demeanour, that the mouths of those in risk of injury have been shut. Even the "Men of Kent," who threatened the Home Secretary with a revival of "the spirit of Wat Tyler," have not found a plausible case for wielding such an illogical argument. But the danger of respectable persons being annoyed must not be forgotten nevertheless. It is a very real one, though it has hitherto been practically escaped, and we look with gratification on the energy of those ladies and others who have been agitating the matter in opposition. The same earnest activity will probably, in the event of this legislation being extended, watch jealously over the conduct of its administrators; and all cases of abuse will be rigidly investigated, and the machinery of the Acts improved by experience.

It is important that both sides in this controversy should be thoroughly agreed upon the end aimed at by the Acts. As we understand them, the end is not the suppression of vice, not the suppression of disease, not even of all contagious disease, but the diminution of syphilis only. The curing of gonorrhœa is a side blow, needful in order to include all cases where the true existence of syphilis might be hidden. It cannot be expected, and we have given some reasons why it should not be altogether desirable, that gonorrhœa should be much checked. The object of these pages has been to show that in the proposed means of diminishing syphilis we need not fear that any germs of injury to public morality are latent.

X.—Sir H. Thompson on Practical Lithotomy and Lithotrity.[1]

Part II.—Lithotrity.

By the term lithotrity the author denotes all those processes by which the stone is broken up, crushed and powdered in the bladder, and by which the *débris* are removed through the natural canal of the urethra without the use of the knife. Thirty-five years ago the operation had barely made good its claim to be admitted into the list of recognised surgical operations. Fifty years ago it was only a theory. The author considers that, as performed at the present day, it is a safer and better operation than it was even ten years ago, and that it is founded on principles and regulated by laws deduced from a large experience, and is no longer a mere experiment.

After a short reference to the introduction of the drilling process by Gruithuisen, the Bavarian, in 1813, and to its modification by Elderton, the Scottish surgeon, in 1819, the author

[1] *Practical Lithotomy and Lithotrity.* By Sir H. Thompson, Surgeon Extraordinary to H.M. the King of the Belgians; Professor of Clinical Surgery and Surgeon to University College Hospital. (*Continued from our last Number.*)

mentions the instruments of Civiale devised in 1817, and the contributions of Leroy d'Etoilles and of Amussat.[1] The first successful operation of Civiale was performed in 1824, before a Committee of the French Academy. Improvements in the lithotrite have been made by Weiss, Heurteloup, L'Estrange, Costello, and Charrière; but the experience of Civiale himself the author regards as exceeding that of any other operator. The author's "own experience, however, now very large," has led him to modify the character and action of the lithotrite, and the method, to a certain extent, has necessarily somewhat changed with it, and he considers it, as at present practised by him, to be more rapid and a safer method than any previous one.

On turning to page 169 we find that the author's contribution towards the perfected instrument is that of a cylindrical fluted handle, associated with Weiss's excellent and simple method of changing the sliding into the screw action, by shifting a button along a groove in the handle. There can be no doubt that this cylindrical handle, as applied by Sir Henry both to the lithotrite and the sound, is a very great improvement upon the awkward and ponderous arrangement which is found even in Charrière's improved lithotrite, and upon the flat spoon-handled figure which forms the termination of the sounds in common use.

The remarks at page 151, as to the treatment preliminary to the operation of lithotrity, concerning the importance of attending in metropolitan patients to the general deterioration of health before operation, and also of allowing a time for rest and acclimatization to patients from the country, show evidences of much thought and experience. To overcome the feverishness and excitement of a country patient, resulting from altered habits and anxiety, he considers that from three or four days in some cases to even a fortnight in others should be allowed. Still more important is it to subdue by rest in bed, regulated diet, anodynes, local bathing, &c., the condition of irritability of the bladder and kidneys, and the chronic cystitis, which exists more or less in all cases of calculus of some standing. A favorite remedy of the author is the decoction of the rhizome of the *Triticum repens*, a pint daily in divided doses. From the language of the author we infer that he believes in the local action upon the bladder and other urinary organs of the demulcent virtues of this decoction and of similar preparations, in addition to and distinct from their good effect as mere diluents increasing the quantity and diminishing the acidity of the urine

[1] The author in these references omits mention of the use of Haygarth's sliding instrument, with a screw added, by the late Mr. Hodgson at the Birmingham Hospital in 1825, and also the invention of the oval slit in the female blade by Mr. Oldham.

which flows through the bladder. In this we fancy he differs from some of the best therapeutic writers of the present day. He recommends a pint of the decoction to be taken daily in divided doses.

He also recommends the preliminary employment of bougies, at first soft and elastic, and then those of a metallic composition, previously to the use of the sound. To most patients who do not suffer from the additional evil of a strictured urethra, the passage of a warm, well-oiled, moderate-sized metallic instrument gives rise to less irritation than any of the elastic bougies. And we venture to think that most surgeons having to deal with a healthy urethra would be apt to prefer the use of the sound possessing these qualities, in addition to its power of imparting information as to the size and character of the stone, to a previous faddling with bougies. It is, of course, understood that an irritable urethra, addicted to spasm and to the production of a subsequent febrile attack, or the presence of a stricture or other impediment, will demand previous appropriate instrumental treatment of the kind indicated by the author, who, it is fair to say, afterwards states that this elaborate preparation by instruments is really necessary in only exceptional cases.

Chapter VIII concludes with an enumeration of the conditions most favorable for the successful application of lithotrity, viz.—

1. A fairly capacious and not very tender urethra.
2. A bladder capable of containing three or four ounces of urine, not very irritable, yet possessing a moderate degree of tonicity; that is, capable of expelling its contents.
3. Fair general health.

Chapter IX treats of the instruments employed in lithotrity. It prudently premises that the object in lithotrity is not merely the breaking up of the stone into fragments, but also, as an ultimate end, the reduction of the stone to powder and small *débris*, which can be easily voided by the patient. To this the author applies, as in his opinion the most expressive, the term "granulation." To this word occurs at once the objection that it is already appropriated to a very different surgical meaning.

Lithotrites are divided by the author into two classes, according to the disposition of the blades. The first class includes those adapted by the formation of the blades to seizure and fracture of the stone. They have a fenestrated female blade and a male blade provided with sharp-wedged teeth of a larger or smaller size, the larger adapted to the stronger instruments for dealing with large and hard calculi. The inclination of the blades to the shaft Sir Henry considers should not be more than 120°, as the instrument thereby loses power, though easier to pass through the urethra.

The second class includes those in which the blades are somewhat shorter, and arranged so as to crush fragments to powder rather than to break them up into pieces. They have plain blades, not fenestrated, the female blade wider than the male, which it receives between its raised edges. One of these, figured at page 163, has the male blade so much narrower than the female as to break up more easily the larger fragments, while the male blade of the other is wider, so as to crush more completely the small fragments into powder. The inner surface of the female blade is smooth, with a small hole at the bend, to permit the escape of fluid. That of the male blade is roughened, to catch a firmer hold upon the fragment, and to grind it more completely to powder. The author states that the prevailing fault of both the London and Paris instruments is to have the female blade too deeply recessed near the head, thereby retaining the *débris* and interfering with the perfect action of the instrument. These instruments act, when necessary, as efficiently in removing *débris* as the older fashioned scoop.

The mechanism of the handle may be the same in both kinds of lithotrite, the distinctive efficiency of each being placed essentially in the blades. The most improved mechanism is a simple application of the wheel and screw, admitting of gradual, even, and continuous pressure; and so adapted, in Charrière's, Coxeter's, and Weiss's latest improvements, as to permit of the male blade being opened and shut by the hand alone to search for and seize the stone. This, by a thumb-catch action, can be changed into the screw power at the option of the operator, to crush the stone when caught by the blades.

This mechanism, enclosed in the fluted cylindrical handle of the author, completes an instrument which, as made by Weiss, Coxeter, and Matthews, leaves little to be desired in the way of lightness and power combined. The author gives also a description and woodcut of the rack-and-pinion lithotrite devised by Sir W. Fergusson, and made by Matthews, and states fairly enough the advantages which it possesses in enabling the operator to apply a jerking force to the stone which will act like percussion and will sometimes break a hard stone when the direct screw power fails, and is also somewhat more susceptible of the direct action of the intelligence of the operator than the screw power. We have lately had occasion to verify this in an operation upon a very hard stone, breaking with a flint-like fracture. We have found in addition that the rack-and-pinion power, acting in Fergusson's instrument *parallel to the axis of the shaft,* had the advantage of not twisting round the blades, as the severe application of the screw does, necessitating, where great force is required, the fixing of a movable cross-bar to the cylindrical handle to enable the operator to resist this rotating tendency, which might, when a large fragment is seized by the blades, damage somewhat the coats of the bladder.

Sir Henry speaks, with a complacency pardonable enough, of his power to detect with one of his improved instruments a piece of stone of the size of a *split pea.*

The diminution of the thickness of the shaft by Sir William Fergusson, so as to permit the freer play of the instrument in the urethra, is commended by the author and applied to all his own lithotrites. He considers that the best and strongest instruments, cut from the solid steel, to be those produced by English makers. For merely reducing fragments, however, he thinks a forged shaft is sufficient.

In Chapter X the author applies the term "sitting" as the equivalent of the French *séance,* and the term "operation" as embracing the same total of sittings required to remove the stone altogether.

Of the positions of the patient during the sitting, two modes are given, viz. the *ordinary* and *extraordinary.* In the first, he reclines upon his back on a couch somewhat higher, for the operator's greatest convenience, than the common sofa (viz. at least thirty inches). If an ordinary bed is used, a hard mattress should be laid upon it, and then a firm pillow two to three inches thick, to prevent the patient's pelvis sinking below the plane of the shoulders, the knees being a little separated and bento ver a couple of small pillows. When the patient is so placed the stone will lie a little behind the neck of the bladder. In the exceptional position the pelvis is raised from five to six inches above the level of the shoulders, with the thighs slightly raised and the abdomen inclined from the pelvis. The stone here lies near the posterior wall of the bladder. This position is required when the prostate is large, the patient stout, and in searching for the last fragments.

The figures given of these positions are very large, and the directions of the lithotrite very clear to the comprehension.

The area of operation in the bladder is thus brought to the centre of the viscus, and the part of the cavity immediately below it, away from the sensitive and easily damaged neck of the bladder, and the lithotrite can be reversed with comfortable facility.

Upon the condition of the bladder at the time of the operation, Sir Henry says (p. 178) that he has for several years discontinued the practice of previous injection of the viscus, or even of requiring the patient to hold his water for an hour before the operation, which the nervous anxiety of the patient usually renders impossible. For young operators the presence of three or four ounces of fluid he considers desirable, but he himself, as frequently as not, operates in the empty bladder. By avoiding the preliminary injection of water he thus shortens the sitting and the manipulation more than one half.

He urges justly that most frequently the only method of subduing irritation is to crush the fragments more completely.

A flabby unexcitable condition of the bladder is as objectionable as an abnormal irritability, because the stone may be, if small, hidden in loose folds, and the bladder rendered irregular in shape by the pressure of the neighbouring tissues, as in the dead subject, instead of presenting an evenly contracted ovoid shape.

In introducing the lithotrite the angular bend requires more management than the even curve of the catheter, and the end of the instrument, if not properly managed, is brought into awkward entanglement with the urethra at the pubic arch.

It must be directed more evenly than the catheter, and for a longer period backward before the handle is depressed, the latter action being performed rather by the weight of the instrument alone. A slight rotary motion may be advantageously impressed upon it as it enters the prostatic portion, with a gentle pressure upon the pubis to push down and relax the suspensory ligament of the pelvis. The author, like most other lithotritists, prefers, while doing this, to stand on the right side of the patient, so as not to have occasion to change position afterwards.

In his instructions for finding and seizing the stone the author premises that the operator should accustom himself to the familiar use of the lithotrite, so that he may, when operating upon the living subject manage the instrument instinctively.

Unmistakable woodcuts render the meaning of the directions so plain that it is impossible to be in doubt.

Sir Henry advocates the method of catching the stone employed by Civiale, viz. bringing the blades of the lithotrite to the stone wherever it may be, in preference to that ascribed to Heurteloup and practised by Sir Benjamin Brodie, the principle of which is to place the closed blades of the instrument in a central and safe position, and then, by depressing slightly with them the base of the bladder, to open them and by a slight lateral concussion with one hand upon the other grasping the handle, to cause the stone to drop between them. He finds usually that the blades need not be inclined laterally more than 30° with the vertical line of the body.

The author first determines, if possible, the position of the stone, and then, inclining the blades slightly to the opposite side, carries the female blade backwards, carefully sliding open the male blade, and then, inclining the opened blades towards the stone, almost certainly seizes it with the sides rather than the end of the blades.

If the stone is not found by this means, he searches the whole of the cavity by a series of five movements, which he designates thus—the vertical, the right and left incline, right and left horizontal, and right and left reversed incline respectively, the two latter being especially adapted to search behind the prostate. He considers it essential to good practice to maintain the axis of the shaft as far as possible always in the same direction, moving only the blades laterally. In cases in which the stone is small, movable, or unusually placed, this maxim, taken literally, must surely admit of many exceptions, unless it is meant to hold good only when the stone is already in the grasp of the blades, a meaning which the context alone renders clear in the allusion to an intentional alteration of the direction of the shaft. When thus explained it means simply, we take it, a steady instead of an undulating grasp upon the handle, and a regard for the urethral turning axis at the triangular ligament upon which the shaft of the lithotrite plays lengthwise.

To the rule which has been laid down that a large stone is usually found near the neck of the bladder, and a small one at the back of the trigone, Sir Henry scarcely assents, to the latter part especially, and moreover considers that to seize a large stone near the neck requires much care, that the female blade should be pushed backwards alone, while the male maintains its position at the neck of the bladder, to insure against the stone being drawn with it when opened towards and against the sensitive neck of the viscus behind the pubis.

As to the manner of crushing the stone when seized, the author inculcates the simple rule of keeping the lithotrite in the centre of the bladder, with the certainty of the fragments being found again at the base of the cavity immediately under the instrument, and may thus be picked up again and again with very little movement of the blades.

We feel compelled to speak with less approval of his recommendation that the lithotrite should not, "at all events," remain in the bladder more than one or two minutes. Such a limitation of the time seems to us to savour strongly of the vice against which, in his section on lithotomy, Sir Henry inveighs, as we took occasion to notice in our last, and concerning which he indorses Dr. Keith's application of the maxim "Festina lente."

It is surely not the length of time, within reasonable limits, during which instruments are in contact with the bladder that influences the success of the operation, but the gentleness and care with which the instruments are used while there, and this gentleness and care is, in the hands of the young operators who will constitute the majority of the readers of the book, much more likely to be associated with deliberation than with haste. The manual dexterity which experience has produced in Sir Henry's own hands may have combined gentleness and care with speed of manipulation and conclusion, but such a happy combination can scarcely be taught in a book which, liberally professing to lead others to a like facile result, should all the less be used to proclaim a fact already sufficiently well known. It is the more surprising that this expression should have escaped from the author's pen at page 197, when, at page 191, a very short time before, he states—" As a rule, all these movements are to be executed without hurry, rapid movement, or any other which partakes of the nature of a jerk or concussion, and without causing more than a very slight degree of pain to the patient."

The judicious recommendation which immediately follows, that after the first sitting the patient should lie on his back for twenty-four hours, even while passing water, to prevent the large angular fragments from being driven forcibly against the neck of the bladder, suggests the idea that it is better to crush or "granulate" such fragments, when made, at once, and as completely as possible, even at the expense of a little minute more of time.

An energetic injunction that on no account should the bladder be washed out or injected at the first sitting, because it is desirable to avoid unnecessary irritation, and because, the first object being to make fragments, no great amount of detritus capable of removal would probably be found, is, of course, a most prudent one under the conditions supposed.

No less judicious is the position that calculous matter in a granulated condition will come away more easily than you can remove it, and that fragments too large to pass the urethra will remain behind, do what you will. It is the further statement, that fragments small enough to pass with some difficulty should be left to take their chance, which admits of much difference of opinion.

Sir Henry's position is, that under the vesical irritation induced by ineffective instrumentation for their removal, they are in danger of beng driven into the neck of the bladder and urethra before their sharp edges are smoothed off by a day or two's sojourn in the viscus, when they will find their own way safely enough. We cannot but suspect that, in spite of the author's weighty and concluding warning against a meddlesome disposition, over-haste, and undue anxiety to show the results of our work, it is in this direction that all future improvements in lithotrity will be made, and that a shortening of the whole time occupied by treatment, and especially of the number of sittings, will be accepted as a boon by the future sufferers from this painful malady. We entirely agree with the opinion which the author shares with Sir Benjamin Brodie and Mr. Charles Hawkins, both of whom he quotes in a foot-note to page 201, that the removal of large portions of stone with the scoop, at the risk of laceration of the urethra, and subsequent impaction of fragments therein, with consecutive abscess and urinary infiltration, is a bad practice, and one to be avoided, the example of surgeons of note in the profession notwithstanding. It is much better and safer to crush such fragments into smaller pieces while they are within the grasp of the lithotrite.

The author says justly that the detection and removal of the last fragments frequently demand more skill and operative power than any other part of the proceeding (page 203), and he uses for this purposes a lithotrite with short, wide, plain blades and well-rounded ends, for searching and catching them. It is an instrument very well suited for the purpose, and recommends itself in the woodcut more powerfully and agreeably than in the example given by Sir Henry of its wonderful properties, shown in his own hands, in producing a click upon a fragment so small as to come easily with the instrument through the urethra, to the astonishment of the admiring bystander, and we must say to the controvention of his own rule just quoted.

He claims, fairly, credit for the handiness of the cylindrical handle of the lithotrite, which is really a great improvement both to this instrument and the sound. A case is given in proof of the value under certain circumstances of a lithotrite provided with a hollow male blade to permit of a flow of urine along it, which had the effect of bringing a very small stone with the current thereby induced into the jaws of the lithotrite; it was then crushed by drawing forwards the female blade. This little bit he had previously failed to catch in two sittings. He mentions, without particular encomium, Civiale's favourite "trilabe," for seizing and removing stones from the bladder or urethra. He is not impressed in favour of the practice of copious washings of the bladder to remove the last *débris*, from the use of which he has obtained very meagre results. He makes an exception, however, in favour of Mr. Clover's evacuating apparatus. As to the use of syringes, double-current catheters, and current propellers, devised by French mechanical genius, he regards them as useless and even positively mischievous, in necessitating prolonged contact of instruments with the coats of the bladder. Among these he includes by name Mercier's double catheter "coudée," as well as Maisonneuve's Lithéxère.

A good description of Clover's evacuating catheters and bottle to receive fragments is given at pp. 213—215. In the use of these he advises great gentleness, and his estimate of their advantages he qualifies by the remark that "their employment is quite as irritating to the bladder as a sitting with the lithrotrite," so that he prefers to do even without them if possible.

In the section dealing with the after-treatment (p. 216) Sir Henry states that impaction of fragments in the urethra is of rare occurrence, but gives a drawing of a very handy pair of urethral forceps made for him by Weiss.

For the removal of stones and foreign bodies from the urethra the author recommends the forceps just alluded to, assisted, perhaps, by a cuvette rather than any of the urethral lithotrites or complicated instruments which have been brought out in France. At page 219 he turns aside from the subject of his book to take a passing glance at the instruments for removing hair-pins, pieces of catheter, and other foreign bodies, from the bladder, a subject so closely connected with the direct object of the work as scarcely to be considered a digression. In one case Sir Henry removed a flexible hair-pin from a male bladder by means of an or dinary lithotrite. In another patient, a female, he used one of the French instruments unsuccessfully, from want of sufficient power to bend the pin, but succeeded afterwards with a stronger instrument made by Weiss.

For the febrile symptoms which occasionally occur after a sitting, Sir Henry recommends preventive treatment by warm drinks, warm clothing, and hot-water bottles to the feet, and, above all, ease, gentleness, and rapidity in the operation. For subsequent pains in the loins and sickness, he knows of no better application than hot linseed-meal poultices, sprinkled with mustard, applied to the back. Hip baths, demulcent drinks, diaphoretic regimen, and mild nutriment, with his favorite decoction of *Tritricum repens*. For cystitis and mucous urine he recommends washing out the bladder, and the application of the scoop lithotrite, to break down the large fragments which keep up the irritation, as soon as possible. In orchitis, which occasionally occurs, fomentation and rest, but no lowering measures, are advisable. Hæmorrhage from the bladder and urethra is exceptional. The author lost one patient from severe bleeding from the bladder after a sitting of short duration, the hæmorrhage continuing for a week until death ensued. An autopsy showed intense congestion of the whole vesical mucous membrane, but no abrasion or injury whatever. The patient had been subjected to free loss of blood by the urine previously to the operation. Would not this seem to have contra-indicated lithotrity, and to indicate lithotomy as a preferable operation in such a case?

In another case the disposition to bleed after the first sitting or two caused some anxiety, but the case terminated well. The treatment followed was ice in the rectum and washings of the bladder, carefully conducted, with ice-cold water, elevated position to the pelvis, and strict rest and recumbency.

In elderly subjects retention of urine after a sitting is very common, resulting, not from the impaction of fragments, but partly to defective power of the bladder, partly to swelling of the prostate and urethra. In these cases the condition of the bladder as to distension should be carefully explored, and the catheter used daily twice or even more times.

Sir Henry does not consider it necessary for the patients to be strictly confined to the house if in good health and with a small stone. Air and even gentle exercise may be taken in the intervals of the sittings. We know of one case, at least, in which exposure to the open air in very cold weather led to an attack of renal inflammation under such circumstances.

With respect to the question of the administration of chloroform, the author says (p. 228)—

> "The pain arising from lithotrity properly performed is really not much, It is uneasiness rather than pain, and as a sitting is rarely longer than one or two minutes, the demand on the patient's fortitude is not very considerable."

And in nervous or susceptible patients, labouring under those irritable conditions of the parts to which calculous patients are subject more on some days than on others, of the cause of which we have little knowledge, and over it but little or no control, he thinks that operative procedure should be always postponed when the mere passage of an instrument causes the patient much pain and uneasiness. This indication, or hypersensibility, is, of course, wanting when chloroform is taken and the voice of nature stifled. This benefit is the only one which Sir Henry considers to be attached to the withholding of anæsthesia on the side of the patient. His remark is, indeed, true, that on the part of the surgeon, operating without chloroform produces an unconscious habit of care, gentleness, watchfulness, and delicate manipulation, which is of great value. But we will venture to remind Sir Henry that all these may be arrived at by the appreciative mind and obedient hand equally without the expense of suffering to the patient. We must do him the justice to say, however, that he does not think the sensibility of the patient's bladder, as written on his face should be necessary to tell the operator when he has seized the coats of the bladder instead of the stone! If it were, we unhesitatingly agree with him that the patient's chances of recovery from such a complaint and such an operator are slender indeed!

Lithotrity in children.—From the narrowness of the urethra in patients under puberty, and from the pyriform shape and greater irritability of the bladder necessitating very small instruments and longer time in finding and crushing the stone, because of the uncertainty of its position, Sir Henry comes to the conclusion that they are not such favorable subjects for the crushing operation as adults are. Impaction of fragments in the urethra and distressing irritability, spasm, and retention of urine, are apt to result.

During the operation chloroform is absolutely necessary. Guersant states that out of twenty-one cases of lithotrity at the Children's Hospital in Paris, there were six deaths, two from the operation and four from intercurrent disease. Three others were subsequently submitted to lithotomy, and all of these died. This is a fearful rate of mortality, and, as Sir Henry remarks, English lithotomy yields very much better results than this. The author seems to conclude that with a very small stone, such as could be crushed at one, two, or three sittings, lithotrity is a good operation for children, but with the larger calculi lithotomy is a safer operation. He recommends a very small lithotrite, of which he gives an illustration, with the handle adapted by means of a cross-bar for crushing by manual force only. Small lithotrites are now made by Weiss, Matthews, and others, so powerful and strong that more will probably be done in this direction in England than has hitherto been the case.

The practical maxims with which Chapter XI concludes express in brief the opinions given by the author upon the whole subject of lithotrity under ordinary conditions. All or most of these have already been passed in review, but we take again occasion to express our dissent from the injunction here again recapitulated, "that no sitting should exceed five minutes in duration, except under very peculiar circumstances," and that "the large majority of sittings should occupy only two or three minutes." Such an arbitrary limitation of time can only contribute to a hurried and careless style in young operators, in which the element of quickness will be unduly and injuriously cultivated at the expense of other advantages of somewhat more value. We think the author unduly exaggerates the effect of mere contact of proper instruments with the coats of the bladder when in a fairly healthy condition.

In Chapter XII are considered cases of calculus complicated with serious organic disease. Sir Henry states that he has not found that organic stricture of the urethra constitutes an inseparable obstacle to lithotrity, and gives in the appendix of cases three notable instances, viz. 91, 110, and 194, of the successful application of the lithotrite under these circumstances. His method is first to overcome the stricture by "continuous dilatation," by tying an elastic-gum catheter, large enough to pass the stricture with tolerable ease, and in two days replacing by a larger size, until No. 10 or 11 is reached. He then gives chloroform, and uses a rather small flat-bladed lithotrite, removing with great care the *débris* made. As the cases referred to in the appendix are very meagre in detail, we are at a loss to understand exactly how Sir Henry accomplishes this desirable result, and are left to the inference that it was done with the scoop lithotrite only. If so the stones must have been of very small size, and since one of these fortunate patients was also the subject of diabetes (Case 110) we presume that none of the five sittings recorded were prolonged beyond the three to five minutes fixed by the operator, for fear of the effect of the continued contact of instruments with the bladder. Sir Henry has no doubt that lithotrity may be made successful in stricture cases, except in case of large hard stones. Does he not think that in such instances some more certain and reliable means of removing the *débris* than the forceps-like action of the lithotrite scoop would render his anticipations more probable?

Still more, if possible, are such means to be desired in cases of enlarged prostate, in which the fragments are not expelled by the natural contractions of the bladder.

Sir Henry states that he has met with less difficulty than might have been anticipated in such cases by the use of the scoop lithotrite and Clover's apparatus. His good skill and, shall we add, good fortune (quite as useful an attribute of the surgeon as the other), have had under these difficulties as good an outcome as in most of the perplexities of lithotrity.

In confirmed atony and paralysis of the bladder there is frequently found the complication of a calculus, chiefly of a phosphatic character. In such cases the author considers that, on account of the habituation of the organs to the use and contact of catheters, we have a far less formidable complication than in enlarged prostate. His treatment by the use of the lithotrite and the evacuating apparatus of Clover is mentioned only by a very brief allusion.

Sacculation of the bladder the author considers to be one of the most serious and unpromising complications the surgeon can meet with, both in lithotomy and lithotrity. He considers that he has overcome the difficulty of diagnosis in these cases by causing the patient, after apparently complete evacuation of the bladder by means of a catheter, to lie on each side alternately for two or three minutes and then to rise to the erect posture; then, if some ounces more are withdrawn, "there can be no doubt as to the existence of some sac in the bladder" (p. 241). Although Sir Henry has tried this experiment repeatedly, and has founded upon it a diagnosis of sacculation which has been ultimately confirmed by examination after death, a shrewd doubt is irresistible as to the possibility of distinguishing by this means a true sacculus, especially if small enough to retain a stone, from irregularities of the cavity of the bladder arising from nodulated tumours of the prostate and other growths. Something of this has apparently dictated the author's exclamation, "Unhappily, the diagnosis of sacculation is not by any means always possible."

In these cases, again, the desirability of removing manually the *débris* after crushing is presented to the author's mind, although his means of doing so are so limited as to lead him to the opinion that lithotomy is, *cæteris paribus*, somewhat less hazardous in such cases than lithotrity. In one case of malignant tumour of the bladder the author crushed a coexistent calculus with extreme gentleness and care, with the effect of much mitigating the sufferings of the unfortunate patient during the remaining months of his existence.

He has had several times to perform lithotrity for patients suffering from advanced renal disease. He found them more liable to severe constitutional disturbance and fever after a sitting, and hence advises the sittings to be short and at longer intervals. All rapid changes of temperature should be provided against.

In cases of multiple calculi he considers the advantages of lithotrity to be more promising than a single large one, weight for weight. The presence of two or three stones, each of considerable size, however, augments the risk. The author recommends that one should be attacked at a time and perfectly crushed before meddling with the others.

Chapter XIII deals with the results of lithotrity. In discussing this subject the author premises some remarks upon the difficulty of comparing the results with those of lithotomy on account of the selection of the more favorable and smaller cases for the former operation, and the unavoidable discrepancies in the rule of selection with different surgeons, some limiting the application of the lithotrite to stones of three drachms' weight, and others extending its use to those of five or six drachms or more. The latter will, of course, have results more unfavorable than the former.

The estimate made by Sir Benjamin Brodie in his paper in the 'Medico-Chirurgical Transactions' he looks upon as a valuable record of his own experience, but no more, and so also with the annual reports of Civiale. The latter surgeon he considers to have been so biassed in favour of the method he has practised so long and so successfully, and towards which he contributed so much, that he barely admitted that lithotrity could occasion death at all. This bias influences all his writings upon the subject, so much as to lead to his attributing all the deaths to some other cause than the operation. Upon this the author remarks that "Lithotrity does not less surely destroy life in certain cases, although no palpable lesion may be produced, than lithotomy does when the necessary wound proves fatal" (p. 248).

At the meeting of the British Medical Association, in Dublin, in 1867, Sir Henry presented an analysis of 100 cases of operation, in which 84 cases of lithotrity were compared with 16 of lithotomy. Since that time he states that he has operated upon 120 cases more by lithotrity, making a total of 204 cases, all performed by the method and instruments in present use. All the cases were in adults, the youngest 22 years old; most of advanced age, the oldest 84 years; eighteen of them are recorded twice occurring in the same individual. In the few cases in which a third operation was necessary to remove calculous matter continually forming, none has been recorded more than twice. To this there occurs one remarkable exception (given in a footnote), in which two years after a second operation for a single stone, in a gentleman upwards of seventy years old, he found numerous small stones, which he removed by a third operation, in six sittings, the *débris* collectively weighing upwards of an ounce.

In Brodie's paper one individual reckons for eight cases in the course of as many years. Sir Henry's 204 cases thus represent 185 individuals, who include (as he specifies with excusable though evident complacency) patients from every quarter of the world, "which seek the aid of a well-known operator." As a rule, crushing was confined to calculi below one ounce in weight, *i.e.* about the size of a large date.

In considering the question of the number of recoveries per cent. Sir Henry requests attention to the three following statements (p. 252): 1st. That he has never found it necessary or desirable to complete by the use of the knife a case of stone in which he has commenced the operation by crushing, as has been frequently done in Paris, and thus the death, if occurring, has not been placed to the account of lithotrity, but to that of the sister operation.

In a footnote to the same page, however, he qualifies this statement by giving the particulars of two cases, one in which he removed the phosphatic crust from an oxalate of lime stone, but finding, we suppose, that it was too hard a nut to crack, he afterwards performed lithotomy.

The other case is, we presume, that of a gentleman well know in the profession, for whom Sir Henry crushed several small uric acid calculi, at six sittings, in October, 1868? (*sic*). Afterwards he had severe cystitis, orchitis, and abscesses in the scrotum and abdominal walls, and on the 20th April, 1870, an examination under chloroform having been decided upon in the presence of Sir W. Fergusson and Messrs. Pollock, Marshall, and Hutchinson, two small uric acid calculi were removed by lithotomy by Sir Henry. Half a pint of matter escaped from the incision. Sir Henry states that the stones were not fragments, but entire calculi, and his case is that these were formed afresh in the *five months* between the operation of crushing in the months of October and November and the month of April. We presume that some error has crept, therefore, into the text, either in the date 1868 (? 1869) or 1870. The case, of course, is open to the surmise that these stones were present in the bladder at the time of the operation of crushing.

2ndly. Sir Henry states that he has not left incomplete or unfinished a single case upon which he has begun a crushing operation, and that the practice of doing so, not unusual in Paris, as also the custom of refusing operative interference in a certain proportion of unfavorable stone cases, is almost unknown to English surgeons. It is much to the credit of the latter, indeed, that such a statement can be made.

3rdly. Sir Henry has never refused to operate in any case of stone, either by lithotrity or lithotomy, and in some he has crushed at the patient's request when lithotomy would have been a preferable operation, although absolutely refused by the patient. He states that in two or three fatal cases given in his table, lithotomy would have been, as he advised, the preferable proceeding.

By a saving clause he asserts, however, that, "as a rule, the surgeon must decline to perform any operation but that which appears to him the better of two courses open, but in rare exceptions he cannot refuse to be influenced by circumstances, and adopt the less promising when by its means some hope of saving life exists" (p. 253).

The summary of his 204 cases, averaging 61 years of age, he gives as follows (p. 254):—13 cases ended in death, and the rate of recovery was therefore $93\frac{1}{2}$ per cent. The first 84 cases included 4 deaths, or at the rate of 95 per cent. of recoveries. The last hundred were rather less promising, though he remarks justly that two or three bad cases tell heavily against a small percentage rate of mortality in the total.

The most common cause of death in the fatal cases was inflammation ofboth kidneys. A certain amount of cystitis is usually met with in cases where several sittings are necessary. Rigors more or less severe indicate that the kidneys have been invaded. Fever continues, and in the fatal cases slowly increasing, exhaustion and death occurs in from seven to twenty-one days. Traces of inflammatory action and deposits of pus in the kidneys and course of the ureters are the post-mortem signs. The author is satisfied that in some or all of these cases chronic pyelitis or nephritis has pre-existed, set up by the stone before operation, and he states that we have no certain means at present of detecting such a condition. There may be no albumen or any significant deposits or renal casts in the urine. The mucus, pus, and blood, which may be present in any cases, are usually derived from the bladder, and it is impossible to determine whether their source may not be partially from the kidney. We are not so certain that the microscopic pathologists of the urine will unreservedly accept Sir Henry's conclusion upon this matter, viz. that "in fact, neither physical signs nor subjective symptoms are by any means frequently present, yet advanced pyelitis and even sometimes chronic nephritis may exist" (p. 255).

Acute *cystitis* is more liable to be fatal when the bladder is previously diseased and sacculated, when frequent and continued catheterism to relieve the atonied viscus may induce slow cystitis or pyelitis. In three cases only the scourge of surgery, *pyæmia,* was the cause of death.

The author thinks that though it is not possible to draw so hard and fast a line as that drawn by Sir B. Brodie between the cases in which a fatal result occurred from the operation itself, and those in which it is to be attributed to the coexistence of some other disease roused into activity by the shock of the operation, but tending to prove ultimately fatal even if let alone, yet that this basis affords a useful and fair classification. He would place among the latter the cases of advanced organic diseases of the bladder or kidneys, such as marked sacculation of the former viscus and pyelitis or other organic change in the latter organs, and he considers that they would prove equally fatal after either lithotomy or lithotrity. These conditions existed in five out of his thirteen fatal cases. In the eight other fatal cases the death may be attributed to the operation. Some showed mere exhaustion, others acute cystitis, and the three cases of pyæmia. Thus, Sir Henry claims as a fair view of the subject a mortality of only four per cent. due to the operation (p. 257). He does not state in the record of any of his pyæmic cases whether any injury or abrasion of the surface tissue had been caused by the operation. This omission, although, perhaps, in one of the cases (Case 193, p. 321) unavoidable from the patient's absence from town, is yet unfortunate as giving us no clue to the occurrence of a malady which is so often traumatic in its origin.

It is always pleasanter for both author and reviewer to dilate upon the details of successful than of unsuccessful cases, and we turn with more satisfaction to read that "by far the greater number (of Sir Henry's patients) are living still, free from all symptoms and enjoying fair health." Three were masters of hounds and over seventy years of age when operated on, all of whom returned to their duties, and two had enjoyed many a hard day's riding since.

In the practice of lithotrity the author is forcibly reminded, in contemplating the result of most of his cases, of Deschamp's classical axiom, "causâ sublatâ effectus tollitur." But, in a few, chronic inflammation continues to affect the bladder, and these, he thinks, occur more in middle life than even in advanced age. He considers that this result ought not to be produced by the operation if gently and properly conducted, and he again alludes in terms of reprehension to the practice of dragging large angular fragments through the urethra.

Among the 204 cases there are eighteen in which the stone has reappeared after a year or more's absence, necessitating another operation. Sir Henry explains that this may occur in two ways. 1st. By a fresh descent from the kidney. This seems to have occurred in seven cases. 2nd. More common than the last, so as to constitute an opprobrium in lithotrity, viz. the formation of phosphatic incrustation through persistent chronic cystitis. This occurred in eleven cases, with the addition, perhaps, of three others, lost sight of afterwards by the author. One of the causes of this persistence, viz. irritation caused by instrumental manipulation, is looked upon by the author as yet capable of diminution by improved instruments, methods of operating, and skill in the operator. A good deal may be done by daily injections of the bladder with tepid water, and thorough washing out of mucus, pus, and sand. Sacculation of the bladder encysting a stone accounts for some few of these obstinate cases. Sir Henry has diagnosed it in three or four living cases, one of which was afterwards *identified by examination* (whatever that soothing *eupheuism* may signify we are left to infer, and venture to do so in the usual way, viz. that it means necropsis) by himself and Mr. Cadge, of Norwich.

At this point of his labours (p. 262), Sir Henry comes to the important practical conclusion, not offered as his own or any other persons' opinion, but as the hard logic of facts, that lithotrity is an eminently successful operation, and that in a number of cases, by no means limited, success may be regarded as a certainty, absolutely and with as little fear of contingencies as the minor operations of surgery, such as the opening of a small abscess or the passing of a catheter. We have a sentence placed before us conspicuously in italics :—"*For I have never lost a single patient in the whole course of my experience after crushing a stone which was no larger than a small nut*" (p. 263). "And this is a size at which, with very very few exceptions indeed, every stone should be discovered." But again, "The rate of mortality will correspond with augmentation in the size of the stone, and with the amount of existing disease and age on the part of the patient."

Perhaps as much could really be said of nearly every operation within the scope of surgery. All diseases and injuries vary in the severity of their consequences with their extent, the coexistence of other diseased conditions, and in relation to the age of the patient. And what can be said of all or nearly all may undoubtedly be said by Sir Henry in support of the operation to which he has devoted his special attention, without fear of contradiction by surgeons perhaps less experienced than himself in this particular operation.

Of the 204 cases of lithotrity, 135 were instances of uric, 47 of phosphatic, 16 mixed, 4 of oxalate of lime, 1 of phosphate of lime, and 1 of a rather large cystic oxide calculus.

The last chapter of the author's work deals with, perhaps, the most difficult part of the whole subject, viz. on the choice of proceedings best adapted to different cases.

So long as lithotomy was the only remedy for the removal of stone from the bladder, little choice was afforded except that between the lateral, median, or high operation. But when the safer operation of crushing became matured and improved, Sir Henry believes that a "*diagnosis of the size, form, and chemical character of the stone was absolutely necessary,*" so that "the case and the method should be judiciously adapted to each other" (p. 266).

He considers that at first "the new method, although a great advance in surgery at this time, actually increased the fatality from stone operations, and was a positive calamity for many stone patients at that era." We have at this point the somewhat strong statement, the words "I have abundant evidence of the increased mortality which was occasioned by it during the period in which its application was mainly experimental." "The cause was, partly, that experience was at first necessarily wanting by which to form rules for the operator's guidance, and partly that the importance of diagnosing the characters of the stone had not been perceived." Cases better adapted for lithotomy were subjected to the crushing operation and terminated fatally. The author still further ventures to say with deliberate conviction, "that the importance of such a diagnosis is not sufficiently perceived *at the present day* and, consequently, only a portion of the advantages placed within our reach by modern methods has yet been attained" (p. 269). Again, unless this is done (*i. e.* the physical and chemical diagnosis) "with tolerable accuracy it will be better to return to the use of a single method, say the lateral operation, than to risk the application by guess of a method inapplicable to the case." This statement Sir Henry repeats a little further on, and in proof of the profound nature of his conviction, in a distinct paragraph and in italics. The means by which he at this place proposes to ensure this accurate degree of information is a measuring sound of his own invention, hollow and provided with an eye at the bend, so as to let off or inject water into the bladder as may be required, and mounted on the cylindrical fluted handle of which we have in the foregoing pages spoken with approval. The shaft is graduated, and has a movable marker to indicate the distance between the touch of the point on the stone behind and that in front.

Sir Henry determines the points of a classification based upon (1) size—small, medium, or large; and (2) texture—soft or hard, and of the hard stones whether friable or compact. His definition of a medium sized stone is, that the mean of two of its diameters is one inch. Soft stones are phosphatic, and hard stones oxalate of lime, uric acid, or urate of soda, sometimes mixed with phosphates. Oxalate of lime and uric calculi are very compact, require much force to break them, and fracture into hard, sharp, sometimes jagged splinters. Urate of soda and cystine are also hard, but more friable. Mixed calculi break easily, because of the unequal power of resistance of its several layers. Phosphate of lime is hard, and breaks up into angular rhomboidal masses.

In a paragraph further on Sir Henry prefers to estimate the size of the stone with the lithotrite; its hardness or softness by the click or ring; its friability by ascertaining the persistent deposit in the urine for some days or weeks. From the last symptoms he says, "one may almost certainly judge of the chemical nature, and hence of the physical structure, of the existing calculus;" and he gives the prudent truism as a reminder, "that phosphates are frequently present from irritation produced by acid calculi." We must here submit, with all deference, that the author has not afforded in these pages a sufficiently explicit method or description by which another may judge with the same quasi-certainty that Sir Henry's *illative* power apparently gives him upon the point immediately before us; and, moreover, that we should hesitate to pronounce with any such a degree of certitude upon the author's premises, with the single exception of the following:—"But in very many cases small calculi have been passed before, and their composition is known." The presumption which directly follows this, "that which is in the bladder will mostly be identical in kind," expresses exactly the degree of certainty, viz. from the probability of sequence, to which a cautious man would like to venture in many of these cases.

The single or multiple character of calculi, Sir Henry prefers to determine also with the lithotrite rather than the sound, which is deceptive. He seizes one stone firmly with the instrument and strikes upon the other with it.

After having carefully studied the herein-contained results of the author's reflection and experience, we can scarcely say that the merits of this simple classification are so striking, original, or beneficial, as to render it probable that the lithotritists who have preceded Sir Henry Thompson, ("fortes vixerunt ante Agamemnon,") should not have been able to shake their ideas into a right form without its aid, to arrive at those safe conclusions which are necessary to make lithotrity a gain upon the older operation, and to guard against that increased mortality of which the author complains, as well as against any underrating of the importance of diagnosis.

The remaining series of necessary data are age, absence or presence of local disease, and susceptibility to constitutional disturbance.

From the table which the author gives in the book, it appears that one third of the total number of cases occurs under six and a half or seven years of age. One half the entire number of cases occur before the thirteenth year. These he considers, with a few exceptions, to be best adapted for lithotomy.

The author considers, however, that in proportion to the number of the existing population at that age, calculus is most common between fifty and seventy, next between two and six years, least frequent between twenty-six and thirty-six.

The author has operated by crushing in two patients affected with Bright's disease. One of them died within one year afterwards; and, as he remarks, encouraged by this result, he operated upon the other, probably a more advanced case, but death rapidly followed. This was anything but encouraging, and, accordingly, Sir Henry's conclusion is the not unreasonable one that, "When the disease is in an advanced stage, no condition is more unpromising for the success of any operative procedure."

Another class of renal cases are the results of continued irritation of calculus in the bladder, of stricture of the urethra, such as chronic nephritis, pyelitis, sacculation and cystic formations. Of these, says Sir Henry, "We can by no means be certain from any signs or symptoms that the changes exist in any given case, and it is certain that they prejudice exceedingly the results of an operation" (p. 279).

Cases of constitutional irritability uncomplicated by organic disease are more hopeful; preparatory treatment and careful manipulation may bring the system into such a tolerating condition as to prevent the necessity of dealing with the stone by the one operation of lithotomy instead of several by crushing.

In case of children with small stones, capable of being pulverized by a single application of the lithotrite, Sir Henry considers crushing to be, at all events, admissible.

He considers that in choosing between lithotomy and lithotrity in adults it is necessary to draw a broad distinction between healthy and diseased subjects. In the former, with tolerant bladder, medium sized stone (and not above that size, viz. one inch diameter, in cases of oxalate of lime stone), whether hard, compact, friable, or soft, lithotrity should be preferred. In stricture cases with a small stone he thinks median lithotrity (which, for the most part, he thinks an unnecessary operation) may be preferred; with a large stone, lateral lithotomy should be selected; but if the stone be friable, even a large stone in stricture cases is best dealt with by crushing.

In patients diseased, but not markedly so as to the urinary organs, yet feeble and failing in strength, he prefers lithotrity. Where well-marked disease of the urinary organs exists, as in stricture, usually lithotomy is chosen, but Sir Henry has been able, as we have before seen, by previous dilatation to treat the stone successfully by crushing.

In enlarged prostate with tolerant bladder, and stones under medium size lithotrity is successful. But with an irritable bladder and large hard stones lateral lithotomy seems preferable to either lithotrity or median lithotomy.

Atony of the bladder, formerly held to contra-indicate lithotrity, is now considered much more favorable for it. Lithotrity is admissible even in marked disease with sacculi of the bladder, cystitis, with constitutional disturbance and tumours of all kinds. Though it is hazardous, it is not more so than lithotomy in such cases.

For cases of very large sized stones the author thinks that the lateral operation is, upon the whole, the best.

Sir Henry closes the last chapter with a glance at the question which must frequently be met with in practice, viz. should we ever refuse to operate for stone, and if so, under what circumstances?

In hospital practice in this country very few cases are rejected as unfit for operation. In private there are more instances, in consequence of the greater means of alleviating the pain in well-to-do patients.

In France a large proportion of cases are rejected, and it appears to the author that lithotomy is more dreaded there than in this country, both by patient and operator, while the French surgeons seem unwilling to hazard the application of lithotrity in very unpromising cases. In the practice of M. Civiale, in 1862, eleven cases out of a total of sixty-nine were rejected as impossible cases for either operation.

Sir Henry thinks, however, that the percentage of recoveries in England after lithotomy, with large stones and in broken-down constitutions, viz. two out of three and a half, justifies us in offering that chance, but he would make an exception against advanced renal disease.

The author concludes by adverting to the importance of applying lithotrity at as early a period in the growth of stones as possible, to detect them early, and operate on them while small, "*rather than to extend the process to large stones,*" and he believes that undue attempts to extend the application of lithotrity to very large stones will end in disaster. The surgeon is compelled, however, to take his patients as he finds them, and must apply his art to the case presented, without laying much stress upon the way in which it came about. Yet the fact adverted to in the last two pages is curious, that the later records of stone cases show a decreasing average size in successive years. Sir Henry ventures to foretell from this phenomenon (while disclaiming pretension to the gift of prophecy) that lithotrity will come to be still more frequently applied, as the stones are detected earlier, and as it becomes year by year more perfect in detail. In the Appendix to the volume are given notes of the 204 cases of lithotrity. They are very brief, not to say meagre, and give little more than the bare evidence that lithotrity is Sir Henry's strongest point, and that in this respect he is, if not a prophet, verily a past master in the masonry of stones.

XI.—The Pathology of Diabetes.[1]

It is a profitable custom to take stock at intervals of the state of our knowledge on certain subjects, to review our progress, and to mark out the line for future advance. In no branch of human inquiry is this kind of survey more useful than in medicine, and in no part of medicine than in the case of such diseases as diabetes, of which our knowledge is daily growing with the advance of physiological experiment. When the subject of this article was last considered in these pages the experiments of Pavy had been confirmed by McDonnell, Schiff, Meissner, Jäger, and Eulenberg. Bernard's theory of the sugar-producing function of the liver was regarded as a view based altogether upon the fallacy of post-mortem change, and the glycogenic function was relegated to the category of many other forgotten speculations relative to hepatic function.

Now, once again the whole subject demands attentive consideration, on account of the new facts which have been accumulated during the last three years. To show how far recent observations tend to establish or refute the conclusions which not long since appeared so certain will be the object of this article.

The works before us naturally divide themselves into two classes, the experimental and the clinical; some being simply a record of physiological experiment, whilst others are wholly occupied with the clinical aspects of saccharine assimilation. In accordance with this division we shall consider the new matter to be found in them.

The experiments on which Pavy based his opinion, that the sugar found in the liver after death was the result of post-mortem change, were, indeed, controverted by Dr. G. Harley in

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[1] 1. *On the Origin of Diabetes, with some new Experiments regarding the Glycogenic Function of the Liver.* By W. T. Lusk, M.D., Professor of Physiology, L. I. Medical College. New York, 1870.

2. *Sugar Formation in the Liver.* By J. C. Dalton, M.D., Professor of Physiology in the College of Physicians and Surgeons, New York. Reprinted from the 'Transactions of the New York Academy of Medicine,' June, 1871.

3. *Sulla Poliuria e il Diabete, Studi e Considerazioni.* Del Prof. Pietro Burresi. Estratte dal volume secondo, fascicolo primo, della 'Rivista Scientifica de Fisiocritici de Siena.' 1870.

4. *Diabète Sucré.* Jaccoud. 'Nouveau Dictionnaire de Médécine et de Chirurgie Pratiques,' tome onzième. Paris, 1869.

5. *Remarks on Diabetes, especially with reference to Treatment.* By William Richardson, M.A., M.D., &c. London, 1871.

6. *The Skim Milk Treatment of Diabetes and Bright's Disease, with Clinical Observations on the Symptoms and Pathology of these Affections.* By Arthur Scott Donkin, M.D., &c. &c. London, 1871.

1860. The strong confirmatory evidence of the accuracy of Pavy's results, which came from so many independent observers, might have been considered sufficient to settle the question. There was, however, left behind some slight room for doubt in the fact that in all these experiments the portions of liver removed ever so rapidly showed a certain trace of sugar, minute as compared with the quantity developed a few minutes after death, it is true, but still sufficiently distinct to induce some sceptics to reinvestigate the question.

In 1868 Professor Flint, jun., published in the 'New York Medical Journal' an account of some experiments on dogs. Portions of liver were cut out during life, sliced into boiling water, and an extract made, which, when tested by Trommer's and Fehling's tests, was found to give no decided trace of sugar. When the operation occupied only ten seconds no trace whatever of sugar was detected. Dr. Flint's next step was to obtain the blood of the hepatic veins, which was done by ligature of the inferior vena cava, this operation occupying less than a minute. In the blood so obtained there was well-marked evidence of sugar. From these experiments he came to the conclusion "that during life the liver contains only glycogenic matter, and no sugar, because the great mass of blood which constantly passes through that organ washes out the sugar as fast as it is formed; but after death, or when the circulation is interfered with, the transformation of glycogenic matter into sugar goes on. The sugar is not removed under these conditions, and can then be detected in the liver." Pavy replied, "That so quickly is sugar formed in the liver after death that the result was to have been expected, considering the expenditure of time involved in applying the ligatures." To test the value of this objection another American physiologist came forward. Professor Lusk made his experiments on dogs, and first confirmed the results of his predecessors as to the absence of sugar in the liver when rapidly removed and treated by methods calculated to arrest post-mortem change. His next step was to obtain blood from the hepatic veins, by lifting up the liver and opening them as they emerge. The blood thus obtained was always strongly saccharine, but the time occupied in the operation left room for objection. He next endeavoured to obtain the blood of the right side of the heart, according to Pavy's method, viz. rapidly opening the chest, and placing a ligature round the base of the heart. The amount of blood procured in this way from a dog of ordinary size was, however, too small to give definite results. There remained only Bernard's plan of passing a catheter down the jugular vein into the right auricle. Dr. Lusk performed this operation, over and over again, with great care, observing all the precautions indicated by his predecessors. He drew only one ounce of blood in each experiment; this was treated with alcohol, and when tested was found to contain only a small quantity of sugar, varying from a quarter to half a grain. The blood of the jugular vein was next tested, and in each case a feeble reaction with Fehling's test was observed. So far every step confirmed Pavy's researches; traces of sugar were proved to exist in different parts of the venous system. The only question that remained was, were the quantities of sugar in the blood of the right heart and the jugular vein at all equal? To determine this question Lusk tested the comparative powers of the extracts of the two bloods, in discharging the colour of a given quantity of Fehling's solution, and found that the result was very much in favour of the blood drawn from the right heart. In order to show the constancy of his results, he repeated his experiments in four additional instances; twice in dogs fasting and twice during full digestion. The quantity of sugar appeared to be two to four times greater in the blood of the right auricle than in that drawn from the jugular vein. Three times the blood was drawn from the jugular vein after it had been drawn from the heart, in order to meet any objection as to the disturbing effects of the operation; Pavy having affirmed "that should sugar appear in the right ventricular blood, it will also be found to a corresponding extent in that of the carotid artery and jugular vein." From these experiments Lusk concludes that, while Pavy is right as to the post-mortem nature of the abundant sugar-production in the liver discovered by Bernard, we are authorised in believing—(1) that the blood of carnivorous animals fed on nitrogenous food contains appreciable quantities of glucose; (2) that the blood of the right side of the heart contains from a quarter to half a grain per ounce, a quantity two to four times greater than that found in blood of the jugular vein; and (3) that this argues a by no means insignificant amount of sugar in the hepatic blood before dilution by the blood of venæ cavæ, and that we are thus forced to admit the sugar-formation in the liver, though we fail to detect the presence of sugar in the liver-tissue.

While these results were being arrived at, Professor Dalton, the distinguished Professor of Physiology in New York, was engaged in a somewhat similar inquiry. His first experiments having convinced him of the necessity of determining the exact limits of sensibility of the various tests for sugar and the best mode of employing them, Dr. Dalton made a very careful preliminary investigation on these points. After carefully computing the limit of the practical operation of Fehling's test, which he found to be the most delicate, and capable of detecting even 1-1000th grain of sugar dissolved in one cubic centimètre of water, he proceeded to test the extract made from portions of liver removed from living dogs fed on animal food. He used a special form of machine for comminuting the liver-tissue, so that the contact of alcohol or boiling water should at once affect its entire mass. In all his experiments, twenty in number, he was aided by three assistants, and the average time occupied, from the separation of the liver to its immersion in alcohol or boiling water, was six and a quarter seconds; the longest time was thirteen seconds, the shortest three seconds. The extract always contained sugar, and in the ten instances in which it was determined this sugar varied from 8-10ths to 4 parts per 1000. Dr. Dalton further proved that the sugar could not be due to the presence of arterial blood in the liver when removed, by excising and testing the spleen in three cases after the removal of portions of the liver. In no instance was any sugar detected in the splenic tissue, and even when the extract was concentrated to one tenth its volume, the reaction obtained was altogether too feeble for quantitative determination. These experiments, which are models of careful scientific research, justify, Dr. Dalton thinks, the following conclusions:

"1. Sugar exists in the liver at the earliest period at which it is possible to examine the organ after its separation from the body of the living animal.

"2. The average quantity of sugar existing in the liver at this time is at least 2½ parts per 1000.

"3. The liver sugar thus found does not belong to the arterial blood with which the organ is supplied, but is a normal ingredient of the hepatic tissue."

These results, and the results previously quoted, demand a careful reconsideration of the position hitherto accepted. It certainly seems from them that we can no longer avoid the acceptance of Bernard's theory of sugar formation in the liver. There may be some source of fallacy, and the extremely varied results which able observers, experimenting on this subject, have obtained, make us fully alive to such a possibility. As far as we can judge, however, the experiments are worthy of acceptance, and once accepted they restore Bernard's theory. Pavy's experiments in fact only pointed out that the abundant presence of sugar in the liver after death was not a condition of life; whether it was an increase of matter already existing in small quantity or a substance entirely of post-mortem production was not shown. The amyloid matter or glycogen admitted to be present in the liver had at the end of Pavy's researches no apparent destination. The results were destructive rather than constructive in their character. Hence they have always failed to satisfy some students anxious to probe the mystery more deeply, and have probably suggested more experimental investigation than most other observations that we possess. From time to time the pendulum of medical opinion has oscillated between Pavy and Bernard, but altogether has mainly inclined towards the former. These results of American investigation once more change the direction. There can be now little room for doubting that the liver does form a small but appreciable amount of sugar, and passes it constantly into the blood in animals fed on purely albuminous diet. The hypothesis originally propounded by Bernard, and advocated by Dr. Flint, appears, after all, to be nearest to the truth.

The destination of this sugar in the economy is the next problem to be solved. On this point we have no data before us. There are many facts which would point to its combustion in the lungs and tissues generally, but the direct evidence is still wanting. Speculations on such questions can lead to little good, and, as they frequently prove productive of much ill, we prefer to leave this part of our subject with the bare statement of the actual position in which recent experiments place us.

Experiments on the artificial production of glycosuria have always formed a most interesting part of the history of our knowledge of diabetes. Bernard originally pointed out that any hyper-activity of the liver circulation was associated with an increase in the amount of sugar formed. From a series of experiments he concluded that the vessels of the liver were influenced by impressions made on the pulmonary terminations of the vagi, by them conducted to the medulla, and thence reflected to the liver along the splanchnic nerves. The line by which the impressions were conducted from the medulla to the splanchnics was, however, not quite clear, and the awkward fact remained that after irritation of the roots of the vagi had produced glycosuria section of the splanchnics did not remove it. Several observers have added to our knowledge by operating on the cervical and dorsal ganglia of the sympathetic. Pavy produced glycosuria by section of the superior cervical ganglion, Eckhart by section of the last cervical or any dorsal ganglion, while section of the splanchnic nerves did not produce it. The explanation offered by this last observer was that exposure of the cut surfaces of the ganglia to the air caused irritation, and not paralysis. Cyon (*vide* 'British Med. Journal,' Dec. 23, 1871) has shown that this explanation is insufficient, and by successive steps has traced the line of conveyance of the nerve influence which governs the liver circulation to the filaments which pass from the cord along the vertebral artery to the last cervical ganglion. Pavy, it should be stated, had previously found that the section of these nerves produced glycosuria. These nerves in their downward course from the last cervical to the first thoracic ganglion enclose the subclavian artery, forming the annulus of Vieussens; in this ring the nerves of the hepatic artery are contained, and their division causes the vessel to dilate, and at the same time produces glycosuria. Section of the splanchnics had no effect on this condition of the urine, unless performed before the division of the annulus of Vieussens. Cyon's explanation is this. Section of the splanchnics or gangliated cord causes dilatation of the vessels of the intestines, and allows the blood to accumulate in them to such an extent that when the hepatic vessels are paralysed by an after operation the liver circulation cannot be increased. On the other hand, when the liver circulation is increased first, the relaxation of the intestinal vessels does not materially divert the current. These experiments render Bernard's hypothesis that glycosuria and possibly diabetes depend on increased circulation in the liver highly probable; at the same time they strike a serious blow at Pavy's and Schiff's hypothesis of a ferment in the blood being the cause of this conversion of the glycogen. Schiff has shown that this ferment does not exist in the blood normally, but is produced by any disturbance of the circulation, such as that produced by section of the nerves mentioned. If this were true the section of the splanchnics ought to favour instead of prevent the development of glycosuria, by allowing the ferment to make its appearance in the blood circulating in the intestines and on its way to the portal vein. We have seen that the contrary is the fact.

Turning now to the clinical aspect of our subject, we find a noteworthy tendency on the part of the authors under notice to accept the glycogenic theory in some form. Dr. W. Richardson, who is evidently a disciple of Bouchardat, accepts the theory in part, and regards diabetes as a result of a too rapid transformation of starch in the stomach and intestines, or of over-production of sugar by the liver. In the first class of cases the rapid change of starch into sugar causes the liver to receive more sugar than it can fix as glycogen, and the surplus consequently passes on into the general circulation to be eliminated by the kidneys. This form of the disease, which would depend really on an insufficient action of the liver, can only occur when the sugar is entirely derived from starchy food. In addition to the assumption on which it rests, it demands also the occurrence of a too rapid absorption of the sugar, and entirely overlooks the fact that much sugar disappears in the intestinal canal by conversion into lactic acid.

Donkin refers diabetes to the liver, and somewhat naïvely remarks, "It has often occurred to me that diabetes may possibly depend on perverted functional action of the liver-cells, whereby they morbidly secrete diabetic sugar instead of glycogen, their normal secretion." In the original this *profound* reflection is printed in italics.

Jaccoud, however, is unwilling to accept the hepatic theory. The results of Pavy's experiments have influenced him against it, although he admits that it more nearly solves the difficulty than any other. After summing up all the evidence against Bernard's views, the French clinician proceeds to expound his own notion of the disease. Diabetes he holds to be the development of a new operation in the body. Believing in a ferment as the active agent in changing the glycogen into sugar after death, he agrees with Schiff that this ferment does not exist in health, but can always be produced by disturbance of the circulation. The sugar production which this ferment excites is altogether a pathological phenomenon, foreign to the healthy economy, and not, as others hold, simply an exaggeration of a healthy function. Schiff limits this sugar production to the liver, Jaccoud does not. He believes that every tissue (and they are many) containing glycogen, or zoamyline as he calls it after Rouget, is robbed of this substance by the ferment, and thus the formation of sugar becomes a process of the whole body, and not of a single gland. Diabetes is a disorder of nutrition, a *dystrophia*. The saccharine state of the urine, which results either from excessive absorption of sugar in the intestine or from defective transformation of sugar into glycogen, he calls glycosuria. True diabetes is, on the other hand, the expression of the formation of sugar at the expense of the tissues containing glycogen, and is a constant de-nutrition of these tissues. Such are the conclusions arrived at in this excellent essay.

The experiments we have referred to in the early part of this article must, in our opinion, bring about a modification of Jaccoud's position, which rests mainly on the experiments of Pavy. The weak point of the whole fabric appears to us to consist in the wild assumption of the existence of a ferment. To drag in this imaginary substance as a kind of *deus ex machinâ* to solve all the difficulties which remain is unworthy of science. While physiologists and physicians lack the courage to state barely their results, and yield to a weak anxiety to make every theory *teres atque rotundus*, such speculations will continue to work evil. The time has fully come when we must face the unknown without such idols.

There is another theory of diabetes on which Jaccoud touches slightly, and which has shown evident signs of vitality of late, we mean the respiratory theory. If the sugar formation in the liver is accepted, the destination of this sugar becomes the next question, and the appearance of sugar in the urine may depend on either increased formation or defective consumption. The hypothesis that the sugar is burnt off in the lungs has always been a favorite idea with many, and some recent researches of Pettenkofer and Voit tend to strengthen it. These observers have shown that a diabetic, while consuming more food than a healthy man, not only absorbs considerably less oxygen, but gives off much less carbonic acid. With more combustible matter there is less combustion. They refer the disease to a faulty relation between the amount of sugar formed and the quantity of oxygen absorbed. Huppert, who has also worked in the same direction, agrees with them in attributing this defect to a peculiarity of the blood-corpuscles in diabetes, which have lost their power of fixing oxygen. Hence the defective oxidation which constitutes the disease. These views have a strong speculative flavour, but nevertheless there has been of late a steady increase in the number of facts which point to a state of suboxidation in diabetes. The low body temperature in diabetics strongly suggests such a state. This is more marked in the later stages of the malady, and may be used, as Jaccoud has remarked and we can confirm, to indicate the more advanced period of the malady. This low temperature, associated, as it is, with rapid tissue wasting, is a phenomenon worthy of the most attentive study. That it is not directly due to the non-combustion of the sugar may be inferred from the fact that the amount of sugar excreted bears no constant relation to the body heat.

Cantani, who has recently proposed the use of lactic acid in diabetes, holds that diabetic sugar, or paraglucose as he terms it, is a non-combustible element, with which oxygen cannot combine, and that consequently all combustion is effected at the expense of the albuminates and fats. The oxygen finds no material wherewith to combine in the lungs, hence the low temperature. Cantani says that respiration in diabetics is very infrequent, and cites this in favour of his view. We have not observed this infrequency, and when we have tested the temperature of the expired air in diabetics we have not discovered any difference between it and that expired in health. With defective oxidation in the lungs it is probable that a difference would show itself. Cantani proposes lactic acid as a remedy to supply a combustible element and save the albuminates and fats. The results of this treatment have yet to be fully tried; the proposer speaks of no slight success, which, if confirmed, will strongly support the respiratory theory.

The suboxidation is most probably not confined to the lungs, but is distributed to all the tissues, and if Huppert's hypothesis of the defective fixing powers of the blood-corpuscles prove true, we have a ready explanation of this state, and of the ill success which has hitherto attended the inhalation of oxygen. Whether the sugar formed by the liver is burnt off in the lungs, or in the tissues generally, is at present open to doubt; it is, however, highly probable, as Bernard himself thinks, that much glycogen remains unconverted into sugar, but is carried away in some combination in the blood. The destination of this we do not know; it is probably stored up in many tissues, hence the general distribution of glycogen in the body, and it may be converted into force by oxidation, and thus give rise to the appearance of lactic acid. The two theories, the hepatic and the respiratory, are so closely connected with our present knowledge of the phenomena of diabetes that in our opinion it is only by a comprehensive application of both that we can gain any clear notion of the disease. To regard it as a deviation from a normal process is, at all events, more philosophical than to adopt with Jaccoud the hypothesis of a ferment, and to describe the disease as the development of a wholly *new* process in the economy. Some theory is almost essential to give interest and method to the work of the clinical observer. Theories are only bad when they cease to be the servants of facts and become their masters. Kept in their proper places they encourage good work, and the two theories mentioned above seem at present to indicate the most fruitful direction for the clinical study of saccharine diabetes.

In the books before us there is a good deal of diversity of opinion as to treatment. Dr. Richardson, who has himself suffered from diabetes in its milder form for many years, has entered on this part of the subject with much earnestness. His remarks on treatment will repay perusal. He very properly insists on the gradual instead of the sudden substitution of restricted for general diet, and points out how often a sudden change is productive of ill effects. In his own case, likewise, he shows that, in the milder form of the malady, the best results will occasionally follow the relaxation of the strict rules of diet. Both he and Jaccoud strongly urge the importance of following Bouchardat's advice, and making daily exercise in the open air part of the treatment. To this Richardson mainly attributes his own recovery, and he quotes other cases in support of its value. The exercise should be regular and always stop short of fatigue. With reference to the use of alcohol there is a variety of opinion. The French author recommends Burton bitter ale, Donkin considers alcohol inadmissible, while

Richardson believes small quantities of brandy, or sherry, or claret, useful in some forms of the disease, and considers their effects constitute a good diagnostic test of the state of the liver function. When the liver function is over-active, alcohol, he thinks, increases the sugar; when the disease depends on a want of power in the gland to convert amylaceous compounds into glycogen, then he looks upon it as useful. Warm baths are strongly advised, and on their use, no doubt, much of the success obtained at Continental spas depends. The waters of Vichy and Carlsbad are recommended, and their alkaline characters referred to as the probable explanation of their good effects. It is remarkable that alkalies administered medicinally have so little control over the disorder. Over and over again their use has been tried, and in our experience little or no diminution in the sugar excreted has followed. We cannot help thinking, therefore, that the regimen and general treatment pursued at such places as Vichy have more to do with the results than the reaction of the waters. The patients get good in some way, and there is no reason to apply Pliny's sarcasm, *Medici qui diverticulis aquarum ægrotos fallunt*, to those who send diabetics to these spas.

With reference to drugs, opium, to our surprise, meets no great support. Richardson says, "I tried it on myself in large doses, as I have tried almost every medicine that has been recommended with any authority, but found mischief from it. The urine was reduced from 76 to 32 ounces; the specific gravity was 1054 instead of 1036; on boiling with Liq. Potassæ the urine became very dark; I felt so ill and heartless that I had not the resolution to make out the quantity of sugar it contained." This evidence is in striking contrast to much that has been published as to the good effects of the drug. In some cases it does much good, as we can testify, but in others it acts as described above, and while diminishing the amount of water affects the sugar much less. In some advanced cases its good effects are often due, in our opinion, more to its power of checking excessive elimination of urea than to lessening the sugar. Pavy's success in its administration has been confirmed recently by independent observation on the Continent. In Jaccoud's experience the two drugs which are most valuable are strychnia and arsenic. The former he thinks keeps up the activity of the digestive process, diminishes the polyuria, and lessens the sugar. In two cases he has seen the sugar disappear under its use. Arsenic has succeeded in some cases where strychnia has failed. Dr. Donkin's book is written to advocate a special treatment, and therefore contains little on the general therapeutics of the disease. By the employment of skim milk he argues that "we can administer to a diabetic both an albuminous and saccharine proximate principle of food. The lactin appears to undergo conversion in the presence of the casein into lactic acid, and thus skim milk becomes a food containing a quality of albumen in the highest degree capable of assimilation, with the addition of a saccharine principle, which is also assimilated." There is no doubt that Dr. Donkin has done good service by his advocacy of this treatment and by the careful rules he has laid down for its application, but we could wish that his proofs of its success were more satisfactory. He gives seven cases in his book, which are none of them as carefully recorded as such cases should be. In no single instance is there a regular quantitative analysis of the sugar given, but the quantity of water passed, its specific gravity, and an occasional appeal to Liq. Potassæ or Trommer's test are alone relied on. Now, we hold that no case of diabetes is recorded scientifically in which there is not a daily, or at least a bi-weekly quantitative analysis of the urine. As a preliminary to any treatment, indeed, the quantity of sugar excreted under mixed and restricted diet should be determined. We are sorry that none of these inquiries were made in the seven cases recorded. That the skim milk treatment is valuable, and possibly more valuable than restricted diet in the early stage of the malady, the first two recorded cases indicate. Dr. Donkin has also shown that skim milk is preferable to new milk. How far this may depend on the conversion of the lactin more readily into lactic acid is a very interesting question. In this there is probably a common point between Cantani's method and the skim milk treatment. The Italian gives his patients nothing but animal food and lactic acid (150 to 250 grains a day), and his practice appears to be eminently successful.

Dr. Balfour has recently tried this plan with a success equal at least to the skim milk treatment. His cases are, however, incomplete, and a much more minute study of the effects of the acid on the composition of the urine must be forthcoming before the true position of the remedy can be ascertained. We trust that ere long such data may be laid before the profession, and that the vaunted effects of lactic acid and skim milk may be established on a satisfactory basis.

XII.—Dr. Allbutt on the Use of the Ophthalmoscope in Diseases of the Nervous System.[1]

THE ophthalmoscope has for some years been making a place for itself in medicine as distinguished from surgery. As long as anything has been known of diseases of the brain it has also been known that troubles and loss of vision were among their consequences, and soon after the discovery of the connection between albuminuria and renal disease amaurosis was noticed as an occasional attendant symptom; but physicians were slow to see that not only might the mode in which sight came to be lost be ascertained by means of the ophthalmoscope, but that the process by which it was lost might afford important revelations respecting the original disease to which the loss was due. Naturally the first steps in the use of the ophthalmoscope in the diagnosis of diseases of the nervous system, and in the investigation of the effect of certain constitutional conditions on the structures of the eye, were made by ophthalmic surgeons, by Sichel, Gräfe, Liebreich and others in France and Germany, and a little later by Hutchinson, Hulke, Carter and others in this country. After a time, however, the ophthalmoscope was taken up by physicians, Germany and France having preceded us again here; but, at length, an English work has appeared which will go far to place us on a level with our Continental brethren, and will certainly give an impetus to the employment of this instrument here. Papers have been written from time to time by Dr. Hughlings Jackson and by Dr. Allbutt, which have already compelled attention to the subject, and it is to the latter of these physicians that we owe the book we now welcome. It has merits, too, in addition to its being the first work in a new and interesting department of medicine. It is one of those books into which the author throws himself body and soul, giving token of what he is and of all he has in him.

This infusion of character is something quite independent of the intrinsic value of the work as a contribution to science, or of its subject-matter, and constitutes a charm which might redeem from dulness a book which had no other merit, and which lends attractions to the most profound research, or the most lucid exposition of principle. Too frequently, and especially in medical writing, the only apparent relationship between an author and his book is the asserted paternity; as far as any likeness or resemblance is concerned, it might just as well be the offspring of X, Y, or Z; all the likeness is on the maternal side,

[1] *On the Use of the Ophthalmoscope in Diseases of the Nervous System and of the Kidneys.* By T. CLIFFORD ALLBUTT, M.D.

as we may call the circumstances—the special opportunities or necessities—which brought it forth. The very contrary is the case here; the embodiment of the author's personality is a feature which is prominent throughout. The preface, the introductory chapter, and last, not least, the dedicatory letter to Dr. Hughlings Jackson, so honorable to both, would alone make the book noteworthy. Such generous appreciation, on the author's part, of merit in one who might be said to divide with him his chosen field of work, and so frank an acknowledgment of obligation, are not common. We are equally struck with the enthusiasm and honesty of purpose in the search for truth which are everywhere displayed; accompanied, perhaps, with too great an impatience of the errors in method and of fact which have come down to us from our forefathers, but associated also with a philosophical breadth of view which not only immediately widens the horizon of the careful reader, but prepares him to attach additional importance to results obtained under such inspiration. We must add that the elegance and felicity of expression found throughout this book are rare in medical literature, and indicate a degree of culture not attained by the average man of any profession. We think, perhaps, that literary instincts or an exuberant fancy have sometimes been allowed too liberal exercise; for example, in the opening of the chapter on the normal optic nerve and retina we have a series of metaphors, more or less appropriate, in illustration of deviations from health, which reminds us of Sancho Panza's strings of proverbs, and we confess to having experienced some irritation when we came to the final address to the 'kind reader.' It was not for the courteous and courtly old patron of former days that this book was written, and it is something like affectation to apostrophise him. Surely Dr. Allbutt has sufficient faith in himself and in his generation to feel assured that there are earnest workers who can appreciate honest work, men who know the ring of true metal, and before whom he is ready to throw down his talent. Readers he will have who may not, perhaps, think of doffing their hats as they express a difference of opinion, but who are keenly interested in any new application of scientific method to medical investigation, and are ready to become followers also, when they see a path opened to a new region of knowledge; these are his constituents, and to them we hope he will appeal in future editions and in the new work foreshadowed in this.

Coming now to our subject, the first question which arises is:—What is the use and place of the ophthalmoscope in medical investigation? It is something different and something higher than that of the laryngoscope or endoscope, which simply reveal local conditions and facilitate local applications. It is this and more already in the hands of the ophthalmic surgeon, who by its aid sees his way clearly to a definite and exact diagnosis among the complicated conditions present in the eye. We are also disposed to give it a higher place than the sphygmograph, one reason being that it will come into more general use. For the sphygmograph is a costly instrument, its application demands the expenditure of much time, not only in the acquirement of skill in its use, but in making individual observations; the results or tracings obtained, moreover, differ greatly in value in different hands; so much depending on the exact adjustment of pressure and on individual aptitude in the use of the instrument. After all, too—and we are not among those who depreciate the sphygmograph, it reveals little at the bedside which the educated finger cannot detect, so that Dr. Sanderson actually found a nomenclature for its indications ready made by the older physicians. Now, the ophthalmoscope tells us something new, something we could not otherwise get at, and that which assists not only in the solution of physiological and pathological problems, but is of immediate value in diagnosis and prognosis; while lastly as an instrument it is inexpensive, and is prompt in its revelations. It cannot, on the other hand, be compared with the thermometer, either in the range or in the value of its indications; nor can it compete with the stethoscope, or rather with the complete physical examination of which auscultation forms a part. The information it affords is neither so extensive, nor so direct, nor so complete. It is restricted chiefly to the state of the intra-cranial circulation, of which the vascular system of the optic nerve and retina is a sort of offshoot; and even on this point it is not infallible, for when positive facts are obtained, their interpretation is not always easy, and the absence of change in the retina and disc is not unequivocal evidence of a normal condition of the cerebral circulation. Experience will, however, render inference more certain, and is almost daily filling up gaps in the knowledge afforded by this method of investigation, while the negative evidence is gradually being explained, and thus made to yield information of a positive kind.

Already the light thrown upon cerebral diseases by the ophthalmoscope is astonishing. While it was unknown that changes in the retina and optic nerve, secondary to, or in continuity with changes in the membranes or substance of the brain, or consequent upon variations in the intra-cranial blood pressure, were a common cause of the loss of vision often met with in cerebral disease, the utmost confusion necessarily prevailed in the diagnosis of the conditions to which this loss was due. At one time a tumour was found in a situation where it might possibly exercise pressure on the optic tract or ganglia; at another, by no ingenuity could such pressure be predicated, and some vague transcendental reason had to be invented, or the sympathetic nervous system was invoked. The result was a disheartening uncertainty. Now the optic neuritis or ischæmia, or the direct or consecutive atrophy, is watched from hour to hour; and if the exact situation of the intra-cranial disease is not thereby indicated, at any rate false conclusions as to this point are avoided; and frequently the nature of the morbid change, which is of more importance than its seat, is revealed. More than half of Dr. Allbutt's work is devoted to the consideration of the ophthalmic signs of diseases of the nervous system, but the eye also affords evidence of certain constitutional conditions, curiously justifying Sir Thomas Watson's classical illustration of morbid processes by reference to what is seen of them in this organ. Among them the most interesting and important is the condition dependent on, or at least associated with, renal disease. But there are other affections, such as leukæmia, diabetes, lead poisoning, &c., which give rise to optic changes. Dr. Allbutt, however, looks not merely to the direct and immediate advantages of increased accuracy of diagnosis, and more extensive knowledge of morbid processes to be obtained by the use of the ophthalmoscope; but takes a comprehensive and almost enthusiastic view of the improvement, both in the method and in the spirit of research,—more especially of research into diseased conditions of the nervous system which must result. The ophthalmoscope has for him this great charm, "that its use must favour a spirit of industrious and accurate observation, and must favour also that wholesome disposition of the mind which tolerates any facts, however far away they may seem to be from traditional doctrines or dignified theories." There can be no doubt that a new instrument of precision, by opening a new avenue to knowledge, does stimulate research and improve method. Possibly, however, Dr. Allbutt attributes too much to the instrument and too little to the pre-existing mental attitude, and applies too directly and immediately the philosophy which makes intellectual improvement consist in adjustment of the mind to new opportunities of acquiring knowledge.

A prominent characteristic of the work before us is, that the author is not content simply to ascertain and state the relation between certain cerebral or constitutional diseases, and certain appearances in the fundus of the eye; but endeavours in all cases to trace the mode of causation of the symptoms he describes. In this, however, he is not singular; the sort of necessity (most felt, apparently, by minds least capable of giving or of estimating it when given) for a reason for everything is universally recognised and very freely met by plausible guesses or vague speculations; it is in the fact that these find no place in his pages, and that anatomical data constitute the sole ground of the explanations given, that he is honorably distinguished. But we are reminded here, again, that there is anatomy and anatomy, and that by some an elaborate description of the distribution of the sympathetic nerve is considered proof of the uterine origin of hysteria, and explanation of all the varied sensations and symptoms described under this term. This is not what Dr. Allbutt would mean by anatomical data. The position he takes up is that the implication of the optic nerve in cerebral diseases is to be accounted for, not by any transcendental considerations, but by its rich vascularity, its large share of connective tissue, and its extensive relations with the parts at the base of the encephalon. We naturally find in the chapter on the aspect, structure, and connections of the normal optic nerve and retina the basis for this conclusion. There are several points in the structure and relations of the optic nerve which call for attention from their bearing on the pathology of the nerve and retina. Perhaps the most important of these is the comparative independence one of the other, in their vascular supply, of the nerve and retina. The central artery and vein of the retina lie, it is true, among the fibres of the nerve, but they distribute no branches to it. The vessels of the nerve are derived mainly from the pia mater, and they ramify between the nerve-tubules, and supply them with blood up to the disc, the vascularity of which forms part of the system, though there is here in the disc itself a network of vessels connecting together the arteries of the nerve, the central artery of the retina, the ciliary arteries, and the capillary coat of the choroid. The disc is thus more directly connected with the vascular system of the cerebrum than the retina, which is supplied by the ophthalmic artery, and may consequently be expected to afford a better index of the condition of the cerebral circulation. Another important feature in the structure of the optic nerve is the amount of connective tissue in which its fibres are imbedded, though we fail to see in this anything more than a difference in degree, and do not understand how it constitutes the marked peculiarity it is made to appear by Dr. Allbutt. The unyielding margin of the aperture in the sclerotic, by which the nerve enters the eye, again appears to play an important part in the production of some of the morbid appearances presented by the disc, as will be seen later. A kind of lymph space is described as existing between an outer and inner sheath of the nerve, but it has not as yet been shown to have any pathological interest.

The plan of Dr. Allbutt's work is, after giving directions for the examination of the eye, and describing the normal appearances, along with the anomalies sometimes presented by the disc and retina in health, to consider the variations from health observed, together with their causes, and subsequently to treat *seriatim* the ophthalmic signs of intra-cranial, spinal, renal disease, &c. It is in the description and in the examination as to the causation of the departures from the normal condition of the disc and retina that the practical interest of the book begins. The healthy appearances are taken as the standard of reference, and hyperæmia, anæmia, œdema of the disc, ischæmia, neuro-retinitis, chronic optic neuritis, retinitis, peri-neuritis, consecutive atrophy, and primary atrophy, are successively described.

A primary and fundamental point to be determined for the due estimation of the ophthalmic signs of intra-cranial disease is, whether the distinction between the 'choked disc' ('stauungs papilla' of German writers),—the ischæmia of the disc of Dr. Allbutt, and optic neuritis proper, is real and valid, and, again, whether these two conditions, if actually distinct, can be distinguished by means of the ophthalmoscope. As to the first question, there can be doubt whatever. There is a congested, swollen, almost strangulated, condition of the disc, which is not primarily neuritis, and which arises from obstructed venous return, and there is a neuritis which descends by continuity of structures along the nerve from the meninges of the base of the brain. The distinction was apparently first pointed out by Gräfe; it is now generally recognised by ophthalmic surgeons, and careful microscopic examinations have established it beyond the reach of question. A drawing of the interior of an eye after death by Dr. Fitzgerald, from a case under the care of Mr. Swanzy, accompanying the description of the frontispiece, which is a coloured plate of the ophthalmoscopic appearances presented by the same eye during life, furnishes an effective and impressive demonstration of the fact; the coloured plate shows in an extreme degree the changes in the disc usually attributed to inflammation, and in the drawing it is seen projecting into the back of the eye like a small nipple, while beneath is the simple statement that there was no neuritis extending up the optic nerve. Are the two conditions, then, distinguishable one from the other, so that the different pathological changes can be made use of in diagnosis? We think this question also must be answered affirmatively. The reply of our author is unhesitating, but it must not be supposed that the task is an easy one. Ophthalmic surgeons of undoubted skill, and perfectly intimate with the use of the ophthalmoscope, hesitate to give an unqualified adhesion to Dr. Allbutt's statement of the diagnostic differences between ischæmia and neuritis; and unless it is quite recently, it has been obvious that

Dr. Hughlings Jackson has not been clear upon the point. It is well known that Dr. Jackson has long been engaged in the investigation of the ophthalmic signs of cerebral disease; he has unequalled opportunities; his enthusiasm, assiduity, patience, and skill, are not to be surpassed by any one. And if he does not recognise the different appearance of the choked disc and inflamed nerve, the difference cannot be very marked. Possibly his conscientious care in making sure of every step as he proceeds, his extreme regard for observation and extreme caution in inference—qualities which are simply invaluable—have here, as in other instances, allowed others to get the start of him.

The importance of the differential diagnosis between ischæmia and neuritis will be seen, when the causes to which they are respectively due are considered; the differential characters, as given by Dr. Allbutt, are the following:—While in both conditions there is swelling and projection of the disc, it is more considerable and more abrupt in ischæmia, and presents a steep elevation on one side, which is peculiar. The colour of the part is different; in ischæmia there is a circumscribed intense redness or brownish-grey, in neuritis a reddish-lilac or grey tint, which diffuses itself more widely over the surrounding retina, and is more uniform and opaque, while there is exudation by which even the large retinal veins are often concealed dipping in and out of it. In both ischæmia and neuritis the retinal arteries become thin and indistinct, and the veins enlarged and tortuous, but in ischæmia there are many more minute branches to be seen in the disc, which becomes extremely vascular, and presents a 'mossy' appearance, while in neuritis the term 'woolly' is considered to convey a better idea. Minute hæmorrhages are common in both, and the outline of the disc is lost in both. These are differences which ought to be appreciable by any moderately competent observer when present in a well-marked degree; but very frequently the same intracranial condition gives rise simultaneously to obstruction to the return of venous blood and to true primary inflammation of the nerve. One point must be mentioned which has struck all observers with surprise, that is the remarkably slight interference with vision in even extreme ischæmia of the disc.

We have still to enumerate the causes of papillary ischæmia and of true neuritis, and to show why this vascular offshoot from the cerebrum exhibits a degree of congestion not seen in the meninges themselves. The causes of ischæmia of the disc are all such as more or less directly distend the ophthalmic veins, of which (1) meningitis, (2) hydrocephalus, and (3) tumours, are the chief. Softening, acute or chronic, hæmorrhage, sclerosis, arterial degeneration, do not give rise to this condition. The explanation of the exaggerated congestion of the disc produced by intra-cranial pressure is very interesting. The sclerotic aperture or ring embraces the nerve at its entry quite closely. When, therefore, from obstruction to the return of blood by the ophthalmic vein, whether this be due to coagulation or stasis in the cavernous sinus, or to local pressure on the sinus and the entering veins by fluid in the third ventricle, or to a general increase of pressure within the cranium, the minute veins and capillaries in the nerve and disc tend to enlarge, but the resulting swelling is prevented at this particular point, and the ring forms an actual constriction. A sort of strangulation is thus produced, and the swelling and congestion of the disc are the result and evidence of this. As Gräfe expresses it, the sclerotic ring "plays the part of a multiplier" placed upon a vascular offshoot of the brain. We are tempted, however, notwithstanding the beauty of the explanation, to ask if it is really necessary. To deviate for a moment into the figurative language which Dr. Allbutt holds in such detestation, are we to suppose that nature neglected to provide for the contingency of intra-cranial pressure, and made the sclerotic ring too small, or was the utility of the condition as a sign of intra-cranial disease foreseen? Seriously, it seems to us that the ocular extremity of the optic nerve, being the only offshoot of the vascular system of the brain, which is not within the rigid walls of the cranio-spinal cavity, might, on mechanical principles, be expected to exhibit extreme congestion on the occurrence of intra-cranial pressure without the intervention of this strangulating ring, just as an india-rubber ball in communication with a cistern will be gradually distended to bursting as the water rises, or as hernia cerebri protrudes and bleeds. A tumour or fluid in the ventricles squeezes the blood *out of* the the meninges, and flattens the convolutions against the roof of the cranium; there being no counter pressure in the eye to compare with that of the cranial walls, the blood will be driven *into* the vessel of the disc. Whether the congestion which is obviously inevitable could reach the degree presented in ischæmia papillaris without the strangulating action of the sclerotic ring, we do not pretend to decide. Gräfe has discussed the question, and regards the strangulation as necessary, but he appears to have taken into consideration only the passive obstruction, such as would be caused by blocking of the cavernous sinus, or obstruction to return of blood by the ophthalmic vein, and not the active hydrostatic or, rather, hæmostatic pressure, of which we have been speaking.

The encephalic causes of neuro-retinitis are meningitis and encephalitis, the former being the more frequent of the two, and it is when the meningeal inflammation has the favorable conditions of contiguity, duration, and activity, as in syphilitic meningitis about the base of the brain, that it is most likely to involve the nerve and its ocular extremity, the disc. If it is remote from the optic tracts and nerves, or if brief in duration, though contiguous, this symptom does not usually appear.

We postpone certain remarks which occur to us till we come to consider the application of ischæmia and neuritis as signs of cerebral disease, but we are tempted to try a fall with the author on the general question of inflammation, into which he enters with a certain vehemence obviously arising out of strong convictions on the subject. We think there are few of his readers who do not recognise with him the distinction between congestion and inflammation, and we do not propose to bring in 'nature' again, the personage of the 'female gender' (would not either sex or feminine have been more appropriate?) for whom he has such a contempt. We question the adequacy and the accuracy of his idea of inflammation, which he defines as "*lesion with reaction or resistance.*" We want a definition of lesion, and we want an explanation of reaction. If, as the context implies, lesion is taken to mean rupture of continuity, then we say that in erysipelas and similar inflammations in which the disturbance of the relation between blood and tissue is on the side of the blood, inflammation is antecedent to lesion; so also in herpes zoster, where the disturbance comes through nerves. Reaction or resistance, again, unless resolved into its factors, unless the different steps of the process are stated, is just as much a figurative term as the old 'effort of nature,' borrowed, it is true, from physical or mechanical science, instead of being a metaphysical abstraction, but none the less likely to become an obstructive idolon. For ourselves, inflammation is a condition which is produced whenever the normal relations between blood and tissue are deranged beyond a certain varying and undefined limit, whether the derangement start in an altered condition of the blood, or in damage to the structure, or be initiated by deviation from the normal nerve-influence (let this last expression be allowed us in the absence of opportunity better to define it). The immediate result we believe to be increased local oxidation, with consequent increased evolution of energy in some form or other. The tendency we may suppose would be to the production of the least specialised form of energy, heat; but whether this be the case or not a part of the excess of force is appropriated by the organic structures, and expended in the increased activity of the tissue changes; any local heat would represent unappropriated energy. The increase of the cohesive affinity between the blood-corpuscles and the capillary wall, leading to their extrusion, as shown by Cohnheim and explained by Norris of Birmingham, is probably the primary effect of this force; the phenomena of congestion and the proliferation of the tissue elements, secondary results. We cannot, of course, here justify the position taken up, we indicate it only to illustrate the inadequacy of the definition of inflammation given by the author. On another question of general pathology or morbid physiology we are thoroughly in accord with him, that is, on the current exaggeration of the influence of the sympathetic nervous system, fancies about which, as he says, are very fashionable; its mysterious agency is perpetually called upon to explain all sorts of phenomena, and the less the writer and the reader know about the sympathetic nervous system the more satisfactory, of course, is the explanation.

In the sections on hyperæmia and anæmia of the disc and retina, or œdema of the disc, these conditions are carefully described, and many valuable hints are given, more particularly with respect to the diagnosis of early stages of hyperæmia, in which the appearance of radiating vessels in the disc is to be looked for, and any difference of vascularity in the two eyes is to be noted as important. Directions are given by which anæmia may be distinguished from atrophy, and the causes of hyperæmia, anæmia, and œdema, are discussed. Chronic optic neuritis is a term introduced by Dr. Allbutt to indicate a condition scarcely less interesting than the more common acute neuritis. It constitutes an early stage of what has been called 'simple optic atrophy,' and is chiefly associated with sclerosis of the nerve centres. Hutchinson has described it as initiating the process which ends in the white atrophy of tobacco amaurosis. The disc is first too red and the choroid too full of blood, but there are no ecchymoses or effusions of lymph; the redness gradually fades, and eventually the discs become too white and the arteries almost disappear.

Consecutive atrophy, again, is particularly interesting. Dr. Hughlings Jackson has been led to make a distinction between atrophy in which the margin of the disc is ragged and the outline blurred, and atrophy in which the surface is brilliant and the edge sharp and even. The former he traces to antecedent neuritis, the latter he considers to be primary 'simple or progressive atrophy.' It is true that the atrophy with an uneven outline is a result of neuritis, but according to Dr. Allbutt's observations the shining smooth-edged condition is the final stage of all atrophies, whether these are primary or are consequent upon the chronic optic neuritis to which reference has just been made, or the results of acute neuritis or ischæmia. The intermediate stage he calls *transition atrophy,* a term which will be found very convenient. We cannot follow the author into the nice distinctions by which primary atrophy is characterised and differentiated from anæmia on the one hand and consecutive atrophy on the other. They are evidently the result of careful observation, and will repay careful study by all who are seeking to make use of the ophthalmoscope in medicine.

One or two points brought out prominently are deserving of something more than the casual mention made of them in passing. Of these, the first is the amazing degree of change often observed in the discs when central vision is so little affected that sight appears to be quite good to the patient and when tested by ordinary methods. As the author says, "To base any inferences as to states of the optic discs upon degrees of vision is simply a waste of time." It is one of the surprises awaiting those who take up the ophthalmoscope as an instrument of clinical research to find extreme ischæmia, or even a considerable degree of true neuritis or appearances characteristic of advanced atrophy, without any acknowledged affection of sight, and without any pain or photophobia. Another point is the advantage sometimes to be derived from mapping out the field of vision, a procedure which may betray an unsuspected failure of sight, and which distinguishes the general feebleness of the retina in anæmia from the local deficiency of functional power in atrophy. Again, the necessity of cultivating the direct method of examination as well as the indirect becomes more and more evident as the numerous instances in which it furnishes information not yielded by the indirect examination are successively adduced.

From the account of the variations from health of the optic nerve and retina we pass to the consideration of the ophthalmoscopic evidences of the intra-cranial disorders which form the subject of the longest and most important chapter in the work. They are taken in the following order, suggested simply by convenience—epilepsy, chorea, mania, dementia, meningitis, concussion and fracture, hydrocephalus, tumours and chronic periostitis, atheroma, softening and hæmorrhage, cerebritis, abscess and sclerosis, and general paralysis; illustrative cases to the number of 123 from the practice of the author and other observers, and statistical tables, forming an appendix.

In epilepsy, properly speaking, we have none of the violent changes in the optic disc, such as ischæmia, or neuritis, or atrophy. The result of careful and repeated examination of the fundus of the eye in epileptics is an opinion that during the intervals there is a higher degree of vascularity in the discs and retina, the discs being more red than normal and the vessels larger. During or immediately after the fits the disc has been found entirely anæmic in a majority of the few observations that have been made. Dr. Hughlings Jackson and Mr. Carter are among those who, with Dr. Allbutt, have had the rare opportunity, and have succeeded in the difficult task of examining the fundus of the eye during an epileptic attack. Out of six cases of which the author has notes, anæmia of the disc was present in three, in three others considerable hyperæmia, but in these latter the fits were succeeding each other in rapid succession, the patients not regaining consciousness in the interval. On the whole, therefore, the ophthalmoscopic evidence tends to confirm the view that the common epileptic seizure is due to spasm of the cerebral vessels; but we agree with the author that epilepsy has more than one cause, and may be associated with diverse conditions of the blood supply to the brain.

Chorea is not attended with any affection of the retina or disc. In mania symptomatic changes in the eye are found in a large proportion of cases, and there appears to be for a period after the paroxysm a pink suffusion of the fundus oculi; a single observation during a paroxysm showed the disc to be anæmic. Dementia, as the author observes, includes worn-out lunatics of all sorts, and nothing is to be gained by associating changes in the eye with this condition as such.

It will give some idea of the labour bestowed on investigations, the results of which are given in a few pages or almost in a few lines, when we state that in the appendix are tabulated 43 cases of epilepsy with insanity, 51 cases of mania, 38 of dementia, 17 of monomania and melancholia, and 12 of idiocy. These tables have already been printed in the 'Medico-Chirurgical Transactions.'

We come now to meningitis, in which the labours of the author and of Dr. Hughlings Jackson have rendered the ophthalmoscope a most valuable and important aid to diagnosis. It is here that the most important addition to our knowledge has been made, and if Dr. Allbutt has not been the only pioneer who has pushed on into untried ground at this point, he is the first, so far as we know, who has systematised the results and secured the newly acquired possessions; whatever else he may have done or may yet achieve, this is one advance with which his name must always be honorably associated. Meningitis has various origins, and is called, therefore, by various names. By far the most important is tubercular meningitis, common enough at all ages, but especially frequent in children; from its usual seat and characters it has been called granular basilar-meningitis. As the author says, the general opinion in the profession is that this disease is invariably fatal, an opinion qualified, perhaps, in the case of most men of considerable experience by a recollection of one or two exceptional recoveries. The rule is that patients who present well-marked symptoms of tubercular meningitis die, and when recovery occurs it is usually concluded that the diagnosis was erroneous, the old-rooted conviction that tubercular disease in any important organ is inevitably fatal overruling every other conclusion. Now, the position Dr. Allbutt takes is this: some at least of these recoveries, with or without damage to the brain, from symptoms characteristic of tubercular meningitis, are really what they seem to be; there has been tubercular meningitis, and the patient has got well. Still more commonly a child will have occasional 'purposeless' vomiting, evening feverishness and restless nights, with movements or even convulsions during sleep, will suffer at times from pain in the head, become irritable and unable to fix his attention, and will eventually regain health with or without some impairment of the mental faculties. Here again there has been, or may have been, tubercular meningitis and recovery. The grounds for this conclusion are as follows: in cases of tubercular meningitis, verified by post-mortem examination, the changes in the eye have been watched during life, and ischæmia of the disc, or neuritis, or both, have been observed;—the ischæmia caused by obstruction to the venous return, or by general intra-cranial pressure;—the neuritis due to extension along the nerve of the inflammatory process going on in the pia mater enveloping it. In cases presenting almost the same train of symptoms, but which have not terminated fatally, the optic ischæmia or neuritis has been equally observed, and it can scarcely have any other than the same cause. But not only in acute, but also in those milder cases which medical men have not ventured to attribute to tubercular meningitis, optic changes of the same kind have been watched in a considerable number of instances, and one case is related by Dr. Allbutt in which, after recovery from a first attack, the patient died in a second, and evidences were found of an old and of a recent meningitis. We should have been glad to have had more definite expectations held out to us that, in the tubercular meningitis of adults, the ophthalmoscope would afford a much-needed aid in diagnosis. There are no cases more perplexing than these; the antecedent history which is so suggestive in the child is often wanting later in life, and there is the utmost diversity in the mode of attack and in the march of the disease. In a certain number of cases, however, optic changes do not occur in the course of fatal meningitis. Bouchut states that he found them in all but two of fifty-nine cases, but Dr. Allbutt met with them in only twenty-nine out of thirty-eight; the absence of any affection of the disc is not, therefore, conclusive against the existence of meningitis, and when the inflammation is on the upper aspect of the hemispheres, away from the optic tracts and nerves, it is not to be expected, according to the author's explanation of the connection between the two, that they will be so liable to become involved. After recovery from tubercular meningitis either the mental powers or vision, or both, may be impaired, and a remarkable fact comes out in Dr. Allbutt's inquiries, namely, that idiocy and blindness are among the results which sometimes follow, as well as the stupidity and change of disposition coming over precocious children. Dr. Crichton Browne has not only no hesitation in classing tubercular meningitis among the causes of idiocy, but he traces insanity to this source also. This causation has only to be stated to be at once recognised, and examples occur to our mind both of idiocy, blindness, and, we may add, deafness, following tubercular meningitis; in two cases of deafness so caused, at the age of about six, the child, after having learnt to speak, became as completely mute as if the deafness had been congenital.

Meningitis occurring in fevers, erysipelas, pyæmia, &c., has not yet been so thoroughly worked out, but there can be no doubt that the ophthalmoscope will frequently be of service in determining whether violent delirium in a given case is due to meningitis or simply to the effect on the brain of the altered blood. Syphilitic meningitis, on the contrary, which is common, and from its chronic course offers a favorable opportunity of watching the optic changes, has been very fully observed. It is here that the true neuritis descendens is most commonly met with and best seen; it is here, too, that treatment meets with the greatest success.

In concussion of the brain and fracture of the skull we cannot expect to have any constant or valuable opthalmoscopic indications; the most interesting point is the fact that blows on the head and elsewhere often seem to determine the occurrence of local syphilitic mischief, an observation which entirely accords with our own experience. We must not pass from the subject without saying how greatly we admire Dr. Allbutt's self-denial in dismissing railway accidents as mere pitfalls for the inquirer. What traps for large fees would a few pompous words on the use of the ophthalmoscope in these important and perplexing cases have been!

With respect to intra-cranial tumours, in which a vast amount of work has been done by other observers, and which have, of course, been the object of most careful and patient observation by Dr. Allbutt, we find him honestly but regretfully confessing that he is unable to indicate with anything like finality the actual value of the presence or absence of optic changes in the diagnosis or exclusion of encephalic tumour, or to state with anything like certainty what are the intermediate processes which connect these changes in the head with inflammatory or congestive changes in the disc. The ophthalmoscope is, nevertheless, of great value in the diagnosis of tumours, and all the more valuable because the two physicians who have taken the lead in introducing it into medical practice in this country, the author and Dr. Hughlings Jackson, have displayed true scientific caution, and have not rushed along the broad path of hasty generalisation.

No fact is more certainly established than that intra-cranial tumours commonly give rise to changes in the optic disc. The mode in which these changes are brought about is a subject of hot controversy. We have above seen that Dr. Allbutt recognises two conditions of the disc, congestive and inflammatory, consequent upon cerebral disease, leaving out of consideration for the moment primary atrophy. The congestive or strangulated state of the disc is the more common result of tumour, but true neuritis also occurs. The question is, are these conditions induced by obstructed venous return on the one hand, and by extension by continuity of the inflammation along the nerve on the other, or are they not both due to general 'vascular storms,' in which the sympathetic nervous system is the agent? The latter view has the delightful vagueness and intangibility so attractive to some minds, but so hateful to such as seek to see clearly and know exactly. Benedikt is the champion of it selected for attack by our author, who is mistaken, as we think, in saying that it is adopted by Dr. Hughlings Jackson, whose attitude has seemed to us to be rather one of dissatisfaction with other explanations than of adhesion to the sympathetic hypothesis as an adequate solution of the problem.

We are not concerned to discuss here the agency of the sympathetic system in the production of the ophthalmic changes; it is admissible only on the supposition that hyperæmia is identical with inflammation, which is not the fact, and to admit it would only shift the difficulty to other ground without in anywise diminishing it. What we have to do is seriously to inquire whether these changes can be explained by the intervention of ischæmia or neuritis, the result of pressure, direct or indirect, or of extension of inflammation by continuity of structure; or if, as must be acknowledged, this explanation is not always tenable, whether it is valid in so many cases that we are justified in exercising a degree of scientific faith in it when it is not obvious to the understanding. We are indebted to Benedikt for putting the difficulties clearly and forcibly, if not quite fairly, and they resolve themselves into the following points:—1. That in intra-cranial tumour, associated with changes in the optic disc, the tumour may be incompetent by its size or situation to give rise to general or local pressure or to venous obstruction, or to communicate inflammation to the optic tract or nerves. 2. That, the tumour constantly increasing in size, the changes in the disc may advance and recede in turns, with exacerbations and remissions in the headache and other general symptoms attributable to 'vascular storms.' 3. That tumours may attain a large size without giving rise to ophthalmic changes. To these it is replied—1. That the intra-cranial pressure giving rise to ischæmia of the disc may be not merely such as might be due to the bulk or seat of a tumour, but to the general vascular pressure of inflammation set up by the tumour, or to dropsy of the ventricles caused by it. 2. That the exacerbations and remissions are traceable to attacks of local or general inflammation, excited by a growing tumour, which subside. 3. That a slowly growing tumour may cut off the blood supply of neighbouring parts, which then fall into softening, or may cause Wallerian atrophy by separating parts from their functionally associated centre and thus make room for itself, and never cause increased pressure. Taking these explanations, and remembering that red softening, arterial degeneration, &c., are not attended with ophthalmic signs, we are disposed to exercise the required faith, even in cases such as those instanced by Benedikt, and in cases like one which came under our own observation, in which the only morbid appearances were three small dead and withered entozoa on the surface of the convolution above the posterior end of the fissure of Sylvius of the right hemisphere, the patient having suffered from unilateral convulsions, loss of sight with atrophy of the disc, and loss of smell.

Dr. Allbutt does not content himself with stating the degree of liability to optic changes attending tumours in different situations, but gives fully and carefully the symptoms to which tumours in various parts of the encephalon and of the cranial fossæ give rise. We cannot follow him in this—the general conclusion with respect to the use of the ophthalmoscope in the diagnosis of intra-cranial tumours is that, while changes in the disc are often valuable evidence of the presence of tumours, and sometimes especially valuable from their early appearance when other symptoms are indefinite, they afford no indication of the nature of the tumour and very little information as to its seat and size.

Recent cerebral hæmorrhage is attended with no important ophthalmic signs, but old clots sometimes give rise to neuritis and atrophy. The retinal changes caused by albuminuria, and the liability to cerebral hæmorrhage in renal disease, make an examination of the eye important, even in recent apoplexy. Embolism and red softening are not attended with significant changes in the eye; cerebritis, abscess, and sclerosis, on the other hand, very commonly set up, the two former neuritis, the last primary atrophy. General paralysis, which is due to diffuse sclerosis, is almost always accompanied by optic atrophy, as was shown by Dr. Allbutt in a paper read before the Medical and Chirurgical Society. In "Locomotor Ataxy," another disease due to sclerosis, it is about equally constant, and it is common in connection with paralysis agitans.

In locomotor ataxy which is due to sclerosis of the posterior columns of the cord, and in other diseases dependent on this kind of change in different parts of the nervous system, the optic atrophy is probably not consequent upon the disease in the cord, but is due to sclerosis of the tract or nerve, arising out of some general condition which is the common cause of the morbid changes in both cord and eye. But in spinal diseases arising from injury, which run a long course, changes in the optic disc are common, and these are hyperæmic and not primarily atrophic; they are, moreover, directly consequent upon the affection of the cord. The temptation to invoke the sympathetic system is here particularly strong, but the well-known indications of injury to this nerve are commonly absent when the disc is affected consecutively to spinal injury, and may be present without any change in the disc when the sympathetic is diseased or destroyed. The explanation Dr. Allbutt offers is that an ascending meningitis creeps upward from the cord to the base of the brain, and gives rise to hyperæmia or ischæmia of the optic discs in the same way as cerebral meningitis.

Next in importance, and almost equal in interest, to the optic changes accompanying diseases of the nervous system, are those associated with diseases of the kidneys. The vitreous body, the retina, and the choroid are all affected; but the most conspicuous change is the retinitis, which has a course and results so characteristic that it is at once recognised as albuminuric. The first stage is one of diffuse infiltration, the retina round the disc becomes swollen and purplish-red, the disc becomes suffused with a dark red colour, and its margin is no longer defined; the arteries are small, the veins large and tortuous, and an exudation spreads over the disc and surrounding retina. If the inflammation goes on, hæmorrhages occur in the retina, and certain white patches appear surrounding the disc and extending outwards upon the retina along the course of the vessels, which are probably patches of coagulated exudation, while other smaller stellate white spots, due to degeneration of Müller's rods, surround the yellow spot. Nothing can be more striking than this assemblage of white patches and hæmorrhages in various stages of retrogressive change around a disc which is itself no longer to be distinguished, except by the convergence of the vessels, together with the independent constellation of white dots about the yellow spot. The vision is usually seriously affected, but recovery may take place and nothing remain of all these appearances; a retinal anæmia and yellow patches in the choroid alone persisting. Many interesting questions arise with respect to this retinitis, which are fully and ably discussed by Dr. Allbutt. Is it peculiar to any form of renal disease? To this he answers, "No." It is most commonly associated with the contracted granular kidney, but has been met with in amyloid and other forms of large smooth kidney, and in the epithelial nephritis following scarlatina. We have ourselves recently seen the first stage of this retinitis well marked in a case of epithelial nephritis following exposure to cold, and of no long duration. Has it any relation with the cardiac hypertrophy also associated with renal disease? Here, again, we concur with the reply of our author—the two conditions certainly do not stand in any constant relation. In the most marked example of albuminuric retinal change which has come under our observation there was no hypertrophy of the heart. In one of the most striking cases of cardiac hypertrophy and high vascular pressure due to renal disease no retinitis or evidence of past retinitis existed. Is there, then, any necessary relation between the retinitis and nephritis at all? Most assuredly, and yet no satisfactory mode of causation of the one by the other can be made out; the most likely guess, as Dr. Allbutt puts it, being, however, that uræmic blood sets up the irritative changes in the retina. The temporary loss of sight from uræmia is, however, not attended with optic changes.

Leucæmia, diabetes, oxaluria, chronic poisoning by alcohol, tobacco, and lead, have been found to cause changes of various kinds in the retina and discs. Cases of this kind are, however, comparatively rare, and the more frequent and important, such as tobacco amaurosis and amaurosis from lead poisoning, are more likely to come under the notice of the ophthalmic surgeon than of the physician. The physician should be aware of their occasional occurrence, and they should not be lost sight of in the study of tissue changes associated with any constitutional condition or due to any particular poison. It should be stated that Dr. Allbutt accepts with considerable reserve, if at all, Hutchinson and Wordsworth's conclusions as to the causation of white atrophy by tobacco.

The final chapter is an essay on embolism of the Sylvian artery, in which the changes following embolism of the central artery of the retina are made use of to explain the consequences of the graver lesion. Into this we do not now enter, but conclude by expressing our sincere admiration of Dr. Allbutt's book. He has not only taught us the great use which the ophthalmoscope may have in medicine, and cleared up many of the difficulties which threw uncertainty upon its indications; he has, while considering the optic changes resulting from cerebral and constitutional diseases, furnished a vast amount of information respecting these diseases themselves, and he has, by the whole spirit and method of his work, given an impetus to the truly scientific pursuit of medicine.

Bibliographical Record.

Bastian on Lowest Organisms.[1]—Dr. Bastian is certainly an adroit controversialist, as well as an innovator. We remember, last year, a violent polemical discussion which was carried on between him and Professor Huxley. In the inception of the dispute the facts were all on the side of the Jermyn Street professor; but, owing to the skill of his antagonist and the peculiar style of argument into which Professor Huxley was often led, Dr. Bastian carried his lance fairly broken out of the arena.

The experiments of Pasteur and Pouchet have so long occupied the attention that it is unnecessary to quote them here. Dr. Bastian professes to have repeated with success the experiments of Pouchet, and to have entirely demolished the counter-experiments of Pasteur, Tyndall, and Huxley. We must glance at Dr. Bastian's experiments before we enter upon the consideration of his arguments. He gives us sixty-five experimental instances, in which he alleges that he has proved what he terms the theory of Archebiosis. Of those, the following, perhaps, best illustrate his method of proceeding:

"Fluid (in vacuo) in a flask, the neck of which was hermetically sealed by means of a blow-pipe flame during ebullition.

"No. XIII.—Urine in forty-four hours showed a very slight amount of sediment. During the next two days the sediment very slightly increased, but was still small in amount. At the expiration of fifteen days, no further increase in the turbidity having taken place, the fluid was examined. The vacuum was still partially preserved, as evidenced by the rapid inbending of a portion of the neck of the flash after it had been carefully made red hot. When opened, the odour of the fluid was stale, but not fœtid, and its reaction was still faintly acid. On microscopical examination bacteria and torulæ were found in tolerable abundance.

"No. XXIV.—Fluid in a bent-neck flask, having eight acute flexures. Urine, in forty-eight hours, showed no change. After twelve days there was still no general turbidity, though there was a slight flocculent deposit of an uncertain nature. Two days afterwards the flask was broken, when the odour of the fluid was still found to

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[1] *The Modes of Origin of Lowest Organisms, including a Discussion of the Experiments of M. Pasteur, and a Reply to some Statements by Professors Huxley and Tyndall.* By H. Charlton Bastian, M.A., M.D., F.R.S. London and New York. 1871.

resemble that of fresh urine, and its reaction was acid. The flocculi were made up of granular aggregations, in the midst of which were a few bodies closely resembling torulæ, though they were somewhat doubtful in nature; neither bacteria nor vibriones could be found. The flask, having a short open neck, was then replaced in the warm bath. In sixteen hours the whole fluid had become turbid, it was also slightly fœtid, and on microscopical examination it was found to be swarming with bacteria, vibriones, and leptothrix."

Against his conclusions may be cited the testimony of such an experienced observer as Dr. Sanderson, on whose researches on microzymes we commented in a past number. In a recent memoir in the 'Quarterly Journal of Microscopical Science' Dr. Sanderson gives a series of experiments which appear to contradict those of Dr. Bastian in nearly every essential respect. Dr. Sanderson shows, firstly, that neither bacteria nor fungi ever develope in solutions raised to the boiling-point, and placed in carefully cleansed and boiled vessels, which are subsequently closed; secondly, that if such solutions in such flasks be exposed to atmospheric air, no bacteria ever develope, but yeast-cells, and ultimately blue mould, do develope (whence it is inferred that the germs of fungi, but not of bacteria, are carried in the air); thirdly, that if unboiled water be used, or glass or other surface not duly cleansed be brought in contact with the above-mentioned solutions, bacteria always develope in great quantity (whence it is inferred that water and surfaces which have been or are more or less damp are the means of dissemination of bacteria). It is impossible to reconcile these statements with those of Dr. Bastian, and when two such skilful observers arrive at such diametrically opposite results, we, at least, must be pardoned for deferring our judgment.

Such, however, are the facts.

"Varius Sucronensis ait; Œmilius Scaurus negat, utri creditis quirites?"

The arguments and conclusions of Dr. Bastian rest upon an entirely distinct basis from his facts. He sums up, we think, the whole case in the following words:

"If fluids in vacuo (in hermetically sealed flasks) which were clear at first, have gradually become turbid, and if on microscopical examination this turbidity is found to be almost wholly due to the presence of bacteria or other organisms, then it would be sheer trifling gravely to discuss whether the organisms were living or dead, on the strength of the mere activity or languor of the movements which they may be seen to display. Can dead organizations multiply in a closed flask to such an extent as to make an originally clear fluid become quite turbid in the course of two or three days?

"In these experiments with heated fluids in closed flasks nothing is easier than to obtain negative results. The same kinds of infusion which, if care has been taken to obtain them strong enough, will in a few days teem with living organisms, often show no trace of living things after much longer periods; when the solutions are weak, again, those cases where only a few organisms exist in a solution which has been made the subject of experimentation, nothing is easier than, by a perfunctory examination of the fluid, to fail finding any of these sparsely distributed living organisms. Experiments, the results of which are positive, may, therefore, in the absence of sufficient care, be cited as negative; and experiments which would otherwise have been crowned with unmistakably positive results may be rendered wholly barren by the employment of infusions which have been carelessly made."

The nomenclature adopted by Dr. Bastian is very peculiar. The hideously ugly word "archebiosis" is coined to express an idea, which, when it is examined, is closely allied to that of heterogenesis. Dr. Bastian would probably not admit this fact.

The possible modes of origin of bacteria and torulæ may therefore be tabulated as follows:

Modes of origin of bacteria and torulæ	1. Homogenesis.	*a.* Direct.
		b. Indirect.
	2. Heterogenesis.	
	3. Archebiosis.	

We confess that we fail to see the logical distinction between these methods of origin. If the presence of organic matter is once admitted as a factor, it matters very little whether the organic matter in its individualised state is living or dead. Whether "particles of living matter" or "certain fluids containing organic matter" are the ambient medium in which organized beings are produced, is a mere question of words on which a wordy war might continue for years. The allegation by Dr. Sanderson that in solutions which have been raised to the boiling-point, and placed in carefully cleansed vessels, bacteria and fungi are not developed, is entirely destructive of Dr. Bastian's forty-fourth experiment. If neither bacteria nor fungi develope under the conditions which Dr. Bastian asserts to be favorable to their birth and existence, the whole controvery is reduced to a dispute between Drs. Bastian and Sanderson on mere facts. Still, it must be remembered that Dr. Sanderson's conclusions are merely negative. It is in the nature of things that they should so be, but Dr. Bastian's conclusions are nevertheless overturned unless some observer of equal scientific weight with Dr. Sanderson arises, who, with the same apparatus as Dr. Bastian employed, will produce results identical with those of the ingenious University College professor. This is really the only satisfactory solution of the difficulty, and until it is carried into execution we hope that Dr. Bastian will postpone the publication of his great work on the physical doctrine of life. It is true that, because his testimony is at variance with that of other observers, it need not necessarily be wrong. There have been many instances, even in anatomy and physiology, of the opinion of one solitary observer being opposed to the unanimous voice of his contemporaries, the one man having been afterwards triumphantly proved to have been correct. Yet it is difficult for dispassionate observers when, as in a jury, investigating questions of absolute fact, not to lean towards the feeling of the majority of witnesses. If the presence of bacteria is merely due to the existence of water or damp substances—if the water is carefully boiled, and other precautions familiar to the readers of M. Pasteur's work are taken—and if, when these precautions are rigorously and formally carried out, no bacteria whatever arise, the verdict of "not proven" must be certainly returned against Dr. Bastian's conclusions.

Then follows the inquiry, what amount of antecedent probability exists in their favour? Were it not that we are investigating a strictly scientific subject, on which the mere facts have to be examined and taken at their value, we would be inclined to think that the probabilities in favour of the origin of living beings, as Professor Owen has pointed out, by a sort of heterogenesis, has much to be said in its favour. We regard the alleged refutation of the probability of heterogenesis, made by Professor Huxley at the Liverpool meeting, as entirely unsatisfactory, based, as it was, upon not a single cited original experiment.

The advocates of spontaneous generation have a right to demand a demonstration of the impossibility of their statements, instead of a mere allegation of their improbability. Dr. Bastian has certainly carried out a long series of experiments, and propounded certain distinct hypotheses, and the character both of his experiments and of his views is such as to call for a serious re-examination and discussion.

Nosology of Zanzibar.[1]—The recent work of the distinguished African traveller Captain Burton contains so many important facts which may be of value to the traveller on the coasts of Eastern Africa, that we have no hesitation in calling our readers' attention to the medical facts which we find recorded therein. The climate of Zanzibar, better than that of the hot damp eastern coast, has nevertheless many unfavorable points, which seem to preclude its ever proving to be a convenient station off which Her Majesty's ships could long cruise. Though on the island many of the white residents have escaped severe fever, the disastrous fate of Captain Owen's surveyors, the loss of life on board our cruisers, and the many deaths amongst the Mombas missionaries, even though, finding the seaboard dangerous, they built houses on the mountain slopes, prove

[1] *Zanzibar; its City, Island, and Coast.* By Captain R. F. BURTON. 8vo. London. 1872.

that malaria is as active in Eastern as in Western Africa. The late consul (Hamerton) once visited the Pangani river in the month of August; of his nineteen men, three died, and all but one suffered extremely. It has been asserted, on good authority, that the prophylactic use of quinine, which was such a success in Western Africa, does not prove equally valuable on the Eastern coast. Contrary to the rule of Madagascar, the lowlands upon which the fresh sea-breeze plays are the only place where the white stranger can live and thrive; the interior, covered as it is with rank vegetation, being fatal to Europeans. It is Captain Burton's opinion that no European, unless thoroughly free from organic disease, should venture to remain longer than three or four years at Zanzibar; the same has been observed at Bagdad and in the Euphrates valley generally. The stranger is compelled to take troublesome precautions. "He may bathe in cold water, sweet or salt, but he must eschew the refreshment of the morning walk; during the rains, when noxious mists overhang the land, the unpleasant afternoon is the only safe time for exercise." Flannel must, of course, be worn, and extra warm clothing is considered necessary as long as mugginess of "msika-weather" lasts. The half-hour following sunset, when the dews fall, is held dangerous, especially in hot weather. Captain Burton, acting on a practical experience of the necessities of the human frame in the tropics, probably contravenes many of the canons of English medicine when he says that he should prescribe for the stranger—

"Contrary to the usual plan, an abnormal amount of stimulants, port and porter, not claret nor Rhine wine. It is evident that, where appetite is wanting, and where nourishing food is not to be obtained, the patient must imbibe as much nutriment as he safely can. In these lands a drunkard outlives a water drinker, despite Theodoret, *vinum bibere non est malum sed intemperanter bibere perniciosum est.*"

It is evident that the gallant captain was not aware, when he thus unconsciously corroborates the late Dr. Todd, of the declaration against alcohol which so many British medical men have signed. The common practice is, after fever, to use purgatives in large doses, for which Captain Burton proposes to substitute tonics and bitters. How far the stomach of a convalescent fever-stricken patient in the tropics is able to endure these "bitters," we are doubtful. Our own experience has told us, that the hideous "cocktail" which the Central American imbibes after fever rapidly induces concomitant dysentery. Captain Burton proceeds to say, "In all debilitating countries, when the blood is thin, laxatives must be mild, otherwise they cause, instead of curing, fever; in fact, double tonics and half purgatives should be the rule." *Crede experto.*

The nosology of Zanzibar is remarkable for the prevalence of urinary and genital diseases. These have been roughly estimated to affect 75 per cent. of the population. Sarcocele and hydrocele attack all classes; elephantiasis affects 20 per cent. Arabs and Hindoos, Moslems and Africans, however dissimilar their habits and diet, all suffer alike. The malady has never attacked a pure white, European or American. It is a strange coincidence that Captain Burton, during his vast experience in the Brazil, never saw a European stranger subject to the leprosy or to the goitre, so prevalent in the great provinces of São Paulo and Minas Geraes. The Banyans have the idea that a journey home to Bombay retards the progress of the incipient disease; it recurs, however, on return to Zanzibar. No cure is locally known for elephantiasis. Phagedænic sores are most common amongst the poor and the slaves, who live on manioc fruit and salt shark often putrid. Scabies, frambœsia, and psoriasis, as in Central America, commonly result from personal uncleanliness, unwholesome food, and insufficient shelter and clothing.

Persians and Northern Asiatics are more liable to attacks of fever than Europeans, and, as in Egypt, rude health is rare. The malignant typhus is rare at Zanzibar; it raged, however, amongst the crew of a French ship, wrecked on the northern end of the island, where the men were long exposed to privation and fatigue. Intermittent ague fevers are as common as a cold in England. They are mild and easily treated with emetics in the preliminary stages. Captain Burton advises evacuants, cooling lotions applied to the head, and sulphate of quinine (4 to 12 grains every three or four hours). He alleges that calomel and tartar emetic must be avoided, on account of their depressing effects. It must not be forgotten that there is no ipecacuanha in Zanzibar. *Liquor arsenicalis* and the patent medicine *Tinctura Warburgii* (which is said to have failed in yellow fever) have cured malignant, inveterate, and chronic cases. It is, perhaps, impossible for the tropical voyager to overrate the effect of this excellent preparation, which in dysentery has produced the most beneficial results.

The Zanzibar remittent fever is of much more danger than the intermittent, and Captain Burton gives a minute description of it.

"When an unfavorable phase sets in, all the evils are aggravated. Great anxiety, restlessness, and delirium wear out the patient; the mind wanders, the body loses all power, the ejecta become offensive, the pulse is almost imperceptible, the skin changes its dry heat for a clammy cold, the respiration grows loaded, the evacuations pass involuntarily, and, after perhaps a short apparent improvement, stupor, insensibility, and sinking usher in death. On the other hand, if the fever intends yielding to treatment, it presents, after the seventh day, marked signs of abatement; the tongue is clearer, pain leaves the head and eyes, the face is no longer flushed, nausea ceases after profuse emises of bile, and a faint appetite returns."

Excessive action of the liver is the consequent of the mildest attacks of the Zanzibar remittent, accompanied by debility and often by boils, which follow each other in rapid succession.

Diarrhœa and dysentery are mostly sporadic; the former, however, has at times attacked simultaneously every European on the island. Dysentery is especially fatal during the damp and rainy weather. It was often imprudently treated with mere astringents, and without due regard to the periods of remission and to the exhausted condition which invariably accompanies it. Captain Burton asserts—

"As in remittents the patient was weakened and his stomach was deranged with 'slops,' when essence of meat was required, the anti-diarrhœa or anti-cholera pill of opium, chalk and catechu, has been fatal wherever English medicine has extended. Witness the Crimean campaign, where the bolus killed many more than did the bullet."

Catarrh and bronchitis are common in February. Pneumonia, asthma, and consumption are frequently amongst the higher classes, especially the Arab women, debilitated by over-seclusion. Hæmorrhoids are very common both on the island and on the coast; the people suffer as much as the Turks in Egypt, without wearing the enormous bag trousers which have been so severely blamed. The smallpox (a gift of inner Africa to the world) is fatal, as at Goa or Madagascar. It disfigures half the population, and is especially dangerous to full-blooded Africans. All classes of the ignorant natives were equally prejudiced against vaccination.

Until 1859 cholera was unknown, even by name. In 1835, it is true, there was an epidemic whose principal symptoms were giddiness, vomiting, and purging, the peculiar anxious look, collapse, and death. After 1859 cholera of the most fatal type broke out, and decimated the population of Zanzibar.

The medical profession is not represented, and is entirely unknown.

"Amongst Arabs, and indeed Moslems generally, every educated man has a smattering of the healing art. H. H—, the late Sayyid, was a 'hakim' of great celebrity. A physician is valuable on the island; throughout the African interior he is valueless in a pecuniary sense, as every patient expects to be kept and fed."

Upon the whole, there can be little doubt, from the experienced and accurate account of Captain Burton, that Zanzibar is one of the best places either for the professional or layman to "live out of" in the old world.

St. Andrew's Graduates' Transactions.[1]—The wide but somewhat loose bond of graduation at the same University (and this, too, for the most part, without residence there), and the feeling of

[1] *St. Andrew's Medical Graduates' Association Transactions,* 1869. Edited by Leonard W. Sedgwick, M.D. London, vols. iii and iv. 1870 and 1871.

fellowship derivable therefrom, has given rise to the St. Andrew's Medical Graduates' Association. Moreover, the goodly company of graduates, not content with the ordinary amenities of such associations centering in meetings for mutual gratulations and festive doings, has sought to render itself a scientific body for the extension of medical knowledge. The present volumes, the third and fourth published, exhibit the results of the association's work in that direction, besides presenting the reports of meetings of the association, memoranda of the results of deliberations of the members, and lists of office-bearers and associates. Of the value of the papers generally printed in these volumes no doubt can be entertained; but the question arises, is such a special medium for publishing the lucubrations of St. Andrew's graduates needed? We have many 'Hospital Reports,' so called, published, filled with similar matter, and there are several societies which receive and insert in their 'Transactions' articles of the same stamp; and there are not a few medical periodicals in currency, in one or other of which some, at least, of these papers would have found a convenient and wider channel of communication. The members of the association expend from above one half to two thirds of their income derived from subscriptions in producing their annual volume of 'Transactions,' and, at the close of each of the two years, ended with a deficit of £20 and upwards. Hence, the volume represents well-nigh the guinea subscribed by each member, and, at the same time, is an imperfect vehicle for communicating the scientific labours of its contributors; for a glance at its receipts from its sale to non-members reveals the fact that exceedingly few copies find their way to the hands of others outside the association.

Indeed, it may be inferred, from the republication of several of the essays found in these present volumes, that their writers have courted a larger circulation of their views than they could find in their pages.

This fact of the republication of some of the essays in a separate form, and the general inability of reviewing together a collection of essays on divers topics, makes it incumbent on us to do little more than notify the contents of these volumes to our readers.

The first communication in vol. iii is the "Anniversary Address," by the President, Dr. B. W. Richardson, which, like other productions from the same indefatigable worker and independent thinker, adds something to our apprehension of the subject treated, which, in in this instance, was "The Science of Cure." This address is followed by a long essay, by Dr. C. Black, on the "Clinical Examination of the Urine in relation to Disease," of which we have spoken elsewhere when dealing with it as a separate publication. Professor Polli contributes two short papers of observations "On Haschish;" Dr. W. Cholmeley some "Notes on the Therapeutic Value of

Chloride of Ammonia;" Dr. W. Procter, a few "Remarks on Therapeutics;" Dr. S. Lawrence discusses "Aphasia and its Seat;" Dr. W. H. Day, the features, varieties, and treatment of "Gastric Neuralgia;" and Dr. W. Norris narrates some cases of melanosis. An important paper entitled "A Study of Convulsions" follows, written by Dr. J. Hughlings Jackson, a competent writer and skilled observer in such matters. Dr. D. Lloyd Roberts collects and enforces the "Points to be observed in Ovariotomy;" Dr. Tilbury Fox examines very carefully the doctrines afloat respecting the relations of pediculi and skin affections, particularly porrigo; and the President reappears, to close the volume, with a learned paper "On Intermittent Pulse and Palpitation," which has since been reprinted in a volume of essays entitled, 'Discourses on Practical Physic."

After the 1869 meeting two memoranda were placed before the members—one, relative to a proposed Royal Commission to inquire into the state of the law and the legal dicta concerning the criminal responsibility of the insane; the other, concerning the advantages of the registration of disease, and the instrumentality required to secure them. The former memorandum called forth, at the meeting in 1870, a resolution which did nothing more than refer back the question to the council of the association, whilst the latter seems not again to have attracted the attention of the members.

The fourth volume equals the previous one in its contents, for, besides the President's address, with the title "For the Future of Physic," there are eleven essays or communications, some of which have assumed an independent existence in the pamphlet form. To cite their titles, they are—"On the Effects of recent Sanitary Legislation on the Health of the Metropolis, and on our Present Urgent Sanitary Needs," by Dr. J. Whitmore; "On the Diagnosis and Treatment of Aortic Aneurism," by Dr. George W. Balfour; "On Syphilis," by Dr. Drysdale; "On Atmospheric Dust: is its total Interception absolutely necessary for the preservation of Health?" by Dr. F. E. Jencken; "Clinical Notes," by Dr. Mackinder; "On Diphtheria and the Diseases allied to it, or which may be mistaken for it," by Dr. R. H. Semple; "Therapeutic Memoranda," by Dr. W. B. Woodman; "Brain Exhaustion," by Dr. F. Needham; "On the Extrusion of the Morphological Elements of the Blood," by Professor Norris, M.D.; "On Apoplexy, and the Value of the Abstraction of Blood," by Dr. T. Ballard; and, lastly, "Notes on the late Prussian Siege of Paris," by Dr. C. A. Gordon, C.B.

Of these several papers the most considerable as to length, and the one which strikes us as the most important in its matter, is the essay of Dr. Balfour, on "Aortic Aneurism," containing, as it does, the collected histories of twenty-nine cases of the lesion illustrating the diagnosis, course, and treatment. But although we single out this paper for especial remark, we are, nevertheless, able to report that it is in very good company, and that in both volumes now before us are to be found contributions to the science and practice of medicine which must be highly appreciated by the profession as upward steps in the advancement of the medical art.

Digitalis; its Mode of Action and its Use.[1]—In this essay Dr. Fothergill brings together a great number of facts, experiments, clinical observations, and reasonings on the subject of digitalis and its action on the heart and circulating system generally. He begins by showing its action on the lower forms of organized beings, and proves that it exercises a toxical influence even upon plants, some of which wither and die when treated by an infusion of the drug. The invertebrated animals are not much affected by it, but its effect on fishes and birds is well marked, for it kills them, and after death the heart is found to be firmly contracted. Special reference is made to the action of digitalis on frogs, for these animals have been subjected to a great variety of experiments, on account of their great susceptibility to medicines. Dr. Fothergill has himself made many experiments with digitalis on these creatures, and he finds, as other preceding physiologists have done, that the uniform effect is contraction of the heart under its use; and it is shown that this effect may be produced, not only by the administration of the drug by the ordinary methods, but also by its direct application to the organs of circulation, the heart being caused to contract by having its apex dipped in a solution containing digitalis, and even the capillaries exhibiting contractile movements when infusion of digitalis is applied to a frog's foot. This contractile influence of the drug on the heart and the vascular system,—which influence is inferred, on good grounds, to be exerted through the medium of the vaso-motor or sympathetic nerves, is the text from which Dr. Fothergill unfolds his views as to the therapeutical action of digitalis. The opinions formerly entertained as to its sedative operation are shown to be erroneous, or, rather, the sedative effect is proved to be primarily due to a contractile action. Palpitation of the heart having been formerly regarded as being an instance of overacting, the remedial agency of digitalis, was supposed to be derived from sedative powers; but palpitation is really an evidence of deficiency of the heart's power, and digitalis acts remedially because it is a tonic. Hypertrophy of the heart is often accompanied by palpitation, but Dr. Fothergill argues that in this case the hypertrophy, which is a com-

[1] *Digitalis; its Mode of Action and its Use. An Inquiry illustrating the Effect of Remedial Agents over Diseased Conditions of the Heart.* The Hastings Prize Essay of the British Medical Association for 1870. By J. MILNER FOTHERGILL, M.D. London. 1871.

pensatory condition to counteract valvular obstruction or insufficiency, is inadequate to its object, and digitalis acts remedially by assisting it in that object and restoring the normal conditions. The limits of the present notice will not permit us to follow Dr. Fothergill through his chain of reasoning, intended to show the influence, remedial, injurious, or doubtful, of digitalis on the various diseased conditions of the heart; although, as we have just indicated, the starting-point is to be found in the power of the herb to induce contraction of the muscular fibres of the heart and vascular system. Thus, in palpitation from weakness, digitalis induces contractions, and thereby strengthens and regulates the organ, in hypertrophy the drug aids the enlarged organ in doing its additional work; in dilatation it induces contraction, and thereby counteracts further dilatation from engorgement of blood; in valvular disease the drug acts differently according to the valve affected, and according as the condition is one of stenosis or of insufficiency. For instance, in aortic obstruction digitalis is serviceable because it assists in increasing the driving action of the heart, but in aortic regurgitation its administration is hazardous from the risk of its bringing the ventricle to a state of contraction which may never be relaxed. These views are, of course, open to controversy, but, on the whole, we may state that Dr. Fothergill's essay is a valuable contribution to therapeutical science, and well deserves a thoughtful perusal, both on account of the experimental and clinical evidence which it adduces and of the valuable suggestions which it offers for the treatment of a very numerous class of diseases.

State of our Knowledge respecting the Action of Mercury on the Liver.[1]—Dr. Fraser, in this useful little pamphlet, gives a sketch of the history of mercury as a therapeutical agent, from its earliest introduction as a medicine down to the present day, and he examines in consecutive order the difficult theories which have been suggested as to its mode of operation. He arranges the various doctrines which have been held as to the action of mercury under four heads. 1st. That the drug simply increases the *flow* of bile into the intestines; 2nd, that it causes an increased *formation* of bile by an indirect action on the liver; 3rd, that it causes an increased *formation* of bile by a direct and primary action on the liver; and under a 5th head he adduces the doctrine held by some modern physicians, that mercury has no cholagogue action whatever. After quoting a great number of authorities in proof of the physiological and therapeutical action of mercury upon the biliary secretion, and showing that under its use the amount of bile secreted is certainly increased, Dr.

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[1] *Sketch of the Present State of our Knowledge respecting the Action of Mercury on the Liver.* By THOMAS R. FRASER, M.D., F.R.S.E., F.R.C.P.E., Lecturer on Materia Medica, Therapeutics, &c. Edinburgh, 1871.

Fraser examines more particularly the fourth doctrine laid down, namely, that mercury acts directly and primarily on the liver, and thus causes an increased formation of bile. He thinks it quite legitimate to suppose that mercury increases the formation of bile by a direct and primary effect on the liver-cells, and that this view may be maintained as a possible or probable explanation of several of the observed facts, and he entertains the opinion that mercury undoubtedly possesses a cholagogue action.

This doctrine is founded on the facts that certain characters of the alvine dejections imply an absence or diminution of bile; that in such conditions mercury will restore the natural constituents of the discharges; and that the characteristic appearance caused in the alvine dejections by the use of mercury are due to the presence of bile-constituents. Dr. Fraser then sets forth the objections that may be raised to these propositions, but he shows how they may all be answered. Lastly, he considers the fifth doctrine laid down, namely, that mercury has no cholagogue action whatever, a doctrine, as is well known, which has been founded chiefly on some experimental researches made by the "Edinburgh Committee on the Action of Mercury on the Liver," and conducted by Dr. Hughes Bennett. To these researches, and to the doctrine founded upon them, Dr. Fraser, while giving the greatest credit to the labours of the investigators, offers various objections, the chief of which are that the division of the bile-duct (which was an essential feature in the Edinburgh experiments), and the consequent derangement of the whole digestive process, introduced entirely new elements into the inquiry, and that the inflammation and suppuration caused by the experiments themselves, the probable derangement of the nervous supply of the digestive organs, and other disturbing influences militate against the reception of the conclusions which the experiments are designed to support.

Dr. Fraser's opinion is, that the doctrine advanced by the Edinburgh Committee is not supported by sufficient evidence, and this opinion will probably be indorsed by the profession generally, the more especially as it is in opposition to daily experience and a multitude of recorded facts, showing the power of mercury, both in health and disease in the human subject, in increasing the biliary secretion.

Dr. Atthill on Diseases of Women.[1]—Dr. Atthill tells us in his preface that he publishes these lectures at the request of some members of his class, and also because the candidates for degrees at the Queen's University and the College of Physicians (Ireland) display great ignorance of the diseases peculiar to women, "pleading that

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[1] *Clinical Lectures on Diseases peculiar to Women.* By Lombe Atthill, M.D., Univ. Dub. Pp. 208. Dublin, 1871.

they have not leisure to read up the subject in the voluminous [? West, Hewitt] works which exist on uterine disease." We trust the teachers in the medical schools in Ireland will accept the challenge thus given by the examiner for the said degrees.

The present book is addressed to the student, but others may here and there glean a useful hint.

The first chapter is taken up with a description of the mode of examining patients, supposed to be suffering from uterine disease—by the touch, the speculum, and the sound. There is nothing in the text requiring remark, but we may call the attention of other writers, who employ diagrams and drawings to illustrate their teaching, to the common error committed in representing the bladder and rectum distended to their utmost as the ordinary condition of the parts. This error is repeated in the present book, in a vertical section of the pelvis introduced to show how the sound should be passed, and besides this the vagina, instead of being shown flattened, is also represented as distended enough to take a couple of fingers and the sound. The first step before making an examination of the uterus to see that the bladder and rectum are empty is consequently ignored.

Dr. Atthill divides leucorrhœa into vaginal, cervical, and uterine. He finds tobacco injections of great service in allaying pruritus accompanying chronic vaginitis; he makes the injection by infusing two drachms of the unmanufactured leaf in a pint of boiling water, and declares that "he has never seen the least unpleasant results follow its use, while the relief it afforded has often been marked." His remarks on menstruation and its irregularities appear to us judicious and practical, but we are much surprised to read at page 38 what he has written about a *galvanic* intra-uterine stem. After detailing a case of amenorrhœa in which the catamenia reappeared subsequent to, and apparently consequent on, the application of the poles of a galvanic battery, one to the sacrum and the other to the vulva, Dr. Atthill says, "There is another mode of stimulating the uterus, which I think I prefer to electricity as ordinarily applied—I allude to the galvanic stem pessary. This little instrument is made of copper, the upper half of the stem being coated with zinc." The stem is to be worn for some time with occasional removals. Dr. Atthill plainly refers the stimulation of the uterus to the galvanic action set up by his compound metal stem immersed in alkaline mucus, and yet we think that, if he were to try and measure the quantity and tension of the electricity that such a battery in such a position would yield to the galvanometer, he would get but a negative result. We are strongly reminded of the galvanic rings that some years ago were bought by credulous old ladies to be worn against rheumatic pains. The presence of a foreign body in the cervical canal, and especially in contact with the os internum, which we know to be sensitive,—shown by the sickness and other sensations caused on the passage of a sound through it, sufficiently accounts for the advantages which often undoubtedly result from this treatment without looking for any hypothetical voltaic action which is simply non-existent. The glass stem with vulcanite button of Dr. Meadows is cleaner than metal.

Dysmenorrhœa, which Marion Sims and others refer to a supposed mechanical obstruction, consisting in the narrowing of the cervical canal by the tumefaction of its coat, Dr. Atthill argues is not merely mechanical but due to endometritis, and that it is this diseased condition of the mucous membrane which occasions the dysmenorrhœa. He concludes this to be the case because he found he could reproduce the pain felt at "the period" whenever he passed a sound into the cavity of the uterus, although it met with no obstruction at the os internum. Dr. Atthill's remedy is to dilate the cervix and mop out the uterus with strong nitric acid.

Division of the cervix in the case of "the pinhole os" he thinks very feasible for dysmenorrhœa, but that "it is not warranted as a proposed cure of sterility, because the narrow os and contracted cervical canal are not the cause of the sterility, but merely an index of some congenital condition or defect in the uterus itself which hinders conception."

In referring to the constitutional causes predisposing to menorrhagia, Dr. Atthill quotes the case of a woman, for some time under his observation, who suffered from profuse and dangerous menorrhagia apparently dependent on mitral obstruction in the heart. The local causes of the malady he enumerates are (1) subinvolution of the uterus; (2) granular ulceration of the os and cervix uteri; (3) inflammation and congestion of the membrane lining the cavity of the uterus, and a granular condition of that membrane; (4) retention within the uterus of a portion of placenta or of the fœtal membranes; (5) polypus of the uterus; (6) fibrous tumours of the uterus; (7) inversion of the uterus; (8) ovarian excitement or congestion; (9) epithelioma. In the treatment of menorrhagia due to subinvolution, Dr. Atthill finds "nothing so simple and so safe as the introduction of a piece of solid nitrate of silver, weighing ten grains, into the cavity of the uterus and leaving it there. It checks the menorrhagia and stimulates the uterus to take on the suspended disintegrating process." The secretions of the uterus probably form inert albuminates and chlorides of silver, and thus stay its destructive powers.

To dilate the cervix, Dr. Atthill prefers sea tangle tents to sponge, and speaks of putting as many as seven pieces side by side into the cervix. His method is to introduce first a piece long enough to pass the os internum, and then pass by the side of this guide as many short pieces as he can introduce. He prefers to place the patient in "Marion Sims'" position, and to use a duckbill speculum and tenaculum.

If on examination the mucous membrane of the uterus is found rough, Dr. Atthill applies strong nitric acid freely by means of a strip of lint on a holder; the cervix having first been dilated, "at the end of five or six weeks I introduce the speculum, and examine the os. The slough caused by the nitric acid has generally by this time separated, and you have a healthy granulating surface exposed to view. I brush this over with a ten-grain solution of nitrate of silver at intervals of a day or two, and in a fortnight, as a rule, it is perfectly healed." It is not clear whether Dr. Atthill applies the nitrate of silver to the mucous membrane of the uterus or only to the cervix, for he says "you have a healthy granulating surface *exposed to view,*" while the treatment is intended for the uterine cavity. In retained placenta after abortion, Dr. Atthill gives ergot combined with strychnia, and recommends such a combination to be used in labour cases requiring ergot. He finds good results from gallic acid in ten-grain doses, but thinks acetate of lead very irritable in metrorrhagia. When treating of fibrous polypi, Dr. Athill brings under notice a modification of the écraseur. As all operators know the difficulty in getting the wire (the chain is now seldom used, as it breaks more readily) round the base of the tumour when high up in the uterus is sometimes very great, Dr. Atthill has had an écraseur made with the end so modified as to allow of the passage through it of the tubes of Gooch's canula. The wire being adjusted by means of these canula, they are passed through the eyes of the écraseur till it reaches the base of the tumour, when they are withdrawn and the wire fastened in the ordinary way. Weiss is the maker.

Gastrotomy for the removal of the sub-peritoneal fibrous tumour of the uterus, Dr. Atthill utterly repudiates. He considers these tumours "to be entirely beyond the reach of treatment, and yet it may reasonably be questioned whether in the case he quotes, as illustrative of the disease, operation would not have been justifiable. "The patient, æt. 35, had detected a small tumour in the iliac region; this in two years' time had increased till she had attained the size of a woman when near the full term of pregnancy. The diagnosis of uterine cystic disease was made, and all idea of surgical interference was given up. This patient subsequently died of an attack of acute peritonitis. The post-mortem disclosed an enormous tumour which consisted mainly of an immense cyst; it sprang from the anterior and upper surface of the uterus, being connected with it by a short thick pedicle."

Dr. Atthill most judiciously urges the necessity of dilating the cervix previous to injecting either Tinct. Iodini or Tinct. Ferri Perchlor. for the purpose of restraining hæmorrhage; almost all, if not all the cases, in which this treatment has been followed by peritonitis have been when the dilation of the cervix has not been performed; for even a few drops of glycerine have induced uterine colic. Abstraction of blood from the cervix is often required when there is congestion of the part. Dr. Atthill prefers puncturing the organ to leeching it. "The depth to which I make the point of the knife penetrate varies from one eighth to a quarter of an inch or even more according as the cervix be soft and vascular or firm and indurated. I punctured it in two or three places, using a long straight-backed French bistoury, which terminates in a very sharp point." We think, however, it is the general experience that, if it be required to take blood from a hard fibrous feeling cervix, leeches are the only effectual mode. If the os is plugged with a piece of cotton wool, and four or six leeches be passed down to it, the speculum being carefully kept in *sitû*, a better result will be obtained than by stabbing the gristly mass.

Inflammation of the cervix is minutely treated in the eleventh lecture. Dr. Atthill divides the acute form into two stages—the congestive, manifested by great vascularity of the mucous membrane, covering the vaginal portion of the organ, which becomes of a bright pink colour, and by engorgement and tumefaction of the substance of the cervix, which, however, feels soft and elastic to the touch. In the second stage the mucous membrane is denuded and its epithelial covering presents the appearance of an irregular abraded surface of a deep red hue, which pours out a profuse mucopurulent discharge, and is studded with papillæ. This latter form Dr. Atthill terms ulceration, following Dr. Bennett's definition, "A solution of continuity from which is secreted pus or a puriform sanious or other matter." At the same time he believes actual loss of substance, to which state Dr. Fane and others restrict the word ulceration, is very rare. The treatment advocated by Dr. Atthill is to puncture the cervix if abstraction of blood be desirable, as is almost always the case; or if it be desirable to stop hæmorrhage, he paints with a saturated solution of the perchloride of iron in glycerine. When the canal is involved he dilates it and applies strong nitric acid, and subsequently puts up a plug of cotton wool saturated in glycerine, in which ten grains of tannic have been dissolved. In place of the nitric acid he has also applied a styptic colloid made by dissolving ten grains of benzoic acid and fifteen grains of tannic acid in four drachms of collodion, and adding twenty-five grains of carbolic acid if uterine disease is present. "This application has proved very successful."

Endometritis is characterised by paroxysmal pain, dysmenorrhœa, and is frequently accompanied with pain of an unusually severe character felt along the edge of the false ribs; the sound passed internally causes severe pain on touching the fundus; menorrhagia is a frequent sequel when the inflammation has become chronic, while the bladder and rectum are often severely affected.

We have now brought forward the principal points of interest, and must refer those who wish to know Dr. Atthill's opinions on ovarian disease, displacement of the uterus, cancer, and epithelium to the book itself, which, with the exceptions we have taken to it above, will be found to be a very useful and judiciously written work.

Murray, cure of Aneurism.[1]—This brochure gives a full account of the well-known case in which Dr. Murray, of Newcastle, cured an aneurism of the abdominal aorta by the "rapid pressure treatment." As the patient was exhibited at the Royal Medical and Chirurgical Society in 1864, and as a description of his case appeared in the 'Transactions' for that year, our readers are probably familiar with its chief features. In the essay before us the history of the patient is completed to the time of his death in 1870. The narrative throws a most interesting light upon the treatment of aneurism by means of rapid pressure, as practised by the author. All the information that can be given with regard to the experience of surgeons upon its use is here collected, and other cases, more or less like his own, in which the treatment he recommends has been followed, and with no small success, are referred to by Dr. Murray. Indeed, we are encouraged to hope that the rapid pressure may enable us hereafter to deal successfully with a class of cases which have hitherto been regarded as incurable, and to expedite the treatment of other aneurisms for which long-continued pressure has been applied. "Are we to have cure by the coagulation of blood in *five hours* by completely arresting the current through the aneurism, or cure by lamination of fibrine in *twenty-five days* by frequently and imperfectly arresting the circulations?" This is the question, as it is very pointedly stated by Dr. Murray; and he has no hesitation in replying, that the former is the most desirable course.

Dr. Murray's brilliant achievement has been fully appreciated by the profession; but perhaps it has hardly been noticed how wide a bearing it is likely to have, and how many cases may be treated on the same principle. All surgeons who are called upon to undertake the charge of cases of aneurism should make themselves acquainted with this little monograph, for it is scarcely too much to say that the treatment which it explains adds another notable step to those which have already been taken in the cure of this serious affection, and that it is as great an advance upon slow and gradual compression as the latter was upon the use of the ligature.

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[1] *The Rapid Cure of Aneurism by Pressure; illustrated by the Case of Mark Wilson, who was Cured of Aneurism of the Abdominal Aorta in the year* 1864. By WILLIAM MURRAY, M.D., &c., Lecturer on Physiology in the University of Durham, &c. London, 1871. Pp. 43.

Adams's Operation for Bony Anchylosis.[1]—In this pamphlet Mr. William Adams has brought together, in a collected form, a variety of materials relating to his new operation for the cure of bony anchylosis of the hip-joint with malposition of the limb. These materials have for the most part already appeared elsewhere, and are now merely reprinted; consequently, they need not be noticed at any length. But the subject is one of so much interest to the profession, that we should be sorry to pass it over in silence.

The operation of subcutaneous division of the neck of the thigh-bone has attracted so much attention, and has been so generally recognised as one of the most remarkable advances that operative surgery has recently made, that Mr. W. Adams has acted wisely in putting all the information that he is able to give respecting it in a concise shape. Many surgeons, who may have under their care cases of anchylosed and distorted hip-joints, will be glad to learn from this pamphlet to what classes of cases the operation is suitable, and what are the steps in its performance.

Mr. W. Adams gives first a slight sketch of osteotomy so far as it has been practised for the relief of anchylosis of the hip-joint, and he takes the opportunity of pointing out wherein his own operation differs from every other previously adopted. He next goes on to explain fully the operation which he performed in the end of 1869, and which was the first of the kind in the annals of surgery. It was speedily accepted and practised by other surgeons, and the author is now able to give us brief notes of six other cases in which his operation has been carried out. Only one of these cases terminated unfavorably; and the nature of this fatal one was such as to throw much light on the subject, and to indicate clearly the class of cases to which the operation is most applicable.

The pamphlet concludes with a reprint of the public correspondence which passed between Mr. Adams and Mr. Brodhurst with respect to priority in adopting this operation. There can be no doubt, we think, that the former has thoroughly established his position, and we can only regret that there should have been grounds for such a correspondence in the first instance, and that it should now be thought necessary to reprint it.

Dr. Pullar's translation of Neumann's Skin-Diseases.[2]—Having reviewed the first German edition of this work at some length, we need not repeat the high estimate there given of Dr. Neumann's per-

[1] *A New Operation for Bony Anchylosis of the Hip-joint; with Malposition of the Limb by Subcutaneous Division of the Neck of the Thigh-bone.* By WILLIAM ADAMS, F.R.C.S., Surgeon to the Royal Orthopædic and Great Northern Hospitals, &c. London, 1871. Pp. 68.

[2] *Text-book of Skin Diseases.* By Dr. ISIDOR NEUMANN. Translated from the second German edition by special permission of the author by ALFRED PULLAR, M.D. 1871. Pp. 327.

formance. The second edition, published in July, 1870, which forms the text of the translation before us, corrects some of the numerous misprints of the former one, and adds fresh information on a few points. The number of engravings of microscopic appearances is also increased, and some of the earlier ones are corrected, *e.g.*, that of the hair-bulb and sheath (fig. 8). But the most important additions are those in the section on senile changes of the skin and in that on parasitic diseases. The effects of age upon the skin are—atrophy of the papilla, especially marked on the face, so as to render the surface of the cutis perfectly level in advanced cases, atrophy of the hair-follicles and the muscular fibres connected with them, with their loss of elasticity as well as of contractility, dilatation of the sebaceous glands, pigmentary degeneration and thinning of the rete mucosum, and accumulation of masses of horny cuticle. The atrophy of the subcutaneous fatty tissue and the loss of elasticity in the cutis produce the lines and furrows characteristic of the aged skin, and additional depressions are formed by open orifices of the shrivelled hair follicles, into which the dilated sebaceous glands open.

The chapter on parasitic fungi is well illustrated. Dr. Neumann does not undertake to decide the botanical relations of the plants which produce the various diseases. It excludes from this group the so-called *Tinea decalvans,* which appears in the chapter on atrophy of the hair, under the title Alopecia areata. Whatever may be the result of the long-continued controversy as to the parasitic origin of this disease, the woodcuts and further details given in this edition can leave no doubt that the affection described by Hebra as eczema marginatum owes its peculiar features to the presence of a fungus. Dr. Neumann finds that by cultivating the mycelium for a week or more penicillium is produced.

As the most compact and accurate handbook of the first school of cutaneous medicine now existing, and as the only complete account yet published of the great advances recently made in the morbid anatomy of the skin, this work well deserved translation, and Dr. Pullar has on the whole performed his task with care. There are, however, numerous misprints, especially of names, *e. g.*, Caillants for Caillault, Grubi, and some of the prescriptions are a curious jumble of Latin, German, and English. Blunders such as ἤλη for χήλη have survived both German editions, and now reappear in the translation.

The first volume of Hebra's great work is referred to (p. 7) without any hint that the second has now appeared for several years, and has been translated and published by the Sydenham Society. The admirable microscopic drawings which form perhaps the most valuable part of the German work are reproduced in the translation, and lose very little of their sharpness of outline and clearness of detail. The printing of the letter-press is, however, inferior, and the book has a curious resemblance to the excellent translations published in America. On the whole we can conscientiously recommend Dr. Pullar's translation to every medical man who cannot read German. It is the most complete and accurate book on cutaneous diseases published.

Gant's Science and Practice of Surgery.[1]—In the opening paragraph of his Preface, Mr. Gant writes:—"It is now many years since a new systematic work, representing the science and practice of surgery, has appeared in this country." On reading this statement we rubbed our eyes to be assured it was no delusion we were under, for, without straining the memory, some not inconsiderable works on surgery offered themselves to our recollection, and we should have ventured the opinion that Mr. Gant must have heard of them, did not his remarks intimate the contrary. For example, we may allude to the treatises bearing the names of Fergusson, Erichsen, Holmes, and Spence. But, letting this marvellous assertion pass, we hope it may be many more years before another volume like the one under notice makes its appearance, so little credit does it to the writer as a contributor to surgical science and practice.

So far as it represents modern practice, it does so by giving a *réchauffé* of other men's opinions. The very name and external appearance of the book at once suggest one of our best-known and most valuable works on surgery, and a careful comparison serves to show how closely Mr. Gant has fashioned his treatise on the model of Mr. Erichsen's 'Science and Art of Surgery.' But it is not only the distinguished surgeon of University College who has reason to complain of our author's plagiarism. He has made an equally free use of Mr. Holmes's 'System of Surgery;' and, indeed, there is scarcely a writer of any reputation who has not been laid under heavy contribution by Mr. Gant. It is not only that their opinions are alluded to, and quoted for the purpose of discussing them—a proceeding that could, indeed, not be avoided in any treatise on the same subject, but whole passages have been transcribed. In fact, a great part of the book is composed of extracts from the writings of living authors. It is not difficult to supply examples of this method of book-making. Almost any chapter, if closely examined, may be traced to some well-known book or essay, and paragraphs have been transferred to Mr. Gant's pages, *verbatim et literatim*, but with very scanty acknowledgment. Thus, the chapters upon Cleft palate and Lithotomy are taken from Sir W. Fergusson's 'System of Practical Surgery.' In a similar way the chapters upon Hernia and Aneurism are derived from Mr. Erichsen; while Mr. P. Hewitt's article upon

[1] *The Science and Practice of Surgery.* Illustrated by 470 wood engravings. By FREDERICK JAMES GANT, F.R.C.S., Surgeon to the Royal Free Hospital; formerly Surgeon to Her Majesty's Military Hospitals, Crimea and Scutari. London, 1871. Pp. 1265.

"Injuries of the Head," in 'Holmes's System,' appears to have supplied the material for Mr. Gant's remarks on the same subject. Examples of this kind might be multiplied to almost any number; indeed, the frankness with which our author points out the sources whence he has drawn his information would be almost amusing if he were not guilty of a very serious literary fault.

There are, however, some subjects upon which Mr. Gant flatters himself that he has some original material to lay before us. One of these is the arrest of hæmorrhage. But here his observations are by no means on a level with the knowledge of the day. He seems to be ignorant of the minute changes which take place when a vessel is ligatured, and of the researches upon this point both by English and Continental writers. Indeed, though our author speaks much of "Pathological Surgery," and of what he has done to enforce it, his own acquaintance with pathology appears very limited. For instance, at p. 259, speaking of the ligature and its effect in normal cases, we are told—"The included portion of external cellular coat and sheath, having undergone continued compression, sloughs, and is detached with the thread, in a period varying from twenty-four hours or so to about three or four weeks, chiefly according to the size of the artery." Now, as a matter of fact, no sloughing occurs. The compressed portion is absorbed by the action of the lymphatics and veins. When, unhappily, sloughing, or even ulceration, does take place, it involves the patient in all the risks of secondary hæmorrhage.

There is another subject upon which Mr. Gant has given his own experience, and here with better effect. The chapers upon "Excisional Surgery" are the best in the book. They are the expansion of the author's Lettsomian Lectures, which were favorably received at the time of their delivery; and Mr. Gant would have consulted his own dignity and reputation if he had been contented with writing a monograph upon a subject of which he is confessedly master, instead of publishing a patchwork treatise upon general surgery.

But it is not merely opinions that he has derived from others, but also illustrations. Everywhere throughout his pages we are met by old familiar woodcuts, some of which are quite obsolete, and represent methods of treatment which have now passed out of use.

It will be seen, therefore, that there is but a very small portion of this ponderous volume which we can recommend to our readers. For the rest, they will do better to consult the original treatises from which our author has borrowed his materials so extensively.

Gulia on Diptheria.[1]—We have on our table a volume written in choice Italian by Dr. Gavino Gulia on the subject of diphtheria, under the title of 'Clinical Notes on Diphtheria.' The writer hails from

[1] *Notizie Cliniche sulla Difteria.* Per GAVINO GULIA.

Malta, one of our most cherished possessions, and might almost call himself a fellow countryman. As in his Preface he appeals "to the kind reader," and states that his work is a "raccolta" (collection) of various writings on diphtheria printed from time to time in the 'Corriere Mercantile,' which is not a medical paper; and as he adds, that, without "presumptuous vanity," he has published it for the benefit of young medical men, old practitioners may be excused for noting that it *is* simply a compilation. In short, it is a made-up book with pages of quotations from well-known writers on the subject, and includes a chapter which drags its length along from page 186 to page 206, in which almost the only allusion to the disease the author is supposed to be writing about is in page 204, where an extract is copied from Bricheteau.

Dr. Gulia's industry and perseverance as a practitioner are well known in the "Fior del mondo,"—the name which his fellow-citizens delight to give to their beloved island; but we fail to find in the treatise he has now published any original observation, or, indeed, any marks of its professed character as a result of clinical observation. Indeed, the latter defect is not much to be wondered at, since his opportunities for careful clinical inquiries, properly so called, are, as we can speak from personal knowledge of the island, necessarily limited by the comparatively small population coming under his professional supervision in the absence of a hospital appointment to furnish him with cases.

It is, however, but fair to remember that a *réchauffé* of this sort, though of little value to practitioners in Great Britain, may prove instructive and useful to those in Malta, and be well appreciated by them.

Camp Life as seen by a Civilian.[1]—This book is sadly out of time. The camp life it has to tell of is that of the British army in the Crimea, and considering the many volumes of descriptions contained in books and papers of all matters, great and small, connected with the belligerents, with the seat of operations; giving a "true, full, and particular" account of what was said and done, or intended to be done, by all classes in the ranks of the British forces, and bristling with impressions of every hue of peoples and places having the charm of novelty; considering (we repeat) a veritable plethora of descriptions, of which a large proportion have in the course of seventeen years lapsed into oblivion, it required much hardihood to challenge the attention of the public to yet another personal narrative of bygone events. In short, we hold Dr. Buchanan to be a very bold man, and one who, though not a soldier, would like to fight his battles over again. We fear, how-

[1] *Camp Life as seen by a Civilian; a Personal Narrative.* By George Buchanan, A.M., M.D. Glasgow, 1871. Pp. 298.

ever, he will lack the encouragement of a large and attentive audience, the part of the narrator of personal experience in the Crimean campaign having been played out.

We should have attributed the appearance of the book wholly to those fosterers of unnecessary literature, the kind and appreciative friends whose mission is to make work for printers and publishers at the cost of the aspiring littérateur; but the author tells us that another incentive to his putting his memoranda into print was, that his friends had, by their eager perusal, nearly thumbed his MS. into shreds, and so jeopardised its longer existence.

The result in the matter of typography is everything that could be desired; and although our wonderment at the appearance in 1871 of a narrative of ofttold events that happened seventeen years ago remains, we are bound to say that Dr. Buchanan's jottings of his observations, sayings, and doings, have much vivacity, and bear the marks of truthfulness and of an observant mind.

They are by no means of a medical character, although he served for a short time as a civil surgeon in the army, was attached to an hospital, and had medical charge of troops.

The contents are in the form of a diary, written whilst the events described transpired, and now printed almost verbatim from the original memoranda. Notes of his own doings, of his illness in Turkey, of his impressions of towns, scenery, and people, and of camp gossip, constitute the substance matter of the book. The production of the volume will at least give its author that degree of satisfaction which every ordinary mortal experiences on finding himself in print, and we hope his partial friends will now demonstrate their appreciation of his narrative by largely possessing themselves of copies of it in its present very eligible form.

Original Communications.

I.—Observations on the Use of Mercury in general, and in certain diseases of the Brain in particular. By E. Copeman, M.D., F.R.C.P.

It must not be expected that in the compass of a paper of this description, I shall be able to enter fully into a subject of so much importance as the one I have chosen; but as the minds of medical men are by no means settled with regard to the employment of mercury in disease, or its influence upon the system, it may be well to say a few words in explanation of one's own experience of nearly forty years as to the benefits to be derived from mercurial treatment. At the outset of my professional career mercury was used to a very considerable extent and in almost every form of disease, and I have lived long enough to find it almost discarded from the prescriptions of the medical practitioner. Now my firm belief is, that both these extremes are wrong, and that mercury is a very powerful agent in our hands either for good or harm, according as it is used with judgment and ability, or recklessly and without discrimination.

I believe it is pretty well determined that mercury is not suitable for the treatment of acute serous inflammations *in their early stage.* Inflammation has now, as in Hunter's day, *natural terminations,* viz. effusion, adhesion, suppuration and mortification, the latter in more senses than one. Now I do not think that mercury is adapted to the *prevention* of any of them; but that it *may* prove a hindrance to some of the natural processes, especially that of adhesion, which nature sets up for the cure of inflammation; and when the terminations by effusion and adhesion have taken place in organs not endangered thereby, it will be best to leave the further restoration of the affected parts to the efforts of nature, in healthy constitutions. But when the attack of inflammation results in the effusion of fluid (serum) or lymph in organs of vital importance which cannot be damaged even for a time without a certain amount of danger to life, then I believe mercury to be a very powerful agent in effecting the speedy removal of such depositions, and assisting nature in restoring the healthy action of the part affected. Mercury will not prevent the deposition of lymph in an inflamed lung, but it will greatly aid in the removal of it during convalescence from the acute attack. It may not prevent the formation of false membrane in croup, but it will assist materially in removing it when formed. It will not cure the acute stage of phrenitis, but it will tend greatly to assist nature in the removal of inflammatory products. It will not prevent iritis, but it will clear away the lymph deposited by the inflammation, and restore the organ to a healthy condition. I do not advocate the use of mercury to any extent in acute inflammations, but I have firm faith in its power to remove the lymph and cause the absorption of the fluid consequent upon either acute or chronic inflammations in previously healthy organs. Take for example a case of pneumonia in a healthy constitution, in which we find there is always a tendency to the speedy termination of the inflammation in the deposit of lymph, which soon consolidates the portion of the lung affected. Now the effect of giving mercury freely in the acute stage of this disease would probably be to interfere with its natural termination by rendering the effusion less plastic, more like that deposited in strumous inflammation; and the stage of consolidation would be prolonged and imperfectly terminated; but when this stage has been accomplished, the addition of mercury to whatever other treatment may be adopted will, I am quite sure, tend very much more to the speedy aad entire removal of the deposited lymph than if, as a medicine, it be thrown aside or rejected. The great value which mercury possesses in the treatment of inflammations appears to be its power of aiding the absorption of inflammatory products, so as to restore the diseased organ to a healthy condition. I now propose to offer a few cases illustrative of *cerebral disease*, in which mercury has also appeared to have been of material service in clearing up the functions of an affected brain.

Case 1.—A retired tradesman, of full habit and previously in good health, about sixty years of age, was attacked suddenly in 1859 with apoplexy, having stertorous breathing and general insensibility. He could not be roused, and could neither speak nor swallow. But in a few days, without any other treatment than counter-irritation and purgative injections, he partially recovered his senses, and was found to be paralysed on the right side. His tongue, which he was now just able to protrude, was thickly coated with yellowish white fur; his vision was indistinct, and although he appeared to understand to a certain extent, he could not answer questions in an intelligible manner, neither could he swallow without difficulty. Ten grains of calomel were put upon his tongue, and some of it was washed down with a little water; a blister was applied to the nape of the neck, and purgative injections continued. His bowels were several times well relieved and the stertor abated, but for several days he lay in a semi-conscious state, and made but little progress towards recovery. It was now determined in consultation to try the full effect of mercurial action, and 2-grain doses of calomel were given at repeated intervals. It was not, however, pushed to full salivation; but during its exhibition he gradually became more sensible, could swallow better, and seemed to be slowly regaining the power of moving the paralysed limbs. He was supported by beef tea, milk, and wine and water. His pulse had never varied much from the natural standard, his bowels were every now and then largely relieved though not purged; his tongue cleaned, his speech gradually improved, and there seemed a good prospect of recovery.

The mercurial treatment was continued in a modified form (Bichloride) for several weeks, whilst his strength was supported with improved diet; and in four months after his attack his health was so much restored that he was able to walk about a little, and complained only of a slight degree of weakness in the right limbs. He continued to improve, could soon walk without help, and I frequently meet him in the street, looking perfectly well, and showing no sign of his former illness (which has never been succeeded by a second attack) except slight hesitation in his speech. He has remained well up to this present month of September, 1865, rather more than six years from the attack.

(Since this report I have heard of his death rather suddenly by an attack, I believe, of diarrhœa.)

Case 2.—J. C—, a cabinet maker, of sallow appearance and weak health, about forty-five years of age, had been subject to headaches and sharp diarrhœa every now and then for two or three years, and for the last few weeks his bowels have been so disordered as to materially reduce his strength. His pain in the head was more frequent, alternating with the diarrhœa, and there was a suspicion that slowly progressive cerebral disease might be the "fons et origo mali." On the 13th of March, 1856, I was called in consultation in consequence of increased cerebral disturbance, and found him confused in intellect and partially paralysed on the left side, pulse feeble, tongue red and dry, and bowels not now acting. He could be roused, but answered questions imperfectly, and his countenance was expressive of pain. We ordered him an aperient and nourishing diet, but my colleague was afraid of acting much on the bowels, as he had several times experienced great difficulty in controlling the diarrhœa. He did not improve, but daily became more obtuse, and, after a short time, completely comatose, with stertor and entire loss of consciousness, indeed quite apoplectic, and we thought it was almost the closing scene of long standing cerebral disease. In this dilemma we agreed, as a *dernier ressort*, to have recourse to mercury; and although we could scarcely make him swallow, we did manage to get some Hyd. c. cret. down, thinking this the best form to give it in with his irritable bowels. He soon began to improve a little, and as he became more able to swallow, we gave him 5 grs. of the powder every four hours, with beef tea, and wine and water. It is needless to report the changes from day to day; suffice it to say that in the course of a few weeks, to our utter astonishment, his mental faculties gradually reappeared, his paralysis gradually disappeared, and in course of time his health was so much restored that he was able to resume his occupation, and was much less subject to diarrhœa than before his attack. He remained well until the year 1859, in the early part of which he became dropsical, but from this he also recovered. Just before Christmas of the same year, when seemingly quite well, he caught a severe cold and died in six days with epileptic fits.

Case 3.—On the 9th of August, 1856, I was summoned a few miles into the country to visit in consultation Mr. H—, a wine merchant of about thirty years of age, married, and who had travelled a good deal abroad. He had been too fond of the commodities in which he dealt, and was spare and feeble in constitution. I found him just recovering from a severe epileptic fit, with frequent pulse, strabismus, semi-consciousness, and every sign of exhaustion. It was difficult to assign a cause, unless it were the habitual use of stimulants in greater degree than was prudent or safe; but from his previous history, disease of the brain was suspected. He was treated by mercurial purgatives, ammonia, and blister to the nape of the neck. The attack passed off, but left his nervous system a good deal disturbed; he had confusion of thought and partial loss of memory, and, for these symptoms, he was treated with drachm doses of the Liq. Hyd. Bichlor., and a seton in the back of the neck. The result was satisfactory, and after a short time he seemed to have quite recovered.

I heard no more of him till December of the same year, when, on the 22nd of that month, I was summoned in consequence of an attack of sudden loss of consciousness preceded by an unusual degree of mental depression and forgetfulness. He had feeble pulse and pale countenance, respiration loud, and it was with great difficulty that he could be roused at all. The apoplectic symptoms, however, soon abated, and after a few hours he was able to answer questions. He became very sick and vomited freely; we gave him a turpentine enema, which acted on the bowels. The rest of the treatment consisted in giving him a colocynth and henbane pill every night, and, owing to his depressed condition, a dose of quinine three times a day. But he did not improve much, his intellect remained dull, his muscular power became more and more feeble and imperfect, his stomach was at times so irritable that nothing would keep down, and he became emaciated; indeed, he appeared to be labouring under gradual softening of the brain, and his case seemed almost hopeless. He remained much the same for two or three weeks, and then became insensible; he seemed to have general paralysis, could neither speak nor move his limbs, breathed with a degree of stertor, and lay in bed quite in a passive state. We thought it the concluding scene of his life; but as he could still be made to swallow, we determined as a last resource to try the effect of mercury, and gave him 2 grs. of calomel every four hours, putting it on his tongue and washing it down with wine and water. For two days he continued much the same, and we seemed to have gained nothing but time; but on the third day he was a little more conscious, which encouraged us to persevere till we produced the constitutional effects of mercury. It would be tedious to give the daily reports of the progress he made; suffice it to say, that by keeping up slight mercurial action for a month, he gradually got out of his very critical state, and eventually recovered without any remaining paralysis or mental incapacity. He was able after a time to resume his business; his illness had taught him the necessity for temperance, and to the present time (1865) he has remained in fair health, and conducts his business as usual. I have heard nothing about him since, and for all I know he is even now alive and well.[1]

Case 4.—Mr. M—, a collector of rates, and much engaged in parish accounts, upwards of sixty years of age and inclined to obesity, was seized with a fit of apoplexy in April, 1858, and lost all consciousness and power of motion. I saw him on the 23rd, the day after the attack, and feared from the deep coma, stertorous breathing, coldness of extremities, and contracted pulse, that there might be fatal extravasation of blood within the cranium. We applied a blister to the nape, and after rousing him as much as we could, put some calomel on his tongue, and made him swallow some water after it, although apparently by reflex action only. Mustard had been applied to the legs, and a turpentine enema administered. He had for some time been in low spirits about his affairs, and the keeping of his accounts had lately been very fatiguing to him, producing headache and confusion of thought to such a degree that he was obliged to give it up. I saw him again a few days after and found him somewhat rallied. He could articulate slightly, swallow beef tea, &c., and his limbs were not paralysed. We agreed to give him as nourishing a diet as his stomach would bear, and to put him under a course of mercurial treatment in the form of dram doses of Liq. Hydr. Bichlor. three times a day, with aperients when required. The result of this treatment was, that he gradually recovered,

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[1] Since this was written I have made further inquiries about this patient, and find he is now (1871) living with his family in Italy in good health and without having had any return of his disease.

although his constitution was evidently impaired by the shock he had undergone.

As I observed before, I have no doubt as to the power of mercury to assist nature materially in the absorption of *inflammatory* products; and the cases I have related, as well as others with which I am acquainted, seem to prove also its value as a medicine in clearing up many diseases of the nervous system more or less involved in obscurity, but which we have reason to believe, *as far as morbid anatomy has instructed us,* are dependent on some kind of effusion causing pressure and diseased action in the part, which must be absorbed before recovery can be accomplished. Such effusions or deposits, whether inflammatory or otherwise, may often be removed by the action of mercury, and I should never feel satisfied to let patients die of obscure cerebral symptoms without giving them the chance of recovery which mercurial treatment might afford.

II.—On Cancer of the Tonsil Glands. By Alfred Poland.

Preliminary Remarks.—Cancer seldom attacks the tonsils, and this is remarkable, considering the amount of friction and the contact of all kinds of substances, as well as the influence of atmospheric changes, besides an intimate association with the lymphatic system.

It is true that, like all other glandular bodies, they are liable to the varied forms of inflammation, to ulceration, to sloughing, and to diphtheritic deposits; but all these affections are within the scope of remedial treatment.

It must also be borne in mind, and I will casually allude to the circumstance, that there seems to be some pathological relation of the tonsils with the lymphatic glands, and more especially with those in the cervical region. Enlarged cervical glands are often attended with, or followed by, enlargement of the tonsils, and *vice versâ*, and, again, there is one gland found especially comprised in disease of the tonsils, viz. the one near the angle of the lower jaw; this often becomes enlarged, and gives rise to the popular expression of the "kernels having come down." Indeed, the tonsils, when increased in size, are not far off from the angle of the jaw, and this fact has been made use of to advantage in the operation of removal of the tonsil by external incision below the jaw in the neck, and to this I shall refer presently.

The association of enlarged tonsils with cervical glandular swellings is a well-recognised practical fact, and is a matter of much importance when the question of removal of enlarged tonsils by operation is entertained. Many and many a tonsil has been unnecessarily removed where it could have been dissipated by attention to the general health, and the administration of tonics, &c.

Again, the tonsils are sometimes the seat of *new growths*, some of which have recently received the name of *lympho-sarcoma*, as consisting of a new formation, resembling the lymphatic gland-tissue; others, in which the fibrous elements are more prolific, are called *fibro-plastic* tumours; and, lastly, others in which *cancer* degeneration has attacked the gland. It is to this latter class of affection that I have devoted the following pages.

Cancer of the tonsils is a disease inevitably fatal in its results. This rare affection has more especially arrested attention since the publication of two recent and remarkable cases. The one is a case of encephaloid disease of the tonsil, removed by external incision by Dr. Cheever ('City of Boston Medical and Surgical Journal,' 1871, vol. i, p. 390). The other is a case of cancer of the left tonsil, the lymphatic glands, and the spleen, by Dr. Moxon ('Trans. Path. Soc.,' London, 1870, vol. xx, p. 369).

These two cases may be taken as fair examples of the two forms in which cancer may attack the tonsils. The first case is one of purely primary cancer of the encephaloid or most common variety; whilst the second, on the other hand, may be regarded as an entirely secondary formation, developed in the gland probably from transmission through the medium of the lymphatic system, and not, as is usually the case, propagated by contiguity and extension of the disease from the neighbouring involved organs and tissues.

On referring to the medical literature of this and other countries concerning this disease, I felt much disappointed by the meagre details and scarcity of information on the subject. The only article is one occurring in the second volume of the 'Dictionnaire de Médecine,' 1865, p. 150, by M. L. A. de Saint Germain, "Amygdales—lesions organiques—dégénérescences cancereuses et fibro-plastiques."

Although it may be, perhaps, considered somewhat arbitrary, I have arranged the subject according to the text of the two cases cited, viz. the primary and the secondary forms of the cancer of the tonsils.

A. Primary Cancer of the Tonsil.

a. On the frequency of the disease.—There is much difficulty in arriving at any definite conclusion on this point, for in the majority of cases of deaths from cancer the tonsil is scarcely ever alluded to, little or no attention being paid to it in post-mortem examinations. Still, from what slight information can be gathered, these glands do not appear to be a favourite soil for the deposition of cancerous germs.

Lebert ('Maladies Cancereuses,' Paris, 1851) quotes the statistics of cancer, collected by M. Tauchou from the 'Death Register' of Paris and two adjacent arrondissements between the years 1830 and 1840, bòth years inclusive. Of 9118 deaths from cancer, only 3 cases are mentioned as occurring in the tonsils.

Dr. Sibley's statistics of cancer ('Med.-Chir. Trans.,' vol. xlii, p. 111) comprise 520 cases in which the seat of primary cancer was noticed, occurring in the wards of the Middlesex Hospital during 1853, '54, '55, and '56; but he has not made special mention of the tonsil, but has classed it together under one heading, viz., Tonsil, Palate, and Parotid, 6 cases, of which 5 were males and 1 female. And again, in the analysis of 173 post-mortems he gives 4 cases of Cancer of the Throat and Palate, of which 3 were females and 1 male.

Mr. Baker's statistics ('Med.-Chir. Trans.,' vol. xlv, p. 389) are a little more precise as regards the tonsils. The collection comprises

500 cases taken from notes of cases furnished by Sir James Paget, occurring in hospital and private patients between the years 1843 and 1861, the patients being in proportion of two fifths hospital to three fifths private. Of this number there are only two examples of primary cancer in the tonsil, the one in a male, and the other a female. The one was scirrhous and the other medullary; the age of one was between twenty and thirty, the other between forty and fifty.

The average duration of life in cases of cancer attacking the gums, palate, and tonsils, and where the primary disease is not removed, Mr. Baker gives as 12·4 months.

b. On the varieties, symptoms, and characters of primary cancer of the tonsil.—Like as in other structures, cancer may attack the tonsils under two forms, viz. the scirrhous and the encephaloid, and these are often sufficiently well-marked and distinct as to warrant me in assuming a distinction, both as regards their clinical and anatomical characters. The encephaloid variety appears to be the most frequent, and is the most rapid in its progress, and is, moreover, generally attended with encephaloid disease of the lymphatic glands in the neighbourhood; it kills by its prolific growth, blocking up the openings of the larynx and pharynx; the disease seems to involve early the pillars of the fauces, the pharynx, and neighbouring tissues; it projects into the mouth, gradually encroaching upon the isthmus, and rendering deglutition painful and difficult; it soon impedes respiration, rendering the patient's condition peculiarly distressing; there is hunger, without the ability to swallow food; there is impending suffocation, with eager gaspings for breath; and speedy death but too surely carries off its victim. The essential characters are rapidity of growth and rapidity of death.

The scirrhous variety, on the other hand, is by no means so common; it is of but slow growth, and generally attended with ulceration, there being a deep excavated ulcer with hardened base and edges, of the true carcinomatous type, but which must not be mistaken for the syphilitic ulcer of the tonsil. It is generally attended with intense pain, and with difficulty of swallowing; the ulcer gives exit to an offensive ichorous discharge. This form kills slowly, rather by starvation, cancerous cachexia, and exhaustion, than by suffocation. It generally does not immediately attack the cervical glands like the encephaloid variety, by rapidly enlarging growths of brain-like matter, but remains rather as stony hard nodules, little disposed to active extension.

Still, in some instances these distinctive characters may not always be present; nevertheless, the above description may with safety be taken as a fair standard of the two conditions.

I will now briefly detail examples of these forms:

1st. *Cases of primary encephaloid cancer of the tonsil.*—Dr. Cheever's case is a well-marked example of the early condition, but is somewhat modified in character, owing to there having been a previous partial excision of the gland, which led to the existence of an ulcerated surface.

G. M—, a well-formed, robust sailor, æt. 34, without any hereditary predisposition to disease, and in the enjoyment of perfect health previous to his present disability, presented himself at the hospital with the following history :—Six months before, without any known cause, his left tonsil became enlarged and painful. It was treated by his physician as a case of tonsillitis, and in due time a portion of it was excised from within. No relief followed, but instead, the tonsil continued to increase in size, and the region of the section became an obstinate ulceration. His articulation and deglutition were impaired to a considerable degree, and dyspnœa was quite marked, especially at night.

On admission the affected tonsil appeared much enlarged; the mass protruded into the fauces, and at its apex presented an indolent ulcerated surface, an inch and a half in diameter, with raised and everted edges. Externally corresponding with the internal growth, and moving with it as if it were a part, was a nodule lying in the left submaxillary triangle of the size of an English walnut. Manipulation of this mass gave pain. The condition of the man was otherwise excellent; there was neither the history nor any appearance of syphilis.

He was able to take only liquid diet. The discharge from the ulcer being offensive, he was ordered a gargle of the Liq. Sodæ Chlorinatæ.

During an interval of three weeks the tumour doubled its size, internally and externally. The consequent symptoms became much more grave, and called for some operative interference. The situation and large size of the tumour, as well as its projection outside the throat, contra-indicated any operation from inside the mouth, and it was therefore decided to attempt removing it from the outside by external incision, which was accordingly performed.

Examination of the tumour after removal.—It was of a soft and friable nature, slightly lobulated, and of a greyish-red colour. On section it yielded an abundance of juice of a milky colour, and of considerable consistency. Under the microscope both the tonsil and the enlarged lymphatic gland appeared the same; they were composed of cells of moderate and uniform size and ovoid form, containing nuclei, and many also nucleoli. There was no fibrous tissue between them, but a great number of small dark granules, appearing to be freed nucleoli. On the addition of acetic acid, the nuclei became more distinct, and cells were visible, containing three or four of them.

One of the earliest cases on record is quoted by *Lobstein* ('Traité d'Anat. Path.,' Paris, 1829, vol. i, p. 430), as occurring in the hospital under *M. Cailliot.* A man æt. 63 had been the subject of partial paralysis from nervous apoplexy, and was recovering, when he experienced a difficulty in breathing. On examining the throat the right tonsil was much enlarged; by means of the finger a small portion, about the size of a very large nut, was detached, and which had all the appearances and consistence of encephaloid disease. Some months afterwards the whole of the isthmus of the fauces became blocked up by the enlargement of both tonsils, producing difficulty of breathing and threatening suffocation. He continued a miserable existence for two months, and then died quietly without pain.

On examination both tonsils were found in a state of encephaloid degeneration; the lymphatic glands on both sides of the neck had undergone the same alteration. Small encephaloid tumours were found at the base of the epiglottis and on the arytenoid cartilage.

Velpeau ('Traité de Méd. Opérat.,' tom. iii, p. 568) has met with five examples of cancer of the tonsil, and all belonging to the encephaloid variety. He merely states that their situation and relation with the large vessels of the neck have hitherto intimidated surgeons, and preventing them from meddling with them; he, however, once practised extirpation in 1836, but did not find the operation difficult. He gives no further account of these cases, otherwise than the one in which he operated upon; it was that of a peasant æt. 68, who came into the hospital to be relieved of a swelling of the left tonsil, which had first shown itself about two years previously; the tumour was now bleeding, ulcerated on the surface, and blocking up completely the pharynx; it was penetrating into the nostrils, pushing the soft palate forwards, and was threatening suffocation. He determined to give the man a chance for life, and removed it from within the mouth. The patient died on the eighteenth day after the operation from purulent infection, diarrhœa, and prostration. On examination the whole of the cancerous mass was found to have been completely removed by the operation, and there was no appreciable disease in any of the viscera.

Vidal mentions that he had seen one case that occurred in the Chirurgie de la Charité in 1838, having been under the care of M. Velpeau, and it is probably one of the cases cited by Velpeau.

Roux is quoted by M. Fano ('Bull. de la Soc. d'Anat.,' 1846, tom. xxi, p. 109) as having removed a tumour of the tonsil from a female æt. 40, who appeared otherwise healthy. He had also to extirpate by external incision a tumour behind the angle of the jaw. Under the microscope both these structures had all the characters of encephaloid disease.

Lebert (op. cit., p. 422) observed this form of cancer in a very aged female, who succumbed in the space of a few months; the glands of the neck were much enlarged, and were the seat of cancerous deposition. At the autopsy there was no localisation of cancer in the other organs.

Erichsen ('Science and Art of Surgery,' 1869, vol. ii, p. 370) had a case of encephaloid disease of the tonsil under his care, when he obtained some temporary advantage by removing portions of the soft, projecting, and very vascular tumour by means of the écraseur.

My colleague, Mr. Bryant, has furnished me with two cases, occurring among his out-patients at Guy's Hospital.

Henry S—, æt. 62, leather-dresser, had *soft cancer of the left tonsil* of six months' standing; it was accompanied *with enlarged cervical glands* of three weeks' duration. He had pain in the left ear, and for two months some difficulty in swallowing. He continued his visits for three months, when he ceased his attendance, and was evidently sinking. His mother had cancer of the lip, which was removed at the age of forty-six, and she was still living, at the age of eighty-eight, quite well.

James S—, æt. 65, had *soft cancer of the right tonsil,* involving the pillars of the fauces; it was in a state of ulceration. There were also *enlarged cervical glands;* it was of six weeks' standing. As nothing could be done, he ceased his attendance.

2nd. *Cases of scirrhous disease of the tonsil.*—As I have before observed, the scirrhous variety is a very rare form of the disease, and it is doubtful whether some of the cases considered and recorded as such were not instances of chronic hypertrophy and adenoid conditions. Again, several cases are recorded as cancer, without reference to their variety being generally classed under the head of scirrhus and epithelioma.

Dr. J. C. Warren, in his work on tumours (p. 356), was the first to bring under special notice this form of disease; but he asserts "that scirrhous affections of the tonsils are not very rare," and he only adverts to two unsatisfactory cases as examples. He says "that, like those of the palate, they are slow of growth, not painful, and not much disposed to assume the cancerous ulceration; the disease is difficult to eradicate, as it runs into the substance of the pharynx."

One of his cases he describes pretty fully, and the case is again published by his son, J. Masson Warren, in his 'Surgical Observations,' 1869, p. 124, and hence the case has in some instances been quoted as two separate examples. The son's account is the following:

The patient was a Mrs. A—, æt. 65, who observed in January, 1835, a swelling on the left tonsil, which gradually increased in size, became more firm in consistence, and finally extended to the soft palate and attached itself to the lower jaw, so as materially to impede its motions. She was not much emaciated or reduced in strength; the countenance was pale and digestion good. On examination, on the left side of the throat and occupying all the back part of the fauces, was a firm, indurated tumour, extending backwards and upwards into the posterior nares, and forwards to the lower jaw, *to which it was firmly attached.* The tonsil on that side and soft palate were all implicated; there was no doubt about the cancerous nature of the disease. J. C. Warren, the father, states that she was only sixty years of age, that she had been labouring under the disease for some years, and that the respirations were impeded and deglutition difficult; and that she had the appearance of a person sinking under the pressure of disease; that he was urged to perform the operation of removal immediately, in consequence of the distressing state she was in. The tumour was removed through the mouth, and she recovered, after some exfoliation of bone had taken place. She died eight months after the operation from an independent disease, viz. peritonitis, and on examination there was remarkable displacement of the viscera, but no disease of any organ is recorded. The tumour was found to be of a firm, cartilaginous nature, almost of a bony hardness, somewhat ulcerated in its centre.

This case was, no doubt, one of enchondroma of the jaw, and not one of scirrhus of the tonsil.

Warren's second case is a mere casual allusion to a case of scirrhus of the tonsil, in which he cast a loop of wire round the growth, so as to strangulate it; but this produced at the end of five days symptoms of tetanus, so that he was glad to remove the wire, but the tumour afterwards sloughed away.

Erichsen, in his work 'On Surgery,' 1869, vol. ii, p. 370, stated that he has seen instances both of scirrhus and of epithelioma in this organ as a primary affection, but his description of the disease tends more to the encephaloid variety. He has lately published in the 'Medical Times and Gazette' for June 24th, 1871, p. 714, an interesting case, which may be regarded as a fair example of the scirrhous kind.

Eliza B—, a married woman, æt. 50, admitted on May 1st, 1871, with a tumour of the tonsil, apparently cancerous. She stated that she had always enjoyed good health, and that her parents had both lived to a great age; her five children also were perfectly healthy. Last October she caught cold, and with it sore throat, and when this had continued for a month she noticed a swelling in the throat, which had steadily increased, with much pain and soreness. She noticed at the same time that the neck glands became swollen, but these after a time subsided somewhat, although still much enlarged. Her appetite now began to fail, and she lost flesh rapidly.

On admission there was found a large tumour occupying the position of the left tonsil. This growth reached down out of sight, below the root of the tongue, and on a digital examination its lower extremity was found to be about on a level with the epiglottis. It was nodulated on the surface, and the mucous membrane covering it was slightly redder than natural. The soft palate was not implicated by the growth; it was not tender on pressure, nor was there any ulceration on the surface; it was of firm consistence, and covered with thick white mucus. The glands beneath the jaw on the left side were clearly visible, forming rounded prominences as large as a filbert. They were perfectly free from adhesions, were hard, and not at all tender.

It was not deemed prudent to attempt any operation for the relief of the disease in so advanced a stage.

Dr. Burnett, of Biggleswade, is reported by Dr. Tanner ('Practice of Medicine,' vol. ii, p. 11) to have had the care of a woman æt. 68, whose pharynx was much obstructed by a firm medullary cancer of the left tonsil, and where the disease was completely excised, but with only temporary relief. On a private communication with Dr. Burnett for further information, he writes to say that but few notes were taken at the time; the woman was of advanced life, æt. 65 or 66, and had always been exceedingly regular in her habits. The disease was one of scirrhus, and not encephaloid, but it had proceeded to ulceration and sloughing; the mass was about the size of a small pigeon's egg, and was removed by ligature and excision; no hæmorrhage occurred. She had but slight difficulty in swallowing and respiration; there was very great emaciation. She stated that the disease had existed quite two years. She was operated on late in February, and died early in May; there was no return of the disease, but she had enlargement of the cervical glands, and she died from general exhaustion. No post-mortem was made, as it was objected to on the part of the friends.

One of the most remarkable cases of scirrhus of the tonsil occurred among the out-patients at Guy's Hospital in January, 1863, under the care of Mr. Bryant, to whom I am indebted for the following notes:

William S—, æt. 17, a chimney-sweep, who had never had syphilis; he applied to the hospital, suffering from a tumour of the right tonsil, of six months' standing, and which was now in a state of ulceration. On examination it was found to be of a truly carcinomatous nature; it was of stony hardness, and the ulceration assumed the aspect and characters of cancer. The disease steadily progressed during the four months of his attendance at the hospital; he became extremely emaciated, and was evidently sinking fast when last seen.

Mr. Bryant had another case in the year 1869. David B—, æt. 49, who was admitted as an out-patient, suffering from scirrhous cancer of the left tonsil, and having a deep excavated ulcer of the same; there was, moreover, a glandular swelling to be observed externally. The disease had been in progress for two years; and he attended the hospital for only two months, the disease having gradually spread, and his health was failing fast; in fact, he was sinking.

It is to be regretted that these cases were lost sight of, and no opportunity afforded of studying the pathological changes.

M. Houel ('Bull. de la Soc. de Chir. de Paris,' 1869, vol. ix, p. 162), who had had large opportunities of studying pathological anatomy, observed only one case of cancer perfectly limited to the tonsil. It occurred in a medical man, and was removed by M. Nélaton. There is, unfortunately, no record of the case.

M. Desormeaux (loc. cit.) has seen a case where the left tonsil presented an ulceration, which was regarded by M. Ricord as syphilitic, but which subsequently was ascertained to be of a cancerous nature, and was confined to the tonsil.

M. Demarquay ('Bull. de la Soc. de Chir. de Paris,' 1862, series ii, vol. iii, p. 163) relates a case in which the disease had extended to the pillars of the fauces and to the base of the tongue, and which he removed by the écraseur. He says—

"I was called to a strong robust man, æt. 51, who had an ulcerated tumour occupying the right tonsil, the anterior and posterior pillars of the velum, and also the tongue at corresponding side. The disease appeared five months previous to admission; he had undergone several treatments, and chiefly iodide of potassium.

"The right tonsil was occupied by a cancerous tumour ulcerated on its surface. The disease was not confined to the tonsil, but involved the mucous membrane of the pillars, and part of the velum; below, it extended to the border of the base of the tongue, the tumour did not project considerably into the throat; its circumference was very irregular, and was continuous under the line of demarcation with the mucous membrane of the neighbouring parts, from which it was distinguished, however, by the hypertrophy of the glandular follicles and deeper colouration; the centre of its surface was rough, and the ulcer gave exit to a small quantity of ichor mixed with pus. There was slight induration on manipulation, and the tonsil glided over the deep parts; there was no lymphatic enlargement. Deglutition was embarrassed, and he was losing strength."

3. *Cases of cancer of the tonsil, in which the character of the growth is not specified.—M. Chassaignac* ('Bull. de la Soc. de Chir.,' op. cit.) stated that he had met with several cancers of the tonsil, and in one case he removed it with the écraseur.

M. Maissoneuve (op. cit.) had also observed several cases of cancer of the tonsil, which would have required section of the jawbone for their removal. He had not done so, but had effected a sloughing away of the mass by means of "cauterisation en flèches" in one case.

4. *Cases of doubtful character* (lymphoma and lympho-sarcoma.)—*Dr. Meissner,* in his report on cancer ('Schmidt's Jahrbuch.,' 1870, Bd. 146, p. 314), quotes a case of primary lympho-sarcoma of the tonsil as an instance of cancer. It is recorded by Dr. G. Milani, and occurred in a young woman æt. 17, and was first observed in January, 1869, as a red swollen condition of the left tonsil; in June of that year it was of the size of a goose's egg, and produced difficulty of breathing and swallowing; it then rapidly increased to an enormous size, pressing down the epiglottis; the cervical glands became affected with the same disease, as also the left lobe of the thyroid gland. The disease was considered to be primary lympho-sarcoma of the tonsil, spreading to the lymphatic glands of the neck.

Virchow, under the article on "Lymphoma," says—

"The worst cases are those where the tumours quickly grow to large *medullary* tumours (not unfrequently under the form of fasciculated medullary fungi), and where the neighbouring tissue is also changed to lymphoma. Patients with such tumours rarely escape; anæmia comes on, the nutrition is impaired, and hypertrophy of the spleen may appear, and the patient die of excessive anæmia and marasmus. These malignant lymphomata, which Lücke calls lympho-sarcomata, cannot be anatomically distinguished from the benignant forms. But they may be recognised from the fact that they proliferate rapidly, and especially that they unite with the parts immediately around. It seems to me they are certain to recur, and are the most dangerous of tumours."

Mr. Lawrence, in his 'Lectures on Surgery,' published in 1863, p. 611, gives a most interesting case of doubtful malignant disease of the tonsil. He removed a portion of the tonsil, and it was pronounced to be simple hypertrophy of the gland; this was succeeded by a return of the disease, and extirpation of the whole of the tonsil performed. Soon afterwards he was again admitted into the hospital with a malignant affection of the cervical glands. The case is thus described:

A man about forty-five, who had always enjoyed good health, his occupation through life having been agricultural, came into St. Bartholomew's Hospital under my care for an enlargement of the right tonsil, presenting the usual character of hypertrophy, and obviously requiring excision; the only complaint was of some difficulty in swallowing. The enlargement was rather beyond the bulk that could be passed into the opening of the guillotine. I therefore cut off as much as protruded; the part removed was simply hypertrophied tonsil. He returned to the country, but came again to the hospital in the month of October with a swelling as large as an egg, proceeding from the former site, and seeming to fill up the pharynx, so as to produce a formidable impediment to swallowing. This I removed with the écraseur, getting the loop of the chain over the lower end of the swelling, which was at some distance below the tongue, drawing up the ends, so as to include the root of the mass, and then fixing them to the movable branches of the instrument, the action of which caused so much choking feeling and involuntary efforts of the surrounding parts, that it was necessary to hasten the process, which was accomplished almost without loss of blood. The part removed appeared as an entire tonsil, simply enlarged by hypertrophy, with slight surrounding covering of cellular tissue in a perfectly natural state. The section presented a substance of very light brown tint, similar to that of the natural gland, and in a lobular arrangement. Mr. Savory reported that a most careful microscopical examination detected nothing but gland elements.

This patient once more came to the hospital in July, with a swelling, apparently glandular, larger than my fist, under the right sterno-mastoid, of firm but not scirrhous hardness, and covering closely all the important structures at the side of the neck, up to the angle of the jaw. It was not painful, and had not been so during its increase, which had been rather rapid.

My colleague and myself regarded this swelling, which had attained its great bulk within a few months after the removal of the primary disease, as a malignant affection, deciding at once and unanimously that it was not a fit case for operation. If it could have been considered as an innocent growth, I should not have hesitated to remove it.

Considered as a secondary fatal tumour, consequent upon a primary disease to which no suspicion of malignancy could be attached, it is of no slight practical importance.

On the diagnosis of cancer of the tonsil.—In the early stage of both forms of the disease there is no distinguishing mark to guide us as to the nature of the disease. Enlargement of the tonsil is the only sign, and this does not arrest the attention of the patient, nor excite any suspicion in the mind of the surgeon in consequence of the very frequent occurrence of subacute and chronic inflammation of the glands in a very great majority of persons. Hypertrophy of the tonsils seldom alarms or produces any inconvenience unless both tonsils are affected and thus block up the isthmus of the fauces; and seldom are any operative measures requisite, excepting under these conditions or when respiration is interfered with. As the disease advances, the peculiar nature of the fatal disease begins to develop itself, and assumes either a rapid or slow course; when rapid, it steadily encroaches upon the fauces and pharynx, involves the lymphatic gland at the angle of the jaw, and afterwards the cervical glands, and soon destroys the patient; this form of the disease is not liable to be mistaken for any other affection. Encephaloid cancer, wherever it may be, is unfortunately seldom to be mistaken.

The scirrhous variety, on the contrary, may often fail to be recognised; but its slow progress and its becoming ulcerated and excavated on its surfaces (an attempt of nature to enucleate the disease) renders it less liable to be confounded with chronic hypertrophy and syphilitic ulceration; however, both these diseases have passed for cancer, and on the other hand cancer has been presumed when subsequent results have disproved the supposition. Excessive hardness, implication of the lymphatic glands, peculiar ulceration, fœtid discharges, increasing growth and peculiar cachexia seem to be its characteristics.

M. de Saint Germain thus adverts to its diagnosis: If at the outset the cancer appears with the aspect of a benign hypertrophy, or if on the other hand the jagged appearance of the tonsil, the ulceration of the orifices of the lacunæ, in a simple hypertrophy may lead one to think of a cancerous degeneration, there arrives a period when the presence of stony hardness, of sanious offensive ulcerations, of signs of cancerous cachexy, and the progress of the malady, will allow its nature to be appreciated.

The syphilitic changes of the tonsil most often confounded with cancer are the "gummous" tumours at different periods of their development, and especially at the time of their ulceration. Indeed it is an error with difficulty to be avoided in the absence of knowledge of the antecedents of the patient, or at least of the absence of proof of a specific treatment.

Another kind of syphilitic lesion may, moreover, be a source of error. It is the hypertrophy and vegetations of "plaques muquensis" of the tonsils.

M. St. Germain quotes the case mentioned by Fournier, where a syphilitic tumour of the tonsil simulated cancer, and was cured by the internal exhibition of the proto-iodide of mercury. The isthmus of the fauces in his case was completely blocked up—its right half by a large tumour, seemingly developed from the tonsil and pillars of the fauces on that side—the surface thereof was shining, greyish at some points, rugose, granular and ulcerating; its consistence was firm and there was redness and tumefaction of the parts around. It was at first considered to be epitheliomatous, but on careful examination into the history of the case, evidences of syphilis were found.

Blandin ('Bull. de la Soc. de Chir. de Paris,' 1862, p. 467) removed a tumour from the tonsil which was regarded as cancer, and a recovery resulted. The disease shortly afterwards returned, and the patient then came under the care of M. Maissoneuve, who placed him under iodide of potassium, and an effectual cure resulted. The case proved to be a syphilitic tumour of the tonsil, and not cancer.

Dr. J. C. Warren's case may also be considered as one of enchondroma, mistaken for scirrhus; the history and clinical details of the case, the evidence of the tumour itself after removal, and the absence of any cancerous cachexia, fully justify such a decision.

Roux diagnosed a cancer of the tonsil, and was about to remove it when the patient died. On examination there was found besides some small isolated tumours on the epiglottis, and another at the orifice of the Eustachian tube. On microscopic examination the tumours of the tonsil and throat were found to be of fibro-plastic nature.

Dr. Cheever thus sums up the diagnosis of his case:—In my case there seemed to be no doubt as to its being cancer. The *growth* of the tumour as distinguished from the ulceration and waste of syphilis was one marked point. The tumour of the tonsil doubled in three weeks. The gross and microscopic appearances were submitted to several observers, who were of opinion as to its being cancer. The enlargement of the lymphatic gland and the identity of its structure under the microscope with that of the tonsil, were other strong points indicating cancer. The whole aspect of the man was singularly free from syphilitic taint.

On the treatment of cancer of the tonsil.—We cannot but admire the bold and novel operation of Dr. Cheever in extirpating an encephaloid tonsil and an enlarged gland at the angle of the jaw by an external incision in the neck;—the ease with which he carried out his plan, the little hæmorrhage, the enucleation of the tonsil by means of the finger, and the success attending the case almost persuades one to recommend the operation in other similar cases. Before alluding to his operation let us consider the usual and general treatment hitherto adopted. In the early stage of the disease, when its nature cannot satisfactorily be agreed upon, the case must be treated as one of chronic or subacute inflammation of the tonsil, by such means as are recommended for such affections. But above all a careful scrutiny of the history of the case must be instituted respecting the probabilities of a syphilitic taint, and the treatment directed accordingly; and even should there be no evidence of syphilis, still I should be disposed to give the benefit of a doubt by treating it with the specific remedies for that disease, taking care not to lower the patient too much in the event of its being considered necessary to remove the disease by ablation.

When the disease has so far advanced as to have developed its terrible character, active measures must be taken before the lymphatic system becomes too far contaminated.

a. Caustics and escharotics have been recommended and employed, but these generally aggravate the pain, and cannot be borne for any length of time, and indeed often aggravate the disease.

M. Maissoneuve ('Bull. de la Soc. de Chir. de Paris,' 1859, tom. 9, p. 162) suggested "cauterisation en flèches" to be applied to cancer of the tonsil, and he presented to the society a mass of tonsil (which had the appearance of a ball of mastic) removed successfully by him. He plunges these "flèches" of caustic paste into the gland, and at the end of a few days the tumour is destroyed. Should the patient swallow the mass it will not poison. The members of the society entertained some doubts about the nature of the disease, and were not disposed to recommend his treatment for cancer of the tonsil.

b. Removal by the amygdalotome or guillotin is generally quite out of the question in consequence of the size of the tumour.

c. Removal by the wire ligature.—This was performed by Dr. J. C. Warren in one of his cases; he cast a loop of wire around the diseased tonsil, and twisted it until the tumour was strangulated; its effects were distressing and alarming; the patient suffered atrociously from difficulty of swallowing, salivation and dyspnœa; at the end of five days symptoms of tetanus appeared, and then he was glad to remove the ligature without delay. However, the tumour was destroyed and sloughed away. He considers the ligature objectionable.

d. Removal by the écraseur may be undertaken when the tumour has not attained any large size, and when the loop of the instrument can readily embrace the whole base of the tumour; but danger is attached to this operation at all times, as the loop may include some of the important vital structures in the neighbourhood, viz. the internal carotid, jugular vein, and pneumogastric nerve. To obviate this, Demarquay had recourse to Blandin's procedure of a temporary incision in the neck so as to lay bare the structures he wished to avoid, and having them held aside by an assistant, then introducing the finger and conducting the loop of the écraseur over the tumour from within the mouth; he thus so far kept all the important vessels and nerves from the grasp of the loop. His description of the operation is as follows:—The case was one of ulcerating cancer of the tonsil, palate, and advancing on the base of the tongue; there was no glandular enlargement, and iodide of potassium had failed.

Having carefully ascertained the extent of the disease, I thought I could remove the whole with the linear écraseur; but in order to avoid comprising within the loop of the écraseur any important structure, I made an incision four finger's breadth along the course of the internal border of the sterno-mastoid. I reached easily the vessels and nerves in contact with the tonsils, and separated them; having done this I proceeded slowly to remove the tumour with the chain of the écraseur; and having first made an oblique incision through the velum palati to the right of the uvula, and then carefully using Chassaignac's instrument, I seized the tonsil with hook forceps and drew it forwards to the side of the mouth, and threw the chain around the mass comprising the tonsil, pillars, and a large piece of the right portion of the velum. During the proceeding with the écraseur, I took care that it did not pass beyond the prescribed limits. There was no hæmorrhage. I removed afterwards with the curved scissors a portion of the tongue in relation with the tonsil, as also some hypertrophied glands. I closed the preliminary incision, which united in a few days. The conditions inside the mouth proceeded satisfactorily, although he had spitting of blood for forty-eight hours, which ceased under the use of ice. The operation entirely succeeded; his health was restored, and he became stout. However, a return may take place.

By thus making a preliminary incision he not only secured the carotid from injury, but was enabled to ascertain that the cancer had not extended along the pharynx. ('Bull. de la Soc. et de Chir. de Paris,' 1862, p. 467.)

e. Removal by incision from within the mouth.—J. C. Warren describes it as a bloody and dangerous operation, requiring also to be aided by the actual and potential cautery. In his case, the jaws being fixed apart by an assistant, the tumour was seized with a pointed hooked forceps, and then with a round-edged knife the whole mass was removed by two strokes of the instrument; an enormous gush of blood followed. Passing the finger in, another mass was felt in the pharynx, below the situation of the former one, which was seized and cut out by a curved probe-pointed bistoury. When the hæmorrhage had a little subsided, a red-hot iron was passed on a conductor to the diseased spot twice; no further hæmorrhage occurred. The woman recovered. However, subsequent to the operation several applications of caustic potash were required, and exfoliation of a small piece of bone followed.

Velpeau performed the operation upon a man æt. 68, where the mass filled up the pharynx, and suffocation was imminent.

Velpeau first cut down npon the carotid, and placed a controlling ligature under it, to be tied if the occasion should require it; he then hooked the tumour deeply, drawing it forwards towards the middle of the mouth, and with a short knife, curved on the flat, he divided the left side of the soft palate, whereby he was enabled to uproot the entire mass from below upwards, and from within outwards. Seeing that there was no hæmorrhage, he next removed the enlarged lymphatic gland that was situated at the lower part of the parotid gland, pressing against the pharynx, by an external incision. The ligature having proved unnecessary, was removed the next day.

All went on well for the first week, when the patient was attacked with symptoms of purulent infection, diarrhœa, and prostration on the tenth day, and death on the eighteenth day after the operation. ('Med. Operat.,' t. iii, p. 568.)

f. Operation for removal of the tonsil by external incision, as performed by Dr. Cheever, and thus described by him:

Etherization was slow and difficult on account of the obstruction to respiration by the tumour in the fauces. As soon as it was accomplished an incision was made, extending from just within the angle of the jaw downward, over the most prominent part of the tumour, a distance of three and a half inches, and in a direction parallel with the sterno-mastoid muscle; this incision was met by another, one inch and a half long, extending along the lower border of the jaws. The parts were dissected away on either side until the diseased growth was reached. On enucleation this was found to be an enlarged and diseased lymphatic gland, of the size of an English walnut. It had no distinct connection with the tonsil within, but the disease was encephaloid in character. It lay outside all important structures, and was entirely removed without difficulty. The dissection was now extended until the tonsil was reached. In its course the digastric, stylo-hyoid, and the stylo-glossus muscles were divided, the stylo-pharyngeus being left intact, on account of its proximity to the glosso-pharyngeal nerve. The fibres of the superior constrictor of the pharynx were picked apart with a director, and the pharynx thus opened between them. The finger of the operator was now enabled to sweep entirely around the diseased tonsil, the pillars of the soft palate being left intact; the mass was removed, and presented all the appearance of encephaloid disease, its size being of a pullet's egg. The hæmorrhage during the operation was free, but not excessive; the largest vessel divided being the facial artery, which was cut close to the carotid. Twelve ligatures were applied.

A few of the smaller branches of the facial nerve were divided, and paralysis of the lower lip on the same side was the consequence. It was also observed that on account of the section of the stylo-glossus muscle the tongue, when protruded, took a direction towards the opposite side. A single suture closed the horizontal incision; the wound otherwise was left freely open, air passing through it with each expiration. Recovery from ether was speedy, and there was no marked depression from the operation.

Not a single complication occurred to hinder the progress of recovery. Suppuration of a satisfactory character commenced on the second day. During the first week after the operation the patient took liquid nourishment, administered by means of the stomach-pump. From the outset there was no pain, and respiration was easy. The granulating process was rapid, and at the end of eight days no fluid passed through the wound in deglutition.

After eleven days a small patch was observed at the lower part of the posterior pillar of the palate, which had the appearance of the original malignant growth; it was freely cauterized with nitric acid, and there was no subsequent reappearance.

After seventeen days solid food was swallowed without difficulty. In thirty-one days the wound had entirely closed. The pharynx was entirely clear, and except that the pillars of the palate on the side affected were somewhat separated, it appeared in perfectly normal condition. The tongue was protruded in a straight line, and no paralysis of the lip remained. From the operation until recovery there was no constitutional disturbance requiring special notice.

Dr. Cheever, in his remarks, adds:

"The facility with which the tonsil can be enucleated with the finger is surprising. The following anatomical peculiarity, however, explains the reason pretty well."

Chassaignac thus describes this point:

"It is enclosed in a semi-fibrous capsule; when one has enucleated a well-developed tonsil, and examined with attention its external and internal surface, it is found that the external or adherent surface is covered with a fibrous semi-capsule, well circumscribed and independent of the neighbouring aponeurosis, and resting on the cellular tissue."

Dr. Hueter ('Jahresbericht der Gesammten Medicin,' 1869, Bd. ii, p. 435), in his review of Cheever's case, says:

"Dr. Cheever's assertion that as yet no one had extirpated the tonsil from without is not correct. He has seen the operation performed by Langenbeck in 1865, and a second case he himself performed in 1865. In both cases the lower jaw was sawn through at the second molar tooth, and the jaw temporarily displaced upwards so as to lay bare the tonsil. Both cases were instances of large sarcoma of the tonsils. Langenbeck's patient recovered; Hueter's case was operated on in the sixth month of pregnancy, and died in three weeks from pneumonia. Both cases have as yet not been published.

B. Secondary Cancer of the Tonsils.

It is a general impression that cancer of the tonsils is a secondary disease, but the foregoing facts give ample evidence of its occurrence as a primary affection of the gland; still, the subject of secondary deposition is one requiring much further investigation in order to form any definite conclusions. The most frequent mode of its occurrence is undoubtedly by the extension of the cancer from the neighbouring tissues, more especially from the base of the tongue and pharynx; but it may likewise be independently deposited during the progress of the disease elsewhere in other organs and tissues. It is remarkable that in the numbers of post-mortem examinations made of cancer patients, the tonsil is scarcely ever mentioned; it is more than probable that the tonsil has never undergone scrutiny. From what few records exist this secondary affection seems always to have been associated with cancerous disease of the lymphatic system, and in two of the four instances cited, combined with similar disease in the spleen. Secondary cancer of the tonsils seldom permits of any surgical interference, with the exception of the operation of tracheotomy for impending suffocation. It is a lesion of purely pathological interest, and I shall, therefore, merely give an account of such cases as are considered to be examples of the disease.

Case I.—*Lobstein* ('Traité d'Anat. Path.,' vol. i, p. 429, 1829) states that "in 1821 a man æt. 75, tall, and enfeebled by disease for several months, and had suffered from dysphagia, so that he could not swallow the least thing, had at the upper and anterior part of the thigh a tumour as large as a fœtal head; it was indolent, and without any alteration in the integument over it. This man died a few days after his admission from suffocation. On examination the tumour in the thigh was lobulated, like the convolutions of the brain, resting upon the femoral vessels and nerves; it weighed 2 lbs., and had the appearance and consistence of cerebriform fungus. The inguinal and lumbar glands on the same side, and all the glands in front of the vertebral column, were similarly degenerated, extending from the coccyx to the first vertebra of the neck. What was most remarkable, the amygdalæ were very much enlarged, and converted into the same cerebriform substance.

Case II.—*Dr. Carswell* has recorded an interesting case of cerebriform cancer of the tonsils, lymphatic glands, and spleen, and the drawings illustrating this case are preserved in the Museum of University College.

The case is casually alluded to in 'Walshe on Cancer;' but the general outline and description is to be found in Dr. Hodgkin's paper "On some Morbid Appearances of the Absorbent Glands and Spleen," in the seventeenth volume of 'Med. Chir. Trans.,' p. 90, 1832.

The patient, who was between thirty and forty years of age, stout made and not lean, had been affected with swelling of the glands under the jaws, along both sides of the neck; in the axillæ and groins, for between three and four months, and from which he had suffered but little inconvenience, to which he had paid but little attention, and had employed no remedies. It was only a short time before he applied to be taken into the hospital, that he felt a difficulty in swallowing, which rapidly increased, and for the last two or three days was such as to prevent him from taking any kind of food whatever. He was admitted into St. Louis' Hospital at Paris in the month of April, under the care of Dr. Lugol. As his appetite had never been affected by the disease, he was now in a great state of suffering, not only from want of food and from debility, but from the idea he was rendered incapable of satisfying the cravings of hunger, together with the prospect of inevitable death. He lived rather more than two days.

Inspection of the body.—On each side of the neck were large groups of glands, extending from the angle of the jaw down to the clavicle, where they were joined to another group, coming up from the axillæ, and passing under the clavicle. The submaxillary and sublingual glands were greatly enlarged, and united with the other lymphatic glands, formed an almost continuous chain, stretching along the border of the jaw, and uniting under the chin. These glands were of various sizes; some of them were not larger than a pea, while others were as large as a hen's egg; they were round, oval, or of an irregular form, particularly where they were united by a common capsule. A great many of them presented the colour which distinguishes them in the healthy state; others were of a yellowish tinge, with more or less redness and vascularity, whilst a few were of a deep red colour, and highly vascular. The greater number of them when pressed between the fingers felt pretty firm and somewhat elastic; those that were red and vascular were softer. All of them were enclosed in a thin but firm capsule, which contained a substance of the colour and consistence of brain, and in which were distributed a considerable number of blood-vessels, and on the softest the vascularity was such as to give to the cerebriform matter an appearance resembling a mixture of equal parts of brain and blood. A similar state of the glands was observed in both groins. The greater number of them were as large as pigeons' eggs, and could be followed passing upwards under Poupart's ligament, surrounding the great blood-vessels, and terminating in the diseased lymphatic and mesenteric glands. The diseased appearance observed in the glands of the groin are represented in Nos. 4—6, fig. 1; those of the neck and axillæ No. 4 *a*.

In Nos. 4—6 is seen the appearance of the substance of which the glands were formed; in one of them the vascularity of this substance is shown to be very great, whilst in the others the vessels are few in number, long, and slender. The quantity of cerebriform matter is also seen to differ considerably in each. Besides, in the lower figure the lobulated structure which it presents is pretty well marked.

In fig. III two of the glands are represented after having been injected. In the upper one a large vein is seen coming out from it, and arising from a great number of minute vessels, which apparently are situated near the surface of the gland. In the lower one the corresponding artery is shown, dividing and subdividing into an immense number of extremely fine branches, which are distributed throughout the substance of the gland.

No. 4 *c*, fig. I, represents an enormous tumour formed by the lymphatic glands situated near the liver, duodenum, pancreas, and great blood-vessels of those parts. It was as large as an adult's head, projecting forwards on a level with the convex surface of the liver, and carried before it the duodenum, pancreas, and gall ducts, which pushed over its anterior surface.

Fig. II represents a section of this tumour, which is seen to be formed of a great number of glands, some of which are as large as a small orange. Like those of the neck and axillæ, they were composed of cerebriform matter, possessing a greater or less degree of vascularity. In the centre of the tumour considerable hæmorrhage had taken place; the centre of the hæmorrhagic effusion was occupied by coagulated blood, and the circumference by layers of fibrine. The vena cava and aorta passed through the tumour, and the former was nearly perforated by one of the diseased glands.

No. 4 *c* represents the same pathological conditions in the glands situated in the posterior fauces. The glands situated around the root of the tongue were so much enlarged as to shut up completely, by their projecting backwards and forwards, the posterior nares and superior aperture of the œsophagus. I could not ascertain the precise state of the epiglottis, but it must, to a certain extent at least, have been free, as it did not appear that inspiration had been much impeded. *The amygdalæ,* formed entirely of cerebriform matter, presented a pale yellow colour, tinged here and there with red specks, produced apparently from the rupture of minute blood-vessels. They have also lost that characteristic appearance from which they derive their name, having become almost perfectly smooth from the accumulation of the cerebriform matter and the distension of their envelope.

The spleen was the only organ apart from the lymphatic glands which in this remarkable case presented a similar, or indeed any, disease. The external surface of this organ is shown in No. 4, *a,* Fig. I. Besides great increase of its bulk, it presents externally a great number of irregular elevations surrounded by redness and vascularity. When divided longitudinally (Fig. II), it appeared to be formed entirely of cerebriform matter and fine blood-vessels, hardly any trace of its natural structure being observable. It presented a lobulated structure, the lobules varying from the size of a small pea to that of a large gooseberry, these being again divided and subdivided into smaller ones; the boundaries of the lobules and the intersections of the latter were the parts in which vascularity was greatest; it did, indeed, appear as if the lobulated structure had been the result of a vascular network, so disposed as to enclose and separate, more or less completely, portions of different sizes of the cerebriform matter. It depended, however, in all likelihood, on the structure of the spleen, in the cells of which, or in the blood which they contain, the cerebriform matter was deposited or formed, while the blood-vessels which surrounded the lobules, and ramified in their interstices, arose from those which belong to the splenic cells.

The body having been removed by inadvertence before I had time to examine the chest, I did not ascertain the state of the bronchial glands; but I was informed by one of the house physicians that they were not diseased.

Dr. Hodgkin, in his criticisms on this case, writes thus:—"Although the doctor has employed the term cerebriform matter, which conveys a ready idea of the texture of the diseased glands, he will excuse my differing from him so far as to regard the affection in this case as distinct from cerebriform cancer."

Case III.—*Sydney Jones* ('Trans. Path. Soc.,' London, 1856, vol. viii, p. 369) exhibited at the society a tumour weighing between 6 lbs. and 7 lbs., formed by encephaloid disease of the cervical glands, and taken from a man æt. 40. who had been admitted into St. Thomas's Hospital, in July, 1856, with extensive disease of the cervical glands on either side; the disease had commenced about five months previously as a small oval tumour opposite the left cornu of the os hyoides. It took a rapid growth, impeded respiration and deglutition, and caused sloughing of the skin and ulceration, from which recurrent hæmorrhages took place, and death ensued in the month of November, from anæmia rather than asphyxia.

The disease was found to have extended to and involved the tonsils, which were much enlarged, being about two inches in length to three quarters of an inch thick; they were composed of soft brain-like matter. The axillary glands and the mesenteric were soft and brain-like. There were some fibroid growths in each kidney, and considered to be cancerous masses of the scirrhous variety. The viscera were healthy, but no mention made of the spleen.

Case IV.—*Dr. Moxon's case of cancer of the left tonsil, the lymphatic glands, and the spleen* ('Trans. Path. Soc.:' Lond., 1870, vol. xx, p. 369). The specimens were removed from the body of a man, æt. 61, who was in Guy's Hospital for tumours in the neck. These were enlarged glands, and of eight or nine months' duration. An operation was undertaken for their removal, as the man was well nourished and had good health and no sign of disease of other organs was present. The operation, however, revealed the nature of the case. The lymphatic glands could not be separated from the great vessels, and those that were removed were found to be in a state of encephaloid disease. Œdema of the glottis came on shortly after the operation, and the man died within twenty-four hours from its performance. On inspecting his body, which was that of a large and fat man, it was found that very extensive disease existed. The cervical glands, some of which had been removed, were of large size, generally about six or eight times their natural dimension. The mediastinal and bronchial glands were large, especially the former; and here a most important condition was observed, for the disease of the glands had infected the neighbouring parts, so that both the tissue of the mediastinum and the neighbouring part of the lung were included in the disease: the lung was affected to the depth of an inch. This was the only instance of infection by contiguity that was present in the body. The visceral glands and the lumbar and iliac glands were in the same state of enlargement, but the axillary and inguinal glands were free. The spleen showed a very remarkable change; it was large, weighing 24 oz.; its surface showed small whitish, granular growths in the capsule. A section of it revealed the same growths, extending throughout it in enormous numbers, these growths varying in size from a scarcely visible minuteness to the size of a vetch seed. They were angular in form and continuous with the tissue around, not appearing to exercise pressure upon it. The appearance of the section exactly resembled that figured by Virchow, under the name of lympho-sarcoma of the spleen. The *left tonsil* was swollen to five or six times its natural size, and one section showed the same soft brain-like substance, which composed the enlarged glands and the formation in the spleen.

Some of the compound follicular glands at the root of the tongue were in a like state.

Mr. Durham also examined and prepared sections of the tonsils and glands of the tongue, and it was found that in microscopic character the growth in these parts strictly correspond with that in the spleen and glands.

On microscopic examination the new tissue was found to consist of two elements, viz., first, cells rather larger than lymph-cells, and having their interior nearly filled by a great nucleus, within which were many nucleoli; and second, a network of fine fibres, the meshes of which were of pretty uniform size, and thus such as would contain one to two dozen of the cells. The cells were in such enormous profusion that the network could not be seen until the section was shaken in water. A good shaking in water washed out most of the cells from the meshwork, but a few remained entangled, and then it was seen that a small number of stellate corpuscles, with smaller nuclei, were present in the network. The cell-wall of the cells was exceedingly perishable on addition of water, so that in a water specimen the cells soon looked like large free nuclei. This simple structure was common to the growth in the spleen, the glands, and the tonsils. There was no excess of white corpuscles in the blood; the lungs, liver, kidneys, and other organs were quite healthy.

Dr. Moxon gives some very interesting comments upon this case, which I should have liked to have detailed in full, but space will only permit me to take one or two short extracts.

He says, "We have here a disease of allied organs. The tonsils, lymph-glands and spleen only are affected, and these, as modern physiology teaches, are organs that resemble each other in essential character. The disease of these organs is the same in all, consisting of a new formation, which resembles the lymphatic gland tissue, but that the elements are larger."

"The tissues diseased are not identical, though they are functionally and structurally allied."

"Taking the facts as they stand on their own merits, and seeing the tumours soft and brain-like, so as to deserve the name encephaloid, and that some of them spread by contiguity to the tissues around, so proving themselves malignant, I have called them encephaloid cancer. The word cancer I use in a strictly clinical signification, as meaning an infecting growth, and not in any pathological sense; indeed, I believe the word cancer is not pathological, and should cease out of existence in pathological discourse. But although it is clinically encephaloid cancer of the glands, yet the relation of the case in point of strict pathological classification is highly interesting."

III.—On Hereditary Transmission of Structural Peculiarities. By John A. Ogle, M.D., F.R.C.P., Physician to and Lecturer at St. George's Hospital.

The general subject of hereditariness of disease and malformation has received much attention of late, and has especially been handled in a series of papers in this Review by Mr. Sedgwick, who has particularly dwelt upon the hereditary transmission of disease, deformity and peculiarity as limited by sex and by the law of Atavism.[1]

The case which I shall immediately describe does not illustrate either of these limitations, but is sufficiently interesting to be recorded as a good example of transmission of a deformity by no means commonly met with; that is, of hereditary deficiency of the distal elements of the fingers and toes.

Those who have studied Mr. Sedgwick's paper will remember with what fulness he exemplified his subjects, quoting cases of affections and peculiarities of the skin, the eyes,[2] ears, teeth, hair, the organs of circulation and respiration, as also of the limbs and bones; but illustrations from the latter were less numerous and important than those from other organs, especially the outer integument.

On referring to illustrations of hereditary transmission of deformities and malformations, and especially to deficiency in parts of the fingers and toes, I find that Mr. Sedgwick quotes[3] a remarkable case of hereditary absence of two distal phalanges said to have existed for ten generations; also a second case, in which for three generations all the toes and fingers had but two phalanges each; and a third case, in which for three generations the hand had only three fingers, and the foot but four toes.

Darwin again (among Englishmen), in his work 'On Animals and Plants under Domestication,' who illustrates not only the transmission of mental habits, of gait, gesture, but also of predisposition to insanity and disease of various kinds,[4] alludes also (vol. ii, p. 73)

[1] See April and October, 1861 and January and July, 1863. Inquiries on these subjects are shown to be most important in connection with marriage and with the insurance of life.

[2] Dr. Armitage, who has paid great attention to the condition of the blind, informs me that the Indigent Blind Visiting Society are collecting statistics among the blind as to hereditary transmission of blindness. They only commenced about two years and a half ago, and have not yet tabulated the results. Congenital and puerile cataracts, atrophy of retina, and glaucoma, seem to be the common hereditary causes of loss of sight. The Blind Institution of New York has collected statistics, but has only published a table showing the connection of blindness with consanguinity. Mr. Wait is the superintendent.

[3] Mr. Sedgwick cites many interesting cases from Petit, St. Hilaire, Lucas, Morel, Webster, &c., in illustration of the various subjects which he treats.

[4] Including especially inherited affections of the eye, as ptôsis, myopia, hypermetropia, cataract, amaurosis, malformations, Daltonism, squinting.

to deficiency in the phalanges having been inherited by females alone for ten generations, evidently the case above quoted by Mr. Sedgwick.

Prosper Lucas also, in his treatise 'De l'Hérédite Naturelle,'[1] gives instances of inherited malformation of the hands, fingers, &c.

Allied to the above is the following remarkable case, which some time ago I met with at St. George's Hospital, of deficiency of the phalanges of the fingers and toes. The patient was a young woman æt. 28, who was under my colleague Dr. Fuller's care for hysteria of six years' standing, and who was constantly emitting a peculiar sound like a "grunt," which came on and interrupted speech, or occurred equally at other times, and was attended by a jerk of the head and neck. In this person the following congenital defects existed:—The first and second fingers of both hands had only two phalanges, the final one of the second finger of the left hand being scarcely detectible, and being shorter than its fellow on the right hand, which was also very small. The third and fourth fingers of both hands had only one phalanx, and none of the fingers were provided with nails. At

Subjoined are some interesting cases of congenital and hereditary affections of the eye which have been lately recorded, and which may be considered along with those brought together by Sedgwick and Darwin, &c. Thus, an interesting case of malformation and cataractous state of the lenses of the eye occurring in four members of the same family, viz. one brother and three sisters, out of twelve children (two boys and ten girls), is related by Mr. Hulme, at p. 618 of the 'Lancet' for December 6th, 1862. Mr. Hulme, at the time of description, considered the state to have been congenital, and looked forward to the time when the cataractous state would be complete. He has lately informed me that the two girls are now married, and that he has operated on the boy as to both eyes. Mr. Hulme alludes to a somewhat similar case, published by Mr. Dixon, in the first vol. of the 'Ophthalmic Hospital Reports,' p. 54. In this case I find the mother and three sons were all affected by abnormal position of the crystalline lens, the eyes of the father and three younger children being healthy. In the same vol. of the 'Ophthalmic Hospital Reports,' p. 104, I find a description of six cases of cataract in one family, viz. a mother and five children, by Mr. Streatfeild, who observes, however, that it did not appear that any of the paternal or maternal relations had been similarly affected. The grandmother had observed the deficiency of vision and defect of eyes of the mother when she was a year and a half old, and noticed the same in the grandchildren at various ages LATER IN LIFE, ACCORDING TO THE SENIORITY OF THE FIVE CHILDREN. Also in the same volume, p. 260, is given by Mr. Streatfeild a description of seven cases of convergent strabismus in one family, viz. six children of one mother, and two cousins, all being boys but one. Again, at p. 153, are described by the same author two instances of rare and hereditary coloboma of the iris, in one case seven members, chiefly males, of the mother's family, being affected; in the other case a brother and sister, two cousins, and a grandfather. At p. 366 of vol. v of the 'Reports' is quoted a case of congenital irideremia in a woman, of whose two children one was in the same state. Mr. Hussey, of Oxford, has lately mentioned to me a case in which a man, æt. 40, had soft cataract, whose mother had had cataract, and whose daughter, at about fifteen, had soft cataract in both eyes. In Mr. Chance's work on 'Bodily Deformities,' part i, 1862, p. 71, is related a case, recorded by Dr. Staveley King, of a woman with the sclerotic perfectly blue, whose mother and grandfather had the same kind of eyes. Professor Humphry, of Cambridge, has informed me of an entire family affected by ptosis, whose father had the same peculiarity.

[1] Tomes i and ii, 1847.

the tip of the first finger on both hands existed, however, a small depression, and at the tip of the third one was a slight amount of thickening of the integument where the nail ought to be. The toes of both feet were exactly in the same condition, *i. e.* the second and third toe had only two phalanges, the fourth and fifth only one phalanx; all the toes, with the exception of two (unlike the fingers), had nails. The thumbs were all right, also the great toes, and were free from any defect.

When further examining the fingers, I found that the young woman was apparently at very little disadvantage by reason of this peculiarity, as she could sew, work crochet, and pick up anything readily with the fingers; she could also play dexterously on the pianoforte. The integument of the tips of the fingers evidently enjoyed a high degree of common sensibility, although it was of unusual thickness and hardness, and possessed its full share of tactile discrimination, as indicated by the aphemetric compass.[1] No other malformation or defect of the body existed.

On examining into the family history, I ascertained that this kind of malformation was not restricted to the young woman alone. Her father had had ten children; and two sons and one daughter, in addition to herself, have the defect. In the case of the sister, all the fingers of both hands were defective, each wanting one phalanx. In that of one brother, who was alive and married, and who had the same defects of the fingers, the defect existed in his two sons also. In that of another brother, also married, and with children, now living in Australia, the same defect exists in some of his sons. The third brother who had the defect never married.

In the case of an aunt (the father's sister) there was the same defect, as also in four of her sons, a fifth son being exempt. An uncle (a brother of the father) who was himself perfect, had married for his second wife a kind of cousin of the name of C—, and out of their six children four had fingers and toes in a similar defective condition. The children by the first wife were perfect. Among these various relatives it was also said that all those who have their toes so deformed have their two middle toes adherent, presenting a webbed condition; in some cases also the fingers are in like manner webbed. It appears also that in some instances the webbed and united toes have been separated, but in the case of a brother the union of the bones was pronounced by a surgeon to be so complete that separation could not be attempted. There are generally two toes webbed together on one or both feet, and in some cases it is the same with the fingers on one of the hands. It is so in one hand of a brother and also of his little girl. In all cases the thumbs are perfect, having nails, but in some individuals they are remarkably broad and flat, and will not bend at the joint; in others the thumbs

[1] An interval of one tenth of an inch between the points being discriminated.

are "partially turned round," to use the woman's expression. It was stated that there was a great and often striking resemblance in features, which are generally considered to be of a Jewish type, between all who have the defect.

Enquiring still further as to the family history, I found that the young woman understood that the above-mentioned defect was known to have existed for two or three generations, and to have existed upon and to have been inherited from the father's mother's side, whose name was C—. Their grandmother had the defect in hands and feet, and herself had nine children, of whom two sons and a daughter had the malformation. (These have been alluded to.) The mother remembers that, when she was twenty-one years of age, she knew a lady, æt. 70, in Lincolnshire, who had the defect in her fingers. Her maiden name had been C—. She married, but had no children. She had also been given to understand that many individuals in and about Manchester, but of different surnames, had a similar peculiarity, and that or near Lincoln, at the present time, resided a family of the name of C—, the members of which were said to have it also, and who were therefore looked upon by her friends in London as being relatives. One was a captain in the army, who is now dead; he had the defect, but never married. On learning this I communicated with Dr. Palmer, resident physician at the Lincoln Asylum, and asked him to be so good as to make some inquiries for me on the matter. He replied that he had found that a family of the name of C—, living in Lincoln and its neighbourhood, have deformity of the fingers; that adjutant C—, of the militia (now dead), had "short fingers;" that his brother, parish clerk of one of the churches (also dead), had short fingers; that two daughters of the latter, and also several of their cousins (all living), have short fingers; and that a second or third cousin of these women, who has a similar deformity, had been recently confined, and that the child has some of its fingers webbed. Dr. Palmer understood that there were several other instances, occurring in the same family, of the same kind of defect.

The young woman whose case I have related also informed me that a family was known to be living in Manchester whose fingers were deformed, and who were supposed in consequence to be distant relations. Mr. Windsor, surgeon of that city, has been able to trace for me, at any rate, one family with the defect. He visited one of its members, a cutler, æt. 72, who "appeared to want the last two phalanges of most of the fingers (so-called spontaneous amputation), and learnt from him, though not inclined to be very communicative, "that there were at least nine relations of his in whom the hands were defective."

The above case is highly interesting in reference to the subject of hereditary deficiencies in or suppression of the phalanges, and may be considered in connection with the instructive communication, to which I will specially draw attention, by Dr. Struthers, professor of anatomy at Aberdeen, published in the 'Edin. New Philosophical Journal' for July, 1863, on variation in the number of fingers and toes and in the number of phalanges in man. In that paper, after reference to cases of increase and diminution in the number of entire digits, with and without hereditary origin (distant or otherwise), the author adduces cases of variation in number, both increase and diminution of phalanges, noticing one which is remarkable in relation to the case which I have quoted, described by Dr. O. H. Bell, of St. Andrew's, in which all the fingers and toes wanted a phalanx in several members of a family; also a case in which four fingers of one hand possessed but one phalanx each, the thumb having two phalanges; also a case in which the fingers were so formed as to give the hand a resemblance to a foot, and also a case of five rudimentary digits on the hand. None of the cases were, however, hereditary. In the instance of the person in whom a phalanx on each finger and toe was wanting (the same defect which exists in the case I have just related at large) a brother and a sister were similarly affected. It did not appear that any other collateral relative or ancestor had the defect in question. The patient could "easily seize and retain minute articles, as a needle or pin, between the thumb and index finger, and could write with comparative ease." Dr. Struthers, contrasting hereditary cases of variation affecting the hand alone with those of the foot, observes that a far higher proportion exists in which the hand is affected than the foot; this fact coincides with the zoological one, that in those mammalia in which digits differ in number on two feet, the greater number is on the fore foot. He notices a curious fact in one case in which an increase of digits existed (this variety being transmitted through at least four generations), and that is, that the variety, so far from being weakened, had gathered force in each new generation, "although it had not the advantage of the greater development attending utility to enable it to increase its hold on the organism." The defect made its way to an additional limb in each successive generation.

Dr. Struthers also noticed that the defect or variation as to digits extended in some cases in depth as well as symmetrically and serially, beginning at one phalanx and extending at length to the metacarpal or metatarsal region. He alludes to two cases, of which he had heard, in which the thumb alone was wanting, in one of the cases on both hands. As before said, Dr. Struthers only adduces one new case resembling the one I have recorded, in wanting a phalanx of each finger and toe, but he quotes the case of hereditary transmission of deficient phalanx by A. Robert, mentioned by Mr. Sedgwick, to which I have already alluded above, and also the case related by Dr. Kellie, of Leith, in the 'Edin. Medical and Surgical Journal' for 1868, alluded to by Mr. Sedgwick, as before described.

Professor Struthers refers to and quotes several of the more interesting cases of hereditary transmission of increase in the number of digits placed on record.[1] This variation in the number of digits or phalanges is much more common than the diminution, and is pictured by Struthers in the paper alluded to, and by Annandale in his book on the 'Malformation, Diseases, and Injuries of Fingers and Toes.'[2]

Lately a case was described in the 'Brit. Med. Journ.,' for April 22, 1871, p. 433, in which two supplemental fingers were removed by Dr. Churchill, jun., of Dublin.

Hereditary transmission of this polydactylism, as well as of webbing of the fingers and toes, is alluded to by Sedgwick, and several cases are cited by him. For some others which he does not quote, or which have been published in England since his papers, see the following places, viz. 'Med. Times and Gazette,' 1861, March 23, p. 312; also 1862, Aug. 23, p. 212; also 1863, June 20, p. 656; also 1863, Nov. 14, p. 520; also 1865, Dec. 2, p. 606.

Darwin also alludes to it in his work before cited, vol. ii, pp. 12 and 13. He remarks, that the number of five digits is not exceeded by any mammal, bird or existing reptile.[3]

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[1] He quotes the Old Testament case of the son of the giant in Gath, who had six fingers on each hand, and six toes on each foot; and also the two cases mentioned by Pliny (Book xi, ch. 43), among the Romans, with six fingers on each hand; and also the case of Anne Boleyn, with six fingers on each hand.

Such superfluities are not always possessed with impunity, as I find that Dr. M. Clarke observes('Trans. of Ethnolog. Soc.,' new series) that among the Africans of Sierra Leone a child, in one instance, born with supernumerary fingers was burnt alive, soon after birth, on that account, and in another instance the children were destroyed by twisting the neck, and then buried in a dung-heap. Burton, in his "Notes on Waitz's Anthropology" (Vol. i of 'Memoirs of the Anthropological Soc.,' 1863-4, p. 235), mentions that the Shaykhs of the Great Fazli Tribe have invariably six fingers on their hands. He also observes that in Persia, if a Sayzid child, a descendant of the Prophet, be born without the upper eyelids being pink, it is not believed to be legitimate.

[2] No doubt, the commonest instance of hereditary transmission of variety, as regards the fingers, is in the crookedness of the little fingers. Dr. Dobell describes, in the 'Trans. of the Med.-Chir. Soc.,' vol. xlvi, p. 25, a case in which, along with thickening of all the joints of the fingers, crookedness of the little and ring fingers was transmitted hereditarily for five generations. Mr. Annandale, in his 'Edinburgh Lectures on the Malformations, &c., of the Fingers and Toes,' refers, among other points, to congenital deficiencies, both of entire digits and of their segments, the phalanges. He observes that diminution in number of the digits is usually associated with deficiency in number and development of the phalanges, and alludes to the description of cases by Simpson and Montgomery, in which portions of the digits have been spontaneously amputated *in utero*. He relates several cases of such deficiencies, which are represented in a plate. In none of these do I find that any hereditary history of the malformations existed.

[3] Darwin, op. cit., ii, p. 13, alludes to the *re*-growth of supernumerary fingers after amputation. At p. 16 he speaks of the latent tendency to the formation of an additional digit as existing in all mammals, including man.

I would here refer to a paper lately published by Dr. Wenzel Gruber, of St. Petersburg, which contains the most complete analysis existing of all the cases of polydactylism, including two additional cases (with illustrations) of his own. This communication[1] gives nearly a hundred references to cases of which a great many were instances of hereditary transmission of the malformation.[2]

Polydactylism is exhibited in that well known variety of our English fowl called the Dorking (which has five toes), also in a variety of the dog described as having a supernumerary toe on the hind foot, by Cuvier ('Disc. Prelim. Ossem. Fossiles,' ed. iv, tom. 1, p. 205), and quoted by Palmer in his edition of Hunter's works, vol. iv, p. 329.

Casting about for other recent instances of hereditary transmission of deficiencies in the fingers or phalanges, I obtained the following highly interesting one from my friend Professor Rolleston. It is that of a woman living at Oxford, who has the following variation of fingers on both hands. The forefingers are the shortest, the second fingers are the next shortest, the ring fingers are the longest of all, and the little fingers the next longest. This peculiarity of structure already extends to four generations, being found in the woman herself, her father, her father's mother, and her own little boy.

As regards observations on hereditary malformations of fingers, I may mention a case of hereditary deformity (webbed fingers),[3] in four successive generations, published by Mr. H. Barker, of Ulverstone, in the 'Medical Mirror' for December, 1864, p. 758. In this singular case, a woman, J. K—, both thumbs are deformed; as respects one thumb, there are two distinct bones in the metacarpus, and the first phalanx is all right, but there are three distinct phalanges in the ungual end of the thumb; the other thumb presents only one

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[1] See 'Bulletin de l'Académie Impériale des Sciences,' vol. xv, 1870, pp. 460—483. Zusammenstellung veröffentlicher Fälle von Polydactylie, mit 6 Fingern an der Hand und 6 Zehen an dem Fusse; und Beschreibung zweier neuen Fälle von Duplicität des Daumens.

[2] The subject, not only of supernumerary fingers, but of supernumerary hands, is exemplified by a case, recorded in vol. xlvi of the 'Transactions of the Med.-Chir. Soc.,' 1863, p. 29, of a woman with three hands, illustrated by analogous malformations in the lower animals, and communicated by Mr. Jardine Murray, of Brighton. The case does not seem to have been hereditary. Mr. Murray exhibited drawings of preparations of double feet in the pig, from the museum, and of a similar malformation in birds, lent by Mr. Cobbold. In passing I would here refer to a communication by Dr. Struthers, in vol. iii of the 'Journal of Anatomy and Physiology,' of a case of additional bone in the human carpus, which occurred in both wrists at the same place; he alludes to the few cases in which an additional bone has been found in the human carpus.

[3] Mr. Murray, in the description of the case of the woman just mentioned with three hands, speaks of the webbing of fingers as due to absence of fission, or properly to an arrest of development; for in the embryo the rudiments of hands and feet do not at first present any division of the fingers and toes.

metacarpal bone, but has two phalanges at its ungual end; also a separate supplemental metacarpus. This person was the only one of five children who had the defect. It appears that her grandmother, during pregnancy, was bitten and seriously injured by a dog, and had her hands much lacerated. Her child (the father of J. K—) presented at birth the same appearances observed in J. K—. J. K— has had five children, viz. one son and four daughters; the son and youngest daughter had the deformity, the three intermediate daughters being free from the deformity. Her son married, and had six children, and one only out of the six escaped malformation. The youngest daughter also married, and had two boys, and of those one had the deformity. To this case I allude later on (see p. 517).

Mr. Brodhurst, who informs me that hereditary contraction of the toes, and especially the second toe, is often hereditary, has only once met with it in the fingers. For club-foot, which is often hereditary, he has operated on three generations of one family.[1]

I find an interesting case of hereditary webbing of the fingers, related by Dr. Harker, of Lancaster, in the 'Lancet' for 1865, September 30th. The individual was an active and intelligent boy, all of whose fingers were webbed and united together completely to their tips, the little fingers having an extra nail.

The thumb of the right hand possessed three sets of phalanges, that of the left hand had two sets webbed together, the right thumb being furnished with three nails, the left one with two. The little toe of the right foot was webbed to the next toe. It appeared that the child's *father* and *grandfather* had similar hands. Some (not all) of its brothers and sisters have similar hands.

In Mr. Chance's work 'On the Nature, Causes, Varieties, and Treatment of Bodily Deformities,'[2] a case is cited of Dr. Pollock's, of Hatton Garden, in which five out of eight children (brothers and sisters) have an absence of a movable articulation between the second and the end phalanges of the forefinger, which thus has two instead of three joints. In two of the children this exists in both hands, and in the three others in one hand. The deformity has existed in the family for four generations, and it is understood that it also existed in a fifth generation.

In considering the above cases of hereditary transmission of DEFECTS in man, one is naturally tempted to inquire whether they can in any degree be referred to "reversion" or retrogression to any ancestral type. My friend Mr. Flower thinks this can have

[1] Mr. Brabazan, describing a case of malformation in the 'Dublin Hospital Gazette' for 1854 (vol. i, p. 375), observes that congenital malformation of the upper limb, as contrasted with the lower limb, are rare. He remarks that congenital malformation of the hand follows the law of other congenital malformations, representing the natural condition of the limb in lower animals, the persistence of the interdigital membrane producing an accurate resemblance to the limb of the Cheiroptera.

[2] Part i, 1862, p. 79. Part ii was, I believe, never published.

nothing to do with defect either in numbers of digits or of phalanges, for the lowest known types of mammals have the same number as in man. He observes:—"The most imaginative evolutionist has never attempted to trace his descent through any of the paucidigitate types, unless it be some in which the pollex or hallux alone are rudimentary or suppressed." Mr. Flower had not met with any cases of hereditary transmission of defective phalanges. Neither had Mr. W. K. Parker, the well-known author of most valuable researches in natural history and comparative anatomy, who observed to me that he did not know of any of the quadrumana that have other than certain toes and fingers abortively developed. He remarks, "In the Potto (Peredicticus) the thumb has the usual phalanges,[1] and the index is a single-jointed stump. The third finger is short, but has possibly the usual number of phalanges." Mr. Parker observes that many odd things of this sort occur in that order, but no regular absence of all the terminal digits. He further remarks that "the number of phalanges has become restricted in man, and if we went down amongst oviparous prototypes we should find *more* and not fewer joints. As a rule, the modification of the human hand is by excess, and that is very doubtfully to be referred to reversion."[2]

I would here allude to the elaborate and interesting researches of Professor Humphry, of Cambridge, "On the Myology of the Limbs of the Man, the Aï, the Two-toed Anteater, and the Pangolin," published in the fourth volume of the 'Journal of Anatomy and Physiology.' In this communication consideration is given to the carpus, tarsus, and digits of these animals, and also observations on the order and manner of suppression of digits.

On the whole, the above recorded cases of hereditary defect of fingers or parts of fingers must be looked upon as instances of arrest of development (*i. e.* of abortion), "anomalous arrest of an early character," the persistence of an embryonic condition, to use Mr. Darwin's phrase, or the result of some perverted formative power in the sperm-cell itself, the tendency to which has been transmitted from ancestors, just as other minute peculiarities or qualities, psychical or physical, may be handed down. We are well aware of

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[1] See Mivart's 'Genesis of Species,' p. 105.

[2] Mr, Harker, of Lancaster, who informs me that he has had under his notice some interesting cases of accessory toes in children, observes that he has noticed "that between the fore and middle rays of the palmar or pedal membranes there is occasionally a certain amount of union. These rays acting in unison, as they do by their anatomical relations, seem, on that account, to be occasionally united at the base of the first phalanges, and this not merely by webbing, but by the whole fleshy structure; this is especially the case in the index and middle toe." Again, he remarks, "that palmar and pedal degeneration shows itself as frequently in the tendency to increase in the number of the rays as in the retrogressive tendency in the way of deficiency of phalanges of the less important rays. The subject of their malformation is, no doubt, one of very great importance, not merely as to degeneration, but also as to human type and character."

numberless such instances among domestic animals, and it is well known that qualities which have only accidentally arisen may be transmitted. Thus we get what is called "breeding" or variety of "race," and even new races. Take, for example, the race of "Otter sheep," which, with peculiarly long bodies and short legs, have originated in Massachusetts.[1] Again, we have a race of Hungarian cattle with *un*cloven hoofs.[2]

It is not unlikely that *mutilation* was originally the source of many inherited defects. In support of this supposition I will quote instances of the hereditary transmission of the results of mutilation, as well among animals as man.

Thus Waitz[3] quotes the observations of Williamson, who saw dogs in Carolina which have been deficient in tails for three or four generations, in consequence of one of their ancestors having accidentally lost it. A cow, three years ago, which had lost by suppuration her left horn, produced three calves which, instead of the left horn, presented only a small protuberance on the skin. Darwin (op. cit., p. 12) alludes to this case and to hereditary absence of a horn in the stag. Dogs and horses whose tails or ears are clipped, as the draught dogs in Kamtschatka, often transmit these deficiencies, according to Blumenbach, to their offspring. A bull without horns, in Paraguay,[4] produced only calves without horns, and a buck goat with cartilaginous prominent nasal organs, transmitted its peculiarities to its offspring.

Darwin, quoting Sedgwick, speaks (op. cit., vol. ii, p. 23) of a soldier who lost an eye by ophthalmia, and whose two sons were microcephalic on the same side; and Sedgwick alludes (op. cit., p. 484) to small-eyed boys, the sons of a father who lost an eye by ophthalmia. Darwin (op. cit., p. 23) alludes to cases recorded of cats, dogs, and horses which have had their tails, legs, &c., injured or amputated, and whose offspring had the same parts deformed, and alludes to a long list of inherited injuries which Prosper Lucas mentions, and which Mr. Sedgwick has availed himself of. He refers to the inheritance of exostoses on the legs (spavin, ringbones, splints) of horses, and quotes a case from Blumenbach, in which the sons of a man with fingers amputated had the same fingers crooked;[5] also to the now celebrated guinea-pigs of Dr. Brown-Séquard, which, when rendered epileptic by injuries, had offspring suffering from the same affection.[6] My friend Dr. Brown-Séquard informs me that division

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1 See 'Waitz's Introduction to Anthropology,' published by the Anthropological Society, 1863, p. 82.

2 At the end of this communication I subjoin some observations by Dr. Shorthouse on the difference betwixt "breeds" and "races."

3 Loc. cit., p. 83.

4 The hornless oxen of Paraguay mentioned by Azara are quoted by Mr. Sedgwick.

5 To this also Mr. Sedgwick alludes.

6 Corroborated by Dr. Westphal. See 'Berliner Klinischen Wochenschrift', 1871, No. 38.

of the sciatic nerve causes epilepsy, and oftentimes guinea pigs will eat their own toes which are deprived of sensation. Their young ones will become epileptic and have defective toes, and also their grandchildren. At page 12 of vol. i, op. cit., Darwin mentions the case of a bitch with a leg deformed, having several puppies with similar defect. At page 173 of the 'Brit. Med. Journ.' for Feb. 10, 1872, under the title of "Transmission of Induced Changes," is the record of the shepherd's dog always having his tail cut off to render him exempt from taxes under the old excise laws, and it is stated that, in process of time, many mothers produced litters or parts of a litter wholly without tails.

Waitz mentions the fact,[1] that among the Chaonia-Berbers in the Auras mountains, the absence of the lobule of the ear, which also occurs among the Cajots, in Spain, has doubtless become general from accidentally arisen peculiarity. He also cites from authors the following cases, viz. the children of an officer whose little finger had been cut across and became crooked, possessing an analogous defect; and the case of an officer, wounded in battle, transmitting to his children a scar on the forehead.

It might have been expected that the results of circumcision, as among the Jews, might have become hereditary. I am, however, informed that this is not known to occur.[2]

It has been thought by some high authorities that, among people who give an artificial form to the skull, succeeding generations exhibit a similar shape.[3]

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[1] Loc. cit., p. 86, quoted from Guyen, in 'L'Institut,' and 'Nouv. Ann. des Voyages.'

[2] I may mention the fact of phymosis very frequently existing in several members of one family. Thus, I am personally acquainted with a family in which operations for its relief had been performed on one brother, and on *his* son; on the son of a second brother; on the son of a third brother; on the two sons of a sister; and on one son of another sister. Mr. Prescott Hewett has lately told me of one family in which he frequently had to operate for this malformation (phymosis), which existed in the father and several of his sons and their children, as also in several sons of some of the married sister's; but the defect was worse in the sons of the brothers than of the daughters. Mr. Hewett had also to operate for sebaceous tumours of the scalp, which had existed in three generations, but which always affected the females, and *never* the males.

[3] As regards the transmission of family peculiarities of various kinds, Waitz observes as follows (op. cit., p. 84):—"The most frequently quoted instances of this kind are the thick lips of the house of Hapsburgh since its alliance with the Jagellones; the tall life-guards of Frederick I of Prussia, who produced a large-sized progeny. Colour and quality of the skin are also transmitted, and so are temperament, acuteness, idiocy, or deficiencies in the organs of sense. Instances of hereditary blindness and deafness, and of alternating dumbness, so that every second or third child was deaf, are given by Lucas. Harris (see 'Highlands of Ethiopia,' second edition, vol. i, p. 286) communicates a case of hereditary blindness in one eye and of a double thumb on the right hand. The so-called Porcupine-men of the family Lambert, with their excrescences on the hands and feet, have often been quoted. Thomson (see 'Ed. Med. Journ.,' 1858, p. 501) has endeavoured to prove that the peculiarities of the skin are transmitted in the male line, which he supports by many illustrative cases. In Burmah, remarkably hirsute men have been met with,

Very recently we have had described an interesting case of transmission of acquired structural defect as occurring in the human subject. I allude to a case communicated by Mr. Addenbroke to the 'Brit. Med. Journ.,' of congenital facial paralysis (on the left side) in two children (cousins), whose mothers, during pregnancy, had nursed their mother in an attack of hemiplegia, which came on suddenly whilst they were lifting her, and which was attended by facial paralysis, which specially attracted their attention.

Sedgwick quotes the case, observed by Stahl, and quoted by Steinan in his essay on hereditary disease, of a soldier who lost in war one of his eyes; on his return home he married, and his wife bore him a son, one of whose eyes was quite dried up, so that he was monocular like his father.

Although Darwin quotes cases of inherited transmission of results of mutilation, &c., it is right to say that he speaks (p. 12) of the doubtful subject of inherited mutilation;[1] and at page 135 of his work on the 'Origin of Species,' when speaking of deficiency or absence of anterior tarsi or feet of certain beetles, observes, "there is not sufficient evidence to induce us to believe that mutilations are ever inherited."

Is it unlikely that the races of one-eared and earless rabbits (the latter are mentioned by Darwin[2]), tailless cats and dogs and monkeys, which are familiar to us, have had their beginnings in mutilations?

Dr. Shorthouse,[3] who for twenty-five years has paid special attention to the physiology of breeding, or "the laws which regulate the function of reproduction," and who has records of many thousands of experiments on the subjects, informs me that though he has known many hundred cases in which physical defects

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to whom the peculiarity was transmitted through three generations (see 'Ausland,' 1858, p. 461). There are also instances recorded of six-fingered, six-toed, and of web-footed individuals, who transmitted these peculiarities to their offspring. The frequent tendency of succeeding generations to reproduce deviations presents itself also in the hereditary transmissions of a number of diseases, such as goitre, cretinism, and mental affections." As a curious and, I would add, questionable instance of the transmission of acquired psychical qualities, Waitz states (see op. cit., p. 83) that dogs who return to a wild state no longer bark, and that "cats are said not to mew in America." Hunter (see vol. iv, p. 329, of Palmer's edition of his works) observes that "the dogs in the South Sea Islands learn 'to bark' by imitation." Pouchet, in his work on the 'Plurality of the Human Race,' 1864, p. 34, alludes to the fact of dogs not barking in certain countries, or on returning to the savage state, and quotes from Roulin observations as to the wild cats losing troublesome mewings.

[1] Sir T. Watson, in the last edition of his 'Lectures on the Practice of Medicine,' makes allusion to Darwin's observations on the transmission of acquired deformities.

[2] Op. cit., vol. i, pp. 12 and 108.

[3] Author of an essay on the 'Physiology of Breeding;' and of 'Pedigree Tables of many of the most remarkable Thoroughbred Horses of the present and past days.'

and deformity have been transmitted, he has not known a single instance in the lower animals of mutilation being transmitted. This he accounts for by the fact that when any animal is born minus a limb or other important member, it is generally destroyed or sold to the showman, and is never used for breeding.

He says he has seen hundreds of cases of malformations of every kind, and thousands of such cases as club-foot being transmitted. "When the malformation is of a serious nature, the animal is not used for breeding purposes; when it appears in a more modified form, so that the animal may be useful for such purpose, and the animal is bred from the malformation, defect is almost certain to be transmitted. The late Marquis of Westminster had a famous mare called Glenzner, which had very defective fore feet, scarcely any hoof or horny crust at all, and was always lame unless she wore 'bar' shoes with high heels. Most of the foals inherited the same defect. One of her daughters, named Melance, the property of the late Earl of Derby, was famous as a racer in spite of her defective feet. She was put to the stud at the end of her third year, and all her foals have had the same defects in the feet. There is now at the stud a horse called Knight of Kars. He was not of much account himself on the turf, because he had a club foot; but when put to the stud he was patronised very extensively, because he was half-brother to Stockwell, who was considered the best stallion in the world, and his covering fee was 200 guineas. The Knight of Kars covered mares at 15 or 10 guineas, and therefore breeders patronised him because they argued that they should have the same blood as Stockwell's at a much lower figure. The horse begat from thirty to forty foals a year for many years, and nearly all these foals had a club-foot, and would not stand training for racing purposes. Consequently the Knight of Kars is now at a discount. The sire of this horse, Nutwith, and his grandsire, Tomboy, both had deformed feet. I could multiply such cases by hundreds.

"There are other characteristics, though hardly to be classed with deformities, which are peculiar to certain family lines, *e. g.* nine-tenths of the descendants of a famous horse called Birdcatcher have a tendency to curbs on the hocks (a kind of exostosis). The descendants of another famous horse called Melbourne are nearly all of them afflicted with roaring."

Dr. Shorthouse proceeds to furnish me with a few cases of hereditary mutilation and deformity in the HUMAN SUBJECT which have fallen within his own practice. They are as follows:

"A poor woman formerly living at Carshalton had mutilated hands —mutilated *in uterô;* the little finger and the fingers next it were entirely missing, and a sort of little nipple grew at the end of the metacarpal bone. That woman had a husband whose urethra opened underneath the penis and behind the glans. The couple had six children, four boys and two girls. Both the girls inherited the mother's mutilated hands, and all the boys had the peculiar state of urethra I have described.

"I have known several instances in which a parent has had an extra finger, or toe, or breast, and has transmitted that superabundant organ or appendage to one or two offspring; but I know a case in which a man who had six toes on each foot—inherited from his father—transmitted the same superabundance to ALL his legitimate children, and to a few illegitimate ones. Most of these children I brought into the world myself. I once attended a woman in childbirth with her first child, and I found that she had three mammary glands and three nipples. I ascertained that the mother had the same peculiarity. The child to which she gave birth was a boy, and he had only two rudimentary nipples; her next child, however, was a female, and that had three breasts like the mother. In this case sex would seem to have influenced the transmission of the peculiarity."[1]

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[1] In reference to cases of "supernumerary nipples," and also to cases of supernumerary fingers above alluded to, I would here quote some observations which I received some time ago (and which are interesting in connection with the question of reversion) from Mr. Sedgwick, and which arose out of a consideration of the use which Mr. Darwin made of such cases. He remarks as follows:—

"If you have not yet seen Darwin's 'Descent of Man,' it may interest you to know that he has, in this lately published work, considerably modified his earlier statements, especially on the subject of 'reversion;' and that the objections urged against his theory in the 'review' of his work on variation under domestication have been strengthened by later observations, and have been accepted by Darwin himself. In this review ('Medico-Chir. Rev.,' July, 1868, pp. 155-6) the subject of supernumerary digits was discussed, and objections were urged against Darwin's reasons for regarding such digits as evidence of our being descended from a fish. At pp. 125-6, vol. i, of his present work, all his former and elaborate reasoning on this subject is given up, and he now sees 'that it is extremely doubtful whether supernumerary digits can thus be accounted for.' The credit of having wrung such an admission from Darwin is due to Prof. Gegenbaur, whose paper on the subject has settled the question on that point.

"Another and, if possible, a still more fatal admission is referable to the occurrence of supernumerary mammæ. In the cases I had published they occurred in the inguinal region, or on the abdominal surface of the body; and Darwin was led to refer such peculiarities to reversion, because in the lower animals which have multiple mammæ, such as the cow, they are so placed. But in 1869 Prof. Preyer published evidence to show that such supernumerary mammæ in the human subject were not limited to the abdominal surface of the body, but had been observed also to occur *on the back;* and again the evidence from reversion has collapsed, and Darwin, with praiseworthy candour, now writes 'the force of my argument is greatly weakened or perhaps quite destroyed' (ibid., p. 125, note).

"You will readily see why in the review referred to so much importance was ascribed to 'reversion,' for without the support of this principle in hereditary transmission cartloads of writings to show that organized forms are liable to 'variation' become valueless. For reversion is, strictly speaking, the backbone of the argument, and without it the whole theory of the origin of species, and of man's descent through monkeys, lemurs, marsupials, &c., from some now lost sight of ascidian progenitor, becomes not merely limp, but altogether without form and void.

"So great, indeed, has been the necessity for modifying the theory of 'natural

In considering the probability of mutilations and injuries having been the original cause of many transmitted or hereditary defects, it may be allowable to entertain the question, whether they could have had their rise in malformations consequent upon fright of the mother during pregnancy. How far mental emotion of a mother can affect the fœtus *in uterô*[1] is of course a very old and very much debated question. But we have some good authority for supposing that such influence may exist and be operative. Darwin, at page 61 of vol. ii of his work, observes, that it is probable that hardly a change of any kind affects either parent without some mark being left on the germ.[2] I do not propose to introduce instances from well-known

selection' as the efficient cause of the origin of species, that the greater part of the present work has been occupied by the consideration of 'sexual selection,' in which the same ground, differently planted, is again gone over, with an equally unsatisfactory result. For in this theory of the origin of species there are two great barriers which block the way—reversion and hybridism. As regards hybridism, the union of distinct species leads to sterility; and as regards reversion, even from the domesticated to the wild state, the change is not true in its completeness, for it appears to be impossible (from various causes) for either animals or plants which have undergone much change to live over again, either as regards themselves or their descendants, in a backward course, the life that is passed. Hence it may be urged now as strongly as in the review of 1868 (p. 166), with respect to the 150 distinct breeds of pigeons descended from the wild rock pigeon, 'that there is, at present, no sufficient evidence to warrant the supposition that time will confirm their promotion, so as to entitle them hereafter to the rank of true species.' "

[1] Regarding amputation of limbs in the uterus, see observations of Sir J. Simpson, in the 'Dublin Medical Gazette,' vol. x, p. 220; also article "Fœtus," in the 'Cyclopædia of Anat. and Phys.,' vol ii, p. 324, where it is alluded to as a result of gangrenous inflammation.

The interesting observations of Harvey on transmitted peculiarities,&c., I have never seen quoted. In his 'Anatomical Exercises on the Generation of Animals' [see 'Sydenham Society's Translation,' p. 429] he observes, "And this, too, is a remarkable fact, that virtues and vices, marks and moles, and even particular dispositions to disease, are transmitted by parents to their offspring, and that while some inherit in this way, all do not. Among our poultry some are courageous and pugnaciously inclined, and will sooner die than yield or flee from an adversary; their descendants, once or twice removed, however, unless they have come of equally well-bred parents, gradually lose this quality, according to the adage, 'The brave are begotten by the brave.'" In various other species of animals, and particularly in the human family, a certain stability of race is observed; numerous qualities, in fact, both of mind and body are derived by hereditary descent." Again, in his tract 'On Conception,' p. 582, he says: "By this same law the son is born like his parents, and virtues which ennoble, and vices which degrade a race, are sometimes passed on to descendants through a long series of years. Even diseases propagate their kind, as lepra, gout, syphilis, and others. But why do I speak of diseases, when the moles, warts, and cicatrices of the progenitor are sometimes repeated in the descendants after many generations?" [Here he alludes to the description given by Aristotle in his 'Hist. Animal.,' lib. vii, cap. 6, and his 'De Gen. Animal.,' lib. i, cap. 17.]

"Every fourth birth," says Pliny [lib. vii, cap. 11], "the mark of the origin of the Dacian Family is repeated on the arm. Why may not thoughts, opinions, and manners now prevalent, many years hence return again, after an intermediate period of neglect?" Again, in his work on 'Generation' [op. cit., see p. 408], he quotes the saying of Seneca [from his 'Nat. Quæst.,' lib. iii, cap. 29], "In the semen is comprised the entire cause of the future man; and the unborn babe sources supporting this view, as the reader will be familiar with them. Mr. Sedgwick[1] quotes the case related by Mr. Wright (in the 'Lancet' of 1830-31, vol. ii, p. 464), of a woman frightened by a ferret when pregnant, and the child, when born, had eyes precisely like that animal; every child born afterwards had the same kind of eyes, and they all became blind or nearly so about the age of puberty. Also the case,[2] related by Dr. Fosbroke (see 'Lancet,' 1830, vol. i, p. 740), of a lady who fell out of a window, in a state of pregnancy, and instantly became deaf in one ear. The child produced by this pregnancy was born deaf in the corresponding ear. Later on, Mr. Sedgwick alludes to the acquirement of disease through the influence of maternal emotion, and refers to cases showing this influence which have been under his own notice.

I have lately heard of the following cases, bearing on this subject:

Dr. Thurnam, of the Wiltshire County Asylum, informs me of the case of a woman, whom he met in Borrodale, in Cumberland, who had the left hand imperfectly developed. The carpal bones were normal, but there was no trace of metacarpal bones or phalanges. The thumb alone was represented by a projection of the skin half an inch in length, and supporting a tiny nail. The stump of the hand had four rudimentary nails planted on the dorsal surface, but these nails were in no relation to bones. No other member of the family was known to have been similarly affected, but the mother asserted that previous to her birth, her mother, going to look at the body of her dead son in the dusk, fancied he rose up and tried to bite her hand. To this occurrence, during her pregnancy, the mother attributed the malformation of the hand of her child.

Mr. Gill, of Woburn Place, met with the following case:

A woman, æt. 40, was about six months' pregnant with her eleventh child, and was feeling quite well when she went to church one Sunday morning, and there saw a girl in front of her, whose left hand looked like a "hook." From her description it would appear as if the index finger only remained, and that the second, third, and little fingers were lost at their metacarpal articulation, owing to burn or scald. The lady was much frightened, and very nearly fainted. She felt no movement of the child (she had quickened two months). She was seen by her medical man the NEXT MORNING, to whom she related what had occurred, and stated to him her firm belief "that her baby would have something the matter with its *left* hand, remarking, at the same time, that 'you doctors don't believe this sort of thing; but I do, and we shall see.'" She

has written within it the law of a beard and a hoary head, for the whole body and head of future years are already traced in delicate and obscure outlines in its constitution."

[1] 'Brit. and For. Med.-Chir. Rev.,' April, 1861, p. 485.

[2] 'Brit. and For. Med.-Chir. Rev.,' July, 1861, p. 202.

went to the full time, and a boy was born. Directly it was born the mother said, "*Now* look at baby's left hand, and see who is right, you or I." We found the LEFT hand with the thumb and INDEX finger all right; the remaining fingers were "hooked" down, and could not be perfectly straightened, even with force, and unless pulled out were firmly fixed against the palm of the hand. Otherwise the child was quite right, though not a strong child.

Mr. Davis, of Harrow, relates the following in the 'Lancet:'[1]

A healthy woman, of twenty-five years of age, whilst pregnant with her second child, about the sixth week, was walking one day in the street, when she met a child without arms. She was "horrified at the sight" at the time, but forgot the circumstance until she was confined, when her infant was found to be quite healthy, but to have no arms. On the left side the glenoïd cavity and acromion process of the scapula may be plainly felt, but there was no trace of a humerus. The right side is similar to the left, except that a fleshy protuberance was attached to it, surmounted by a well-formed finger. This case was quoted by Mr. Chance, in his work above quoted (see p. 507) on 'Bodily Deformities,' and at the time led to a correspondence in the 'Lancet' as to whether maternal impressions had any influence on the fœtus or not. I myself have good authority for two cases in the human subject of malformation of the upper extremities resulting from fright in the mother.

The following case is related in volume ix of the 'Linnæan Society's Transactions' (p. 323) by Mr. G. Milne. A young female cat, pregnant for the second time, and at the middle period of gestation had its tail heavily trodden upon and injured. She screamed most frightfully, and ran out of the room, and from the nature of the noise which she emitted it was evident that a considerable amount of terror mingled with the sense of injury. Eventually she gave birth to five kittens, "one of which, exactly resembling herself, was apparently perfect; but the other four had the tail most remarkably distorted. About one third of the length, reckoning from the base, there was a *nodus*, equal in size to a very large pea, or about twice as thick as the tail itself, the remaining portion being turned on one side at an angle nearly approaching a right angle, and, what may deserve notice, all of them turned the same way, towards the left side." All the kittens were at once destroyed, excepting the apparently perfect one. As this afterwards became ill it was also killed, but it was found that "this also had the tail distorted and turned aside at a considerable angle, although free from the knot which distinguished the other four."

I have already (see pp. 506-7) quoted a case of hereditary deformity of the hand, which appeared to have its origin in an injury to a woman during pregnancy.

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[1] See October, 1850, p. 466.

I may here observe that Geoffroy St. Hilaire, Serres, Breschet, Soemmering, Meckel, were all in favour of maternal impressions being communicated to the fœtus, as mentioned by Mr. Davis in connection with the observations which I have related (see p. 516). I find also that Burdach was inclined to believe in this influence of the mother on the fœtus; and that Morgagni, Boerrhaave and Van Swieten (see article "Fœtus," by Montgomery, in the 'Cyclop. Anat. and Phy.,' above quoted) did not deny it.

The above illustrations of transmission of deformities are all taken from mammals. My friend Dr. Günther, of the British Museum, speaking of deformities of fish, observes that in certain localities defect of the intermaxillaries is common with the river trout; and as these cases are limited to certain places, it may be assumed that this is hereditary. He informs me that a paper had been read at the British Association in Edinburgh on a locality where most of the trout are without caudal fin.

Among plants hereditary transmission of defects is known to be not uncommon. Darwin alludes to the subject in his work on 'Variation of Animals, Plants,' see vol. i, pp. 324 and 365, and also vol. ii, pp. 18, 70, and 166. Dr. Maxwell Masters informs me that the inheritance of abnormal or unusual characters is a very common phenomenon in plants under cultivation. "Most of the so-called 'florist's flowers,' such as pelargoniums, dahlias, asters, roses, auriculas, are abnormal, and are, as well as double flowers in general, reproduced from seed." The same holds good with many "cultivated vegetables, cabbages, broccoli," &c. &c.

In Dr. Masters' 'Vegetable Teratology' (Ray Society) are observations connected with the subject, see p. 15, as regards inheritance of fasciation, also pp. 226, 235, 305, and 396. See also last paragraph on p. 490, and the appendix regarding double flowers. Dr. Masters remarks that the inheritance of abnormal peculiarities is not confined to cultivated plants, but is seen also in wild plants, though to a less degree.

I now add observations of Dr. Shorthouse (to which at page 509, foot-note, I have alluded), and which he has placed at my disposal.

He remarks:—"I divide the lower orders of animals into two 'divisions,' 'races' and 'breeds;' the 'races' being those which naturally herd together, breed together; and the 'breeds' those animals which have been produced or manufactured by the sedulous care of man."

"As illustrations of 'races' we may take deer. The red deer herd together, and never associate or seek sexual congress with fallow deer, but keep aloof. In the park opposite my own house there is a herd of red deer, used for stag hunting with the Surrey hounds, and a herd of fallow deer indigenous to the park; these two herds keep aloof and never even in rutting season come in contact. Amongst cattle, the Devons, Herefords, Alderneys, &c., are the pure 'races,' and the renowned Shorthorns the mongrels or manufactured animals. Amongst sheep the Southdowns are a pure 'race,' but the renowned Leicesters are the manufactured articles. All the horses we have in this country are mongrels, all manufactured. We have not a pure race like the Arab. Some, of course, are called 'thorough-bred,' and some halfbred or cocktails, but that is because the repeated thoroughbreds or their ancestors found their way into the stud book some years ago, and the owners of the 'cock-tails' did not take the trouble to obtain admission into such a pantheon.

"But what I am driving at in my distinction between races and breeds is to show that in the pure races there is no degeneracy and but very seldom any deformity or monstrosity; they hold their own in a state of purity and perfection without the interference of man. The 'breeds,' on the other hand, if left to themselves after being 'manufactured,' resolve themselves into their elements, and traces of all the mongrels and other animals of which they were concocted show themselves in a few generations. The Shorthorns, for example, were made up of Devons, Herefords, Alderneys; and if you breed from Shorthorns now-a-days, *i. e.* from the best Shorthorn bull with the best Shorthorn cow, you must not be certain what the offspring will be. It may be a Devon, Hereford, or some nondescript animal, the like of which was never seen before. Breeders of Shorthorns know well enough that at every generation they must either infuse new blood or dip again into one of the original fountain stocks to prevent their breeds from degenerating. The intelligence and care of man are necessary to keep them from decay or from monstrosity, or from resolving themselves into their elements. This is not the case with the pure races; wherever they may be located, whether on the banks of the Exe, or in the meadows of the Trent, or on the plains of upland counties, they always remain steadfast to themselves and produce reduplications of papa and mamma, and it is but seldom indeed that a deformed, mutilated, or diseased youngster is produced. The human 'race,' 'breed' I call it, as we know it in England, is, of course, only mongrel, a compound of all sorts of races and tribes, and, therefore, deformities, malformations, degeneracy, idiocy, mutilation, &c. &c., are not very uncommon."

Dr. Shorthouse has also kindly given me the following memoranda, which are highly interesting in connection with the subject of breeding and transmission of peculiarities. He says he first received the hint on the subject from Mr. South.

He observes that men with cleft palates do not beget children; they copulate in all ways like other men, but do not procreate. It is a fact for which there is no explanation. He remarks, "We are all acquainted with the relationship which the tonsils and glands at the posterior part of the palate have with the organs of generation; also of the fact that the excision of the tonsil causes the absorption of the testes. Parotitis also is very frequently the cause of the wasting of the left testis. Of this circumstance I have seen at least thirty cases. It is probably because the tonsils and other glands at the posterior part of the mouth are either defective or interfered with. The non-fecundity does not extend to the weaker sex, as women with cleft palate breed as well as their more developed sisters. It is also a fact that cleft palate is about seven times as frequent in females as in men." He goes on to say next, "the young lions (or lion cubs) which are whelped at the London Zoological Society's Garden are all of them born with cleft palate, and ALL OF THEM DIE. I do not mean those which are the issue of *one* particular lion or lioness, but of all the pairs of lions and lionesses at the gardens. It seems a sort of indigenous deformity. But, as a matter of fact, the managers cannot rear any young lions; they all of them die and all of them have cleft palates.[1] Now, in travelling menageries and also at the Dublin Zoological Gardens, the lions and lionesses rear up cubs to maturity, and none, or, at any rate, very few, if any, have cleft palates. But, at any rate, they live and are reared, whilst those born at the London Zoological Gardens all die in infancy." Is this difference, I would ask, solely attributable to climate?

In a further communication, Dr. Shorthouse observes, "There are many other branches of the extensive subject of breeding in which I have amassed a number of statistics, such as the nature of sterility, the propagation of twins. These two circumstances, though at first sight they may appear very wide, are closely akin to each other, and I have a mass of evidence in support of such a conclusion which is overwhelming. The tendency to abort is also nearly allied to the tendency to produce or beget twins, or to impotency in the male and sterility in the female.

"If I am convinced of any one thing more than another relating to the function of reproduction, it is that impotency, sterility, the procreation of twins, and the tendency to abort, are but phases of the same condition. They may seem paradoxical, but it is a fact which I have verified in many hundreds of cases. The mere procreation of twins is no proof of extraordinary virility; on the contrary, it is coexistent with a condition like that of impotency. I have noticed the fact hundreds of times amongst horses, bulls, pigs, and sheep, and in not a few cases in the human family. One of the

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[1] This statement is, I find, not strictly accurate. My friend, Mr. Sclater, the Secretary of the London Zoological Society, informs me that though in several instances the lions born in the Gardens have had cleft palates, it is not always so. He is unable to give any explanation of the fact.

famous families of thorough-bred horses is known as the Pantaloon line, because about thirty years ago a horse of that name was famous on the turf. It is a singular fact that the majority of his daughters have a tendency to barrenness, but if they do happen to get a foal they are very likely to produce twins or to slip twins before they have gone the full period. They are either sterile or supernaturally fruitful. The sons, too, of Pantaloon were all but impotent. One of them (Windhound) covered thirteen mares one year at the Rawcliffe stud; eleven of these were barren, and one of the other two produced twins. Hobbie Noble, another son of Pantaloon, a brother of Windhound, was almost impotent in his early life and *quite* so during his latter years; all the mares put to him proving barren. He could cover the mares but was incapable of fertilising the ovum, but when he did fertilise the ovum he generally fertilised two ova. The Dupe, another son, had a similar propensity. Subjoined are a few names of mares of this family with the results of their stud life.[1] The idiosyncrasy seems to be transmitted for several generations.

"The Melbourne line is also another family prone to impotency and sterility.

"In the human family I have seen hundreds of times that when abortion with early months of pregnancy takes place, it is ALMOST invariably with twins. In my earlier life I had a very extensive midwifery practice, and in cases of abortion whenever I took the pains to search I almost invariably found two or more fœtuses in the discharges and clots."

In some "Notes on Breeding" by the same writer in the 'Sporting Gazette' for March 21st and 28th, 1863, the readers, in addition

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[1] No. 1. Barren in 1858, 1859, 1862, 1863, 1864, 1866, and 1867.
2. Ditto 1856, 1858, 1860, 1861, 1864, and 1865.
3. Ditto 1856, 1858, 1860, 1864, and 1866; slipped foals in 1862 and 1868.
4. Ditto 1858, 1859, 1860, 1862, and 1864.
5. Ditto 1857 and 1860; twins in 1858.
6. Ditto 1867; twins in 1861.
7. Ditto 1858, 1860, 1864, and 1867; twins in 1867.
8. Ditto 1858, 1860, and 1864; twins in 1861.
9. Ditto 1858, 1863, and 1864; slipped foals in 1859 and 1865.

Pantaloon Mares, i.e. daughters of the Horse Pantaloon.

Caricature.—1856, twins; 1852, barren; 1854, barren; 1855, barren; 1861, barren; 1863, twins; 1866, slipped foals; 1867, barren; 1868, barren, and then she was destroyed.

Legerdemain.—1849, slipped a foal; 1856, slipped twins; 1860, slipped twins; 1862, slipped twins; 1852, barren; 1859, barren; 1864, barren; 1866, barren.

Lady John.—1855, barren; 1856, barren; 1858, barren; 1859, barren; 1863, barren.

Miserrima.—1856, slipped foal; 1859, slipped foal; 1863, slipped foal; 1860, had twins (dead); 1862, had twins; 1855, barren; 1858, barren; 1867, barren.

Crystal.—1858, barren; 1860, barren; 1865, barren; 1866, slipped twins.

Slander.—1851, barren; 1854, barren; 1864, barren; 1865, barren; 1866, barren; 1857, slipped twins.

to observations on the effects of impregnation on the female with reference to future offspring,[1] will find interesting remarks on the qualities contributed by each parent to their offspring. It is determined that the male parent chiefly and within certain limitations and restrictions contributes the external characters, the general appearance, in fact, the outward structures and locomotive powers (*e. g.* brain, nerves, organs of sense, skin, bones, and muscles), while from the female are derived the internal structure and the general size and quality, the vital organs, heart, lungs, glands, digestive organs, and the tone and character of defective nutrition, &c. Further allusion is also made to the distinction between "races" and "breeds," and to the self or uniform cattle of pure colour in contradistinction to the patches and glaring colours of cross-breeds, a fact recognised by the ancients and supported by quotations from Homer and Virgil.

As connected with hereditary proclivities I may mention the fact that susceptibility to the effect of certain remedies as mercury and opium has been known to be hereditary, as mentioned by Mr. J. Brown in his remarks on hereditary transmission of disease (see 'Cyclop. of Pract. Med.') A curious statement is made by Mr. Wallace in his work, 'Contributions to the Theory of Natural Selection,' to the effect that WHITE pigs and sheep are not affected by certain plants in the same way that dark ones are.

P.S.—In reference to the subject of Polydactilism, alluded to at a former page, I would quote the case very recently described by Dr. Arthur Mitchell, in his third Morisonian Lecture, given at Edinburgh (see 'Lancet,' March 16, 1872, p. 383). It was that of a man whose father and brother and sister had six fingers on each hand, whilst he himself had the natural number. He married, and his first child was six-fingered.

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[1] Count Strzelecki is quoted to show that if fruitful intercourse take place between an European male and an aboriginal female of New South Wales or Van Diemen's Land, that the female is for ever barren to a male of her own race, and only capable of procreating with white men. Strzelecki, at page 347 of his 'Physical Description of New South Wales and Van Diemen's Land' (London, 1845), referring to the above fact, observes:—"Hundreds of instances of this extraordinary fact recurring invariably under the same circumstances amongst the Hurons, Seminolas, Red Indians, Gakirs, Mendoze Indians, Araucos, South Sea Islanders, and natives of New South Wales and Van Diemen's Land, and all tending to prove that the sterility of the female which is relative only to one and not to the other male is not accidental but follows laws as cogent, though as mysterious, as the rest of those connected with generation." This law does not extend to the Negro race. Section vii of the Count's work, on the aborigines of New South Wales, &c., their language, customs, characteristics, &c., is full of interest, and will well repay the reader. Although some of the facts there recorded are startling, Dr. Shorthouse tells me that the late Mr. Goodsir assured him they were strictly true, and had been corroborated by several travellers and acute observers since the Count published his work.

The reader will find observations on the influence of a first impregnation extending to the products of subsequent ones in the article on "Generation" in the 'Cyclopædia of Anatomy and Physiology,' vol. ii, p. 468. The influence is traced not only in the cases of the mare and the ass, the mare and the quagga, but, according to Burdach, the sow and the bitch, as well as man.

Chronicle of Medical Science.

REPORT ON PHYSIOLOGY.

By Henry Power, F.R.C.S., M.B. Lond.,
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Absorption—Blood-Circulation.

1. H. Auspitz. *On the Absorption of Insoluble Substances in Mammals.* ('Wiener Medizinische Jahrbücher,' N. F., 1871, Band iii.
2. Dr. Genersich. *On the Absorption of Lymph by Tendons and Fasciæ of the Muscles.* ('Ludwig's Arbeiten aus der Physiologischen Anstalt zu Leipzig,') Fünfte Jahrgang, 1870.
3. W. Manassein. *On the Variation in Size of the Red Corpuscles of the Blood as a result of the action of various Reagents.* ('Centralblatt für die Med. Wiss.,' No. 44, 1871.)
4. Dr. D. Gatzuck. *On the Influence of Loss of Blood on the Circulation and on the Temperature of the Body.* ('Centralblatt für die Medizinische Wissenschaften,' No. 53, 1871.)
5. Jakob Worm Müller. *On the Tension of the Oxygen in the Blood Disks.* ('Ludwig's Arbeiten, &c,' 5te Jahrgang, 1870.)
6. Adolf. Gärisch. *Researches on the Inorganic Constituents of the Blood.* 'Stricker's Medizin. Jahrbücher,' Heft iv., Jahrgang, 1871.)

Fr. Jolly. *Researches on the Pressure of the Brain and the Movement of the Blood in the Skull.* (Pp. 65, 'Habilitations Schrift,' Wurzburg, 1871.)

G. Althaun. *Essays on the Physiology and Pathology of the Circulation.* Pp. 247. Dorpat, 1871.

F. Pagenstecher. *Experiments on the Brain Pressure.* Pp. 62. Heidelberg, 1871.

H. C. Sorby. *On Some Improvements in the Spectrum Method of Detecting Blood.* ('Monthly Microscopic Journal,' 1871, vol. vi, p. 9. An important paper for toxicologists.)

J. C. Richardson. *On the Cellular Structure of the Red Blood-corpuscle.* ('Monthly Microscopic Journal,' 1871, vol. vi, p. 17. Reverts to the old cell theory.)

Thomas L. Ralph. *Experiments with the Microscope on the Chemical Effects of Chloral Hydrate, Chloroform, Prussic Acid, and other Agents, on the Blood.* ('Monthly Microscopic Journal,' vol. vi., p. 75, 1871.)

BOYD MOSS. *Hematozoa in the Blood of Ceylon Deer.* ('Monthly Microscopic Journal,' vol. vi, p. 181.')

1. Auspitz, after reviewing the various researches that have been made in regard to the absorption of insoluble substances, gives the results of his own observations on rabbits. The substance he employed and recommends is starch meal suspended in water or oil, which is well adapted for the purpose from its low specific gravity, the well-marked form of its particles, their reaction with iodine, and their aspect under polarized light. He found that when injected into the veins, following the current of the blood, the particles reached the lungs, where the greatest portion was arrested, without occasioning any obstruction to the blood or inflammation, whilst a small quantity, chiefly consisting of the smaller granules, entered the systemic circulation, and was distributed to, and discoverable in, the liver, spleen, kidneys and vessels generally. The grains, whether suspended in oil or in water, but especially in the former, were readily absorbed, both from the peritoneal cavity and from the connective tissue when injected subcutaneously. The granules thus absorbed considerably surpassed in size both the red and the white corpuscles. Some were found in the thoracic duct, showing that the lymphatic system took part in the process of their absorption. Lastly, a salve containing starch-granules was rubbed into the shaved skin of rabbits shortly before or after its separation from the living animal, and this was then examined from within in successive layers, special precautions being taken to avoid any introduction of the starch meal from without. The granules were found in the tissue of the corium of the subcutaneous connective tissue and the muscles connected therewith, but not in the depth of the sebaceous glands and hair-follicles. The skin of children also after death showed, when examined in the same way, that it was permeable to starch-granules after infrication of the salve for a quarter of an hour. The presence of fat or oil, as a general rule, materially favoured the process of absorption.

2. M. Genersich states that after V. Recklinghausen's, Ludwig's, and Schweigger Seidel's researches on the lymphatics of the central tendon of the diaphragm, it appeared very probable that not only this but other aponeurotic structures stand in close relation to the lymphatic vascular system. In favour of the same view were the observations of Ranvier, which demonstrated that the fissures between the tendinous fasciculi are lined with a series of smooth cells. In injection experiments of this nature it is necessary for success that the tissue should be kept thoroughly stretched, and that the injection should be driven in under low pressure. Genersich finds the best material for injection, for many reasons, to be turpentine coloured with alkanin, though he admits it has also its drawbacks. He obtained the most satisfactory results from the lower part of the fascia lata, the fascia antibrachialis, cruralis, and the external lamina of the sheath of the rectus. He adduces evidence to show that the action of the muscles plays an important part in causing the lymphatics of the tendons to absorb by a kind of suction the lymph secreted in and around

the muscular masses. To demonstrate this he injects defibrinated blood through the limb of the animal from which it has just been taken, and incidentally enters into a very interesting discussion in regard to certain partial contractions which occur under these circumstances in the arteries, and which in some points of view resemble, and in others differ from, rigor mortis. He then ascertains the amount of lymph discharged from the thoracic duct in a given time, both when the muscles are quiescent and when excited to powerful and sustained action by induction currents of electricity, and found that this varies from 1 : 4·45 to 1 : 24·5 in favour of the period when the muscles are called into play, which are thus shown to exert an immense influence on the flow of the lymph.

3. The results of 40,000 measurements on 174 animals of different species have shown M. Manassein that the size of the red blood-corpuscles *diminishes* under the action of septicæmic poisoning, exposure to a high temperature, and to an atmosphere containing an excess of carbonic acid, whilst they *enlarge* under the influence of oxygen, and agents lowering the temperature of the body, as cold, quinine, cyanic acid and alcohol. Morphia, however, forms an exception. The corpuscles enlarge in acute anæmia.

4. Dr. Gatzuck's observations were all made on dogs, and the following are the results he obtained: 1. Venesection retards the mean velocity of the current of blood in the carotid and crural arteries and their branches. 2. The withdrawal of blood from a vein or artery of the anterior extremities exercises a greater influence on the rapidity of the current of the blood than a like evacuation from the posterior extremities. 3. The effect produced by the mean velocity of the blood current depends on the quantity of blood withdrawn. 4. The mean pressure of the blood falls under the influence of the withdrawal of blood, although occasionally it remains unaltered, or may even augment; these variations depend both upon the quantity discharged as well as upon the rapidity with which the blood is allowed to flow. 5. After the withdrawal of blood the rapidity of the current and the pressure exerted by the blood quickly return to their normal condition. 6. Coincidently with the diminution of the velocity of the blood current and of the lateral pressure, the pulse usually increases in frequency. When blood is withdrawn in large quantities and rapidly, the sounds of the heart (and especially the second) are weakened in intensity. 7. In consequence of the evacuation of blood the temperature of the body falls both during and after the venesection to an extent that, in M. Gatzuck's experiments, amounted to 1° or even 2° Cent.

5. Jakob W. Müller's paper is a very long one. He first undertook to ascertain how far the tension of the oxygen depended upon the degree of saturation of the blood with oxygen. With this object in view he first agitated, under constant pressure and temperature, blood containing but little oxygen with limited (insufficient for saturation) volumes of oxygenated air; the latter, of course, gave up a certain proportion to the former till a condition of equipoise resulted; and

secondly, blood rich in oxygen was agitated with nitrogen, when the blood yielded up a portion of its oxygen. He found that no complete saturation of the blood with oxygen occurs if the oxygen pressure falls below from 20—30 millimètres at 40° Cent.; (2.) that with further depression of the oxygen pressure the saturation of the blood with oxygen still further diminishes; and (3), that the pressure value of the oxygen of the air, corresponding to one and the same degree of saturation, augments with elevation of the temperature. The last two results, however, are inconstant and irregular. Müller then undertook investigations on the relations of hæmoglobin and oxyhæmoglobin to oxygen. His researches, he thinks, though he speaks with great caution, furnish the key to explain the dependence of the absorption of oxygen in the blood of the pulmonary circulation on the amount of that gas contained in the air in the lungs. (2.) The dependence of the amount of oxygen yielded up by the blood to the tissues upn the amount of oxygen contained in the blood.

6. Järisch gives the following as the arithmetical mean of four very carefully conducted experiments, made with a view of determining the constituents of the ashes of the blood of the dog.

Phosphoric acid anhydride	12·32	per cent.
Sulphuric acid anhydride	4·01	„
Chlorine	31·43	„
Potash	3·83	„
Soda	42·01	„
Lime	1·25	„
Magnesia	0·65	„
Oxide of iron	8·34	„

Digestion—Secretion.

1. Dr. Lépine. *On the Origin and Distribution of the Animal Sugar-Ferment.* ('Ludwig's Arbeiten aus der Physiologischen Anstalt zu Leipzig,' 5te Jahrgang, 1870.)
2. E. Friedinger. *What Cells of the Peptic Glands contain the Pepsin?* ('Wiener Acad. Sitzungsber.' Band lxiv., October, 1871.)
3. C. Ustimowitsch. *Experiments on the Theory of the Secretion of Urine.* ('Ludwig's Leipzigen Arbeiten,' 5te Jahrgang, 1870.)
   Dr. Hofmann. *On the Passage of Free Acids through the Alkaline Blood into the Urine.* ('Zeitschrift für Biologie. Band vii, Heft iii.
4. T. Bogomoloff. *On the Composition of the Milk.* ('Centralblatt für die Medicinischen Wissenschaften,' No. 40, 1871.)
5. Gscheidlen. *On the Origin of Urea in the Animal Body.* (Leipzig, 1871. Engelmann, pp. 44.)

1. M. Lépine's investigations started with the question whether starch was converted into sugar within the cavity of the mouth of the frog. To determine this he placed some boiled starch in a test tube and introduced some fragments of mucous membrane from the tongue and oral mucous membrane into it. In from thirty to sixty minutes the presence of sugar was demonstrable by Trommer's test. Mechanical irritation of the tongue, or irritation of the hypoglossal, again caused the secretion of a slimy fluid, which pos-

sessed powerful catalytic properties. When the tongue was persistently irritated, then cut out and quickly immersed in Müller's fluid, sections of it made when it was hardened showed that numerous cells were accumulated in the vicinity of the acini of the glands. The presence of an animal ferment capable of converting starch into sugar has been shown by Magendie, Nasse, Cl. Bernard, Piotrowski, Thiry, Wittich, and others, to exist in the blood, in the thoroughly washed mucous membrane of the stomach and intestine, in the liver and bile, in the kidneys, the mucous membrane of the bladder, in the brain, and in the muscles. In addition, M. Lépine has shown it to be present in dogs, rabbits, and frogs, in the spleen, the tissue of the lungs, testicles, tendons, serous membranes, cornea, and vitreous; in the external and internal surfaces of the skin of the frog, in the mucus of the ovum of the frog, and in the mucous lining of its oviduct. The only situation in which he has sought for it and has been unable to find it is the crystalline lens of the eye. The ferment is not contained in equal proportion in all the tissues, weight for weight; it is more abundant in the blood, muscles, spleen, vitreous body, testicles and brain than elsewhere. The diffusibility of the sugar ferment is small.

2. Friedinger, as the result of his researches on various classes of animals, arrives at the conviction that the older views were correct, and that, in opposition to the statements of Ebstein, the covering, investing, or superficial cells of Heidenhain, the adelomorphous cells of Rollett, are those which really contain and form the pepsin.

3. Ustimowitsch's paper is a long one, and embraces many points of interest. His researches were undertaken in dogs which had taken no food, either solid or fluid, for eighteen hours, and the urine was collected from the ureters. He found that the lowest pressure of the blood, in the blood-vessels, under which urine continued to be secreted, was about fifty millimètres of mercury, the variations in pressure being occasioned by section of the renal nerves. After pointing out that the experiments of others have demonstrated that the rapidity with which the water, urea, and common salt, of the urine are executed varies with the pressure of the blood in the arteries, with the section of the renal nerves, with the resistance that the urine meets with in escaping by the ureters, and with the amount of these materials contained in the blood; he details his own experiments, showing the changes that the composition of the urine undergoes as a result of the action of woorara; he has never been able to find sugar under these circumstances, though Schiff and others have shown it to be often present. He remarked two points of interest—first, that just as the animal became fully under the toxic influence of woorara the secretion of urine was either arrested or reduced to a minimum; and secondly, that the proportion of urea and chlorides, even in the sparingly secreted urine, was decidedly diminished. The diminution of the quantity of the urine depended, in all probability, on the change of artificial pressure caused by the action of the poison.

4. Bogomoloff gives an account of comparative experiments he has instituted between the milk of the cow and goat and that of the

human subject. He believes, from their reaction with ether, that the milk-molecules of the cow and goat are not, as is usually considered, pure drops of oil, but a mixture of fat and albumen. Human milk-molecules present peculiar characters, which require further investigation.

5. Gscheidlen has examined almost every tissue in the body for urea, and finds it universally distributed, except in the muscles, in which it is never present.

Respiration—Animal Heat.

1. Dr. N. O. Bernstein. *The Exchange in Gases between Arterial and Venous Blood.* ('Arbeiten aus der Physiologischen Anstalt zu Leipzig,' Fünfte Jahrgang, 1870.)
2. H. Quincke and E. Pfeiffer. *On the Blood Current in the Lungs.* ('Reichert und Dubois Raymond's Archiv,' 1871.'
   A. H. Garrod. *On the Relation of the Temperature of the Air to that of the Body.* ('Humphry and Turner's Journal of Anatomy,' Nov., 1871.)
   H. Senator. *On the Development of Heat and Disintegration of Tissue in Health and Disease.* ('Centralblatt für die Med. Wiss.,' No. 47, 1871.)
3. C. Pilz. *The Normal Temperature in Childhood.* ('Jahrb. für Kinderheilk,' N. F. Band iv, p. 414.)
4. W. Manassein. *On Agents effecting a Reduction of the Temperature of the Body.* ('Pflüger's Archiv, Band iv., p. 283, 1871.)

1. Dr. Bernstein remarks that no attempt has hitherto been made to construct an apparatus approximatively resembling the conditions under which an exchange of gases takes place in the placenta. He gives the details of an ingenious instrument he has constructed with this object in view, and represents it in a plate. It consists essentially of a diffusion apparatus composed of two tubes separated by a thin membrane, on one side of which defibrinated arterial blood was kept in motion, whilst on the other was blood obtained from the same vessel after the animal had been asphyxiated. Careful analyses of the kind and quantity of gases contained in the several specimens of blood before and after diffusion were made. The amount of oxygen which diffused from the more richly oxygenized to the more highly carbonized blood, in five or six hours, was unexpectedly small, only amounting to 0·51 per cent. in one instance, and to 0·78 per cent. in another ; nor was the diffusion of carbonic acid in the opposite direction very great, the arterial blood only gaining 1·55 per cent., whilst the venous lost about 2 per cent. These experiments show that the exchange of gases dissolved in fluids is extremely small. The following are two of the experiments :

	Original blood.		Blood after diffusion.		Sum of the gases.		REMARKS.
	Arterial.	Venous.	Arterial.	Venous.	Before diff.	After diff.	
I.							
Oxygen	16·97	2·85	16·46	2·65	19·82	19·11	Duration of diffusion 5 hrs.; Temp. 15° C.
Carbonic acid	29·80	42·97	31·76	38·02	72·77	69·78	
Nitrogen	1·53	1·64	1·38	2·19	3·17	3·57	
II.							
Oxygen	14·95	1·98	14·17	2·03	16·93	16·20	Duration of diffusion 5 hrs.; Temp. 18° C.
Carbonic acid	38·00	46·77	39·55	44·89	84·77	84·44	
Nitrogen	1·34	1·80	1·87	1·52	3·14	3·39	

2. The practical outcome of Quincke and E. Pfeiffer's observations and experiments is, that with each *in*spiration the passage of blood through the vessels of the lungs towards the left ventricle is *accelerated.*

3. Pilz finds from measurements taken in the rectum that the temperature rises from midnight to midday, though with interruptions, the most rapid rise being between the hours of 7 and 9. The temperature falls, on the other hand, from midday to midnight.

4. Manassein finds that swinging lowers the temperature of the body in rabbits, though precautions were taken against the loss of heat by the contact of air.

NERVES—MUSCLE.

1. M. GEORGES POUCHET. *On the Connection of Nerves and Chromoblasts.* ('Monthly Microscopic Journal,' vol. vi, p. 285.)
   Dr. LIONEL S. BEALE. *On the Relation of Nerves to Pigment and other Cells or Elementary Parts.* ('Monthly Microscopical Journal,' vol. vii., p. 45.)
2. C. DITTMAR. *A Fresh Proof of the Excitability of the Centripetal Fibres of the Spinal Cord.* ('Arbeiten aus der Physiologischen Anstalt zu Leipzig' during the year 1870. Mitgetheilt durch C. Ludwig, 1871.)
3. O. SCHMIEDEBERG. *Researches on the Action of some Poisons on the Heart of the Frog.* ('Arbeiten aus der Physiologischen Anstalt zu Leipzig,' 5th year, 1870.)
4. MAHOMMED EFFENDI HAFIZ. *On the Motor Nerves of the Arteries of Transversely Striated Muscle.* ('Arbeiten aus der Physiologischen Anstalt zu Leipzig,' 5th year, 1870.)
5. Dr. F. MIESCHER. *On the Conduction of Sensory Impressions in the Spinal Cord.*
   J. BERNSTEIN. *Researches on the Process of Excitation in the Nerves and Muscles.* 8vo, pp. 240. Heidelberg, Winter.
6. E. LONDOLT. *On the Distance of the Macula Lutea from the Nervus Opticus.* ('Centralblatt für die Medizinische Wissenchaften.)

7. A. Paulus. *On the Tactile Sensibility in regard to Space of the Lower Limb.* ('Zeitschrift für Biologie, Heft iii, Band vii, 1871.)

Dr. E. Klein. *On the Peripheral Distribution of Non-medullated Nerve-fibres.* ('Quarterly Journal of Microscopical Science,' Jan., 1872. Also in the same journal, and by the same author, a paper on "*Remak's Ciliated Vesicles and the Corneous Filaments of the Peritoneum of the Frog.*

M. Lavdowsky. *On the Contractility of Muscular Protoplasm.* ('Centralblatt für die Med. Wissenschaft,' Nov. 25, 1871.)

1. M. Pouchet gives a minute account of the structure of the chromoblasts of fishes, as of various flat fish, which he describes as being essentially composed of a mass of sarcodic substance (or protoplasm) usually surrounding a nucleus, but being able, probably, to exist without one. In the midst of this sarcodic substance is deposited the colouring matter, which is consequently a true pigment. This is of various tints—yellow, orange, red, brown, or black. Sometimes this colouring matter is liquid and forms—as may be seen in the embryos of crustacea—a coloured drop in that portion of the sarcodic substance near the nucleus. When it extends in its amœboid movements it draws over it the coloured drops. At other times the colouring is distributed in the form of granulations throughout the mass of protoplasm. M. Pouchet's observations were made on chromoblasts subjected to the action of acetic acid and carmine or chloride of gold. He shows (a plate accompanying the paper) that delicate nerve-fibres may be traced up to the chromoblasts, and conceives that these bodies are interposed in the course of the nerve-filament, or are in direct contact with it, in consequence of which connection they enter into contraction whenever the nerve is acted upon. If this be true the termination of the nerve-filaments has still to be sought for, and he depicts some suggestive specimens in which the nerve appeared to end in a nucleus with pigment matter on one or both sides of it, the nucleus presenting all the characters of a nucleus of nervous fibre, and not at all those of the large irregular nuclei which usually accompany sarcodic bodies.

2. Dr. C. Dittmar remarks that A. V. Bezold, in his treatise on 'A New Excitor Nerve System for the Heart,' made the observation that the slightest excitation of the skin, or even of the auditory nerves, is answered by an exaltation of the arterial blood pressure and an increase in the number of the pulse, providing that the animal has been placed fully under the influence of woorara, and the trunks of the pneumogastrics have been divided in the neck. These observations have been corroborated and extended by Lovén and Asp. Now, since, according to the researches of C. Ludwig and Thiry, the augmentation of the blood pressure depends on the contraction of the circular fibres of the smallest arteries, which contraction is produced reflectorially by the sensory nerves, this reaction is well adapted to the demonstration of the sensory properties of a segment of nerve. Led by this consideration, C. Ludwig suggested to Dr. Dittmar the advisability of studying the

much discussed question of the excitability of the substance of the spinal cord. The demonstration of the presence of such excitability could, up to the present time, be furnished only in the case of the anterior columns, for the excitability of which we have a sufficiently certain and delicate reagent in the contraction of the muscles. No one has hitherto even attempted it in the case of the æsthesodic substance. Dr. Dittmar employed rabbits in this investigation, which were subjected to the action of woorara, but the vagi were not divided. The pressure of the blood in the carotid was registered by means of a mercurial manometer. Speaking generally, he had frequent opportunities of corroborating v. Bezold's observations. In all cases, where even a feeble excitation affected a sensory nervous expansion, an increase of the blood pressure was observed as an expression of an increase of resistance in the arterial region, caused by reflex contraction of the vascular walls. This reflex action on the vascular nerves, as we know takes place at the medulla oblongata. It appeared at the same time that the elevation of pressure increases with the strength of the excitation, and could thus be employed to some extent as a proportionate measure for the degree of excitation applied. In experiments of this nature it is of importance that the excitation should be of nearly equal duration, if the effects are to be compared. It must also be remembered that the reflecting organ is susceptible of exhaustion, so that if two excitations of equal strength are applied to the same part, the effect produced by the second is usually less than that by the first. Dr. Dittmar points out the importance of avoiding loss of blood, and the necessity of maintaining artificial respiration, not by hand, but by a proper automatic apparatus. It was a matter of capital importance to Dr. Dittmar to corroborate the observation made by Asp, to the effeet that excitation of the spinal cord was capable of setting free the same reflex action on the vascular nerves, the vascular nerves given off below the point of irritation being, of course, withdrawn from the direct action of the stimulus by section of the cord. Their direct excitability has not been contested by any one since the researches of Ludwig and Thiry. In order to see whether positive results could be obtained, in regard to the excitability of substance of the spinal cord, Dr. Dittmar arranged the following experiment. He excited an intercostal nerve centripetally, and compared the augmentation of pressure produced with that induced by equally strong excitation of the medulla at the point of entrance of the nerve. If, he reasoned, the elements of the spinal cord are really excitable, their excitation must produce a greater effect than an individual nerve, which only goes to form part of the cord. The result of the experiment corresponded to his anticipations; the irritation of the cord produced a much greater effect than that which could be obtained by excitation of the intercostal nerve. Undoubtedly this does not prove the excitability of the spinal cord, since the stimulus may possibly have been applied to nerve-fibres that have not traversed any ganglion-cell; Dr. Dittmar therefore performed another experiment, of which he gives the details which essentially consists in removing the posterior columns of the spinal cord; and he finds not only that the cord is capable of receiving and conducting centripetal impressions, but that it is one of the most excitable structures (reizbarsten Gebilde) of the body. In order to determine the path pursued by the excitation, he further separated the anterior from the lateral columns. Excitation of the anterior columns, and also of the grey substance alone, was never followed by any effect, but that of the posterior columns was followed always by some, though slight, increase of pressure. As the general result of his experiments, he thinks it unquestionable that there is a system of fibres in the substance of the spinal cord, which, though they do not belong to the nerve-roots, are accessible to the action of direct stimuli, and are capable of transmitting the impulses so generated along the whole length of the spinal cord to the medulla oblongata, where they are reflected upon motor nerves. They may be regarded as excito-motor nerves. Dr. Dittmar then proceeds to discuss the action of stimuli similarly applied upon the pulse.

3. O. Schmiedeberg states that experiments made by Koppe and himself, and published in 1869, show that the administration of the smallest quantity of the peculiar fungus termed *muscarin* causes the motion of the heart of frogs to be arrested in diastole, without, however, destroying the excitability of the organ. If, now, an extremely minute portion of atropine be subcutaneously injected, the heart is immediately, so to speak, released, and large doses of *muscarin* are then inoperative. It would appear, therefore, that *atropine* paralyses those parts of the nervous system, the electrical or other excitation of which induces an inhibitory action, whilst, on the contrary, *muscarin* acts as an excitant of those parts that are paralysed by atropine, and which possess an inhibitory power. The injection of 1-3rd to 1-8th of a milligramme of *nicotin* causes speedy reduction in the number of the heart's beats and ultimate arrest, which lasts from 1 to $1\frac{1}{2}$ min., when the beats again recommence. Arrest of the heart's action does not occur if atropine has been previously used. The action of small doses of nicotin, therefore, resembles that of muscarin ; it occasions a diastolic arrest of the heart's action, which can be prevented from occurring by paralysing the inhibitory apparatus by atropine. The heart recommences to act when small doses of nicotin have been used, on account of the progressive paralysis of the vagus. With larger doses of nicotin this paralysis sets in so quickly that there is no time for the heart to be arrested by the exciting action of the poison on the vagus, or at most there is only a retardation of its beats. A curious point is that a heart, of which the vagal terminations have been paralysed by nicotin, behaves like a normal heart to muscarin; which at once suggests that nicotin paralyses other and different parts of the vagus from atropine ; and that these parts lie nearer to the trunk of the vagus than those acted upon by atropine is shown by the circumstance that a diastolic arrest can be excited by muscarin, but not by electrical excitation of the vagal trunk. Dr. Schmeideberg considers that in the trunk of the vagus of the frog besides the inhibitory nerves, there rise other nerves which, on being excited, cause acceleration of the heart's action. For if the normal vagus be irritated the

heart's action is never accelerated, but if the inhibitory apparatus be first paralysed with atropine irritation of the vagus will then cause acceleration. He thinks, with v. Bezold, that the inhibitory apparatus consists of ganglionic elements. *Daturin* behaves, as regards the heart, exactly like atropine.

4. Mohammed Hafiz remarks that the vital properties of transversely striated muscular tissue render it *à priori* probable that its blood current should present some peculiarities. Opposite conclusions might be drawn from a consideration of the physical and chemical changes occurring during contraction, in regard to the flow of blood through the muscles; on physical grounds it would be natural to suppose that less blood would traverse the tissue, whilst the larger amount of carbonic acid eliminated would rather seem to show that more blood traversed it. Experiments undertaken by Sczelkow, Al. Schmidt, W. Sadler and Genersich in 'Ludwig's Laboratory,' are opposed to the exclusive adoption of either of these views, and show that great variations may occur in the rapidity of the current of blood traversing the arteries of muscles, as well as in its pressure, quite independently of the condition of contraction, or relaxation, of those muscles themselves. Their experiments tend to show that great powers of contraction and dilatation must be attributed to the arteries distributed to muscle; and in accordance with this is the anatomical fact that the arteries of muscular tissue contain a very well-developed circular muscular layer. Mohammed Hafiz's researches were undertaken to ascertain the course and action of the motor nerves supplying the arteries. Dogs and rabbits were employed in the experiments, which were either in their natural state or poisoned with woorara, and the following results were obtained:—1. During tetanic excitation of the spinal cord the circular muscular fibres of the arteries distributed to muscles contract slightly and transiently, and never to so great an extent as the circular fibres of the arteries distributed to the skin and abdominal viscera. The contraction, if any, is very slight in curarized animals. 2. The nerves of the circular muscles are very easily exhausted. This is well shown by the fact that a wound of a muscle, provided no large artery is injured, as a rule, bleeds but little, but severe hæmorrhage occurs under the circumstances if the spinal cord be irritated, the amount depending on the increase of blood pressure caused by the stimulation of the cord. The more this augments, the more the arteries of the skin and the abdominal viscera contract, whilst those of the muscles permit free bleeding to take place from them, the hæmorrhage lasting as long as the blood pressure is above the normal. 3. Irritation of the spinal cord caused distinctly observable increase in the rapidity of the current of blood through the muscular arteries, as well as by augmented pressure, cessation of the irritation being followed by diminution of the rapidity of the current. 4. The circular muscular fibres of the arteries distributed to muscles expand and contract independently of the nerves supplied to them, and probably as a consequence of the direct excitability of their own proper muscular fibres. 5. The nerves of a muscle, and the nerves of the artery supplying that muscle, seem,

in some instances at least, to have a different origin. 6. From a medium condition the muscles of the vessels may either contract or dilate ; contraction often occurs if the vessel has been long in a state of dilatation, owing to augmented pressure. It was invariably observed after irritation applied to the spinal cord, thus producing first temporary contraction, then dilatation, and finally very strongly marked and persistent contraction. The increase of pressure in the arteries distributed to muscles is, no doubt, in part due to the contraction that occurs in the cutaneous abdominal and other arteries when the spinal cord is irritated.

5. The plan of Dr. Miescher's experiments consisted in determining what influence section or isolated preservation of certain portions of the spinal cord exercised upon the reflex action of certain nerves arising below the lesion upon the blood pressure. He considers his experiments lead to the conclusion that the centripetal fibres of the sciatic nerves, capable of reflectorially increasing the blood pressure, run (in that portion of the medulla which extends between the third lumbar nerve and the last dorsal) either entirely or principally in the lateral white medullary columns of the medulla. Again, he finds that in the same region of the cord the centripetal fibres of the left sciatic chiefly run in the right and in smaller proportion in the left lateral column, and *vice versa*. Lastly, he shows that the fibres of this nature coming from the lower regions of the cord run in its outermost portion, whilst those that enter at the higher plane run, wholly or partially, in the neighbourhood of the median plane.

6. Londolt finds the distance of the macula lutea from the optic disc to be 3·915 mm., in a horizontal plane, whilst it is 0·785 of a mm. higher. The distance is greater in hypermetropic than in myopic persons.

7. Dr. Paulus shows that the sensitiveness of the skin of the lower extremity, in accordance with the theory of Vierordt, increases with its distance from the axis of rotation of the limb, or, in other words, from the proximal joint, and with the freedom with which its movements can be executed. The leg is then more sensitive than the thigh, the foot than the leg, and the toes than the foot. The sensitiveness in each division again augmenting from its own proximal point, except in the single case of the leg proper, when the minimum acuity is opposite the centre of the tibia ; but this, as Paulus points out, really favours Vierordt's theory, since, under different circumstances, sometimes the upper and sometimes the lower part of the leg moves through the greatest arc. In the arm the increase of sensitiveness from the shoulder to the tips of the fingers is as 1·24 ; in the leg as 1·8½, which may be accounted for by the greater rapidity required in the movements of the arm, as compared with those of the leg.

REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

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On the Application of Electrolysis to the Treatment of Disease. By Dr. A. D. Rockwell, of New York.—After describing the general principles of electro-chemical action, and its influence on dead tissue and on the blood circulating in the living body, Dr. Rockwell adduces several cases, arranged in groups, illustrating the efficacy of electrolysis in the treatment of various forms of disease. Clinical experience shows that living tissue is more readily electrolysed than that which is dead, because the former is more capable of absorption, and its solutions are warmer, and therefore better conductors of electricity. When, therefore, a tumour capable of being electrolysed is submitted to the galvanic current, its fluid constituents suffer decomposition, absorption is hastened by the chemical effects of the current and the mechanical and irritating effects of the needles, and disintegration and atrophy take place. In the successful treatment of tumours much depends on their character; and the general rule is, that the harder the tumour, and the larger the extent of tissue to be acted upon, the greater are the quantity and intensity of electricity required. Dr. Rockwell states that his own practice in this department of electro-therapeutics, although not very extensive, has been on the whole moderately successful. In the treatment of some non-malignant morbid growths his results have been very favorable; but in cancers the success has been very questionable, and he gives no cases from his own experience, and only a doubtful one in the practice of the Bellevue Hospital. In the case of ulcers, the continuous current will sometimes produce a growth of healthy granulations, after all ordinary means have failed; and some skin-diseases, as eczema and psoriasis, may be treated electrolytically by means of broad electrodes of various shapes, covered with flannel or linen, or with simple metallic plates.—*New York Medical Journal*, July, 1871.

On the Use of Phosphorus in Certain Diseases of the Skin. By Dr. H. Eames, of Dublin.—It appears that Dr. Burgess was the first physician who recommended the use of phosphorus in certain skin diseases, but Dr. Broadbent has recently given the same metalloid with good effect in some cases, and Dr. Tilbury Fox has also recommended its use; but Dr. Eames considers that phosphorus is much more than a substitute for arsenic, as some previous writers have regarded it, and he alleges that it has been used with marked success in certain cases in which arsenic has failed. The mode of administration adopted by him was a solution of the phosphorus in oil, and the dose of the solution was from 5 to 10 minims three times a day after meals. Dr. Eames relates the particulars of several cases which were thus treated, and in all the results were successful. One was an instance of acne indurata of a most severe character, which had resisted all other local and general treatment; three of the cases were instances of lupus; and two, of scrofulo-derma. In psoriasis Dr. Eames found phosphorus very efficacious, even when arsenic had proved unserviceable, and he gives three cases in proof of this statement. A case of pemphigus was also cured by the use of phosphorus. Dr. Eames found that the drug produced a coated state of the tongue, and sometimes symptoms of dyspepsia, loss of appetite, mental depression and bodily weakness; but when these symptoms appeared the phosphorus was discontinued for a time, and mineral acids substituted. Most of the cases recorded by Dr. Eames had been treated by arsenic and other drugs before they came under his care, and he regards phosphorus as far superior in efficacy to that and other vaunted specifics in skin diseases.—*Dublin Journal of Medical Science*, January, 1872.

On Ozokerit as a Therapeutic Agent. By Dr. H. S. Purdon, of Belfast.—Dr. Purdon was induced to try ozokerit as a local application in cutaneous affections, in consequence of its resemblance in properties to petroleum, naphthaline, coal-tar, and other similar substances, which are found more or less useful in treating scabies and other skin eruptions. Ozokerit is a vegetable wax, being chemically a hydrocarbon, and is found in Moldavia, Wallachia, the Caucasus, and near the Caspian Sea. At the last-named locality it has long been used for illuminating purposes, and it has been lately introduced into England for the manufacture of candles. In the crude state it is of a dirty greenish colour, but when distilled and purified it is a hard, white, waxy substance, something like spermaceti. Dr. Purdon remarks that the crude ozokerit and the yellow oil are the best forms of this substance for medicinal purposes. The oil is obtained in the process of distillation, and it has a smell similar to that of paraffin, but not so unpleasant nor so strong; it also appears to be a slight deodoriser. The therapeutical action of ozokerit is similar to that of tar, but it is not so dirty. In private practice the refined material may be combined with glycerine, but at the hospital Dr. Purdon merely mixes the dark ozokerit with an equal quantity of linseed oil, and the compound thus formed, though rather lumpy, soon melts in the hand when it is well rubbed. The oil can be used in combination with lard. The action of the ozokerit and the oil appears to Dr. Purdon to be that of a stimulant to the diseased skin, and to be superior, for practical use, to tar, Hebra's tincture (a compound of tar, black soap, and methylated spirit), carbolic acid, or oil of cade. It is however only suitable for chronic affections, as eczema of long standing, psoriasis, tinea tonsurans, and scabies.—*Dublin Quarterly Journal of Medical Science*, November, 1871.

On Phenic (Carbolic) Acid and its Therapeutical Properties. By Dr. Ernest Labbee.—Dr. Labbée commences a long and elaborate paper on the properties and uses of carbolic acid by remarking the difference of opinion which has lately existed among medical writers as to the value of this agent, the greater number extolling its therapeutical virtues, while others depreciate it so far as to advise its exclusion from the list of the materia medica. Dr. Labbée steers between the two extremes, and while he regards the universal employment of carbolic acid as an illusion, he believes it to be a powerful agent, which will be appreciated when its physiological properties are more fully known. After alluding to its chemical characters, the writer refers to the fact that it is a violent poison to all living beings, and is especially remarkable for its toxic action on microscopic forms of life, both animal and vegetable; and hence, as is well known, it has been extensively employed, on theoretical grounds, in the treatment of accidents or injuries or diseases, the peculiarities or complications of which are supposed to be influenced by the presence of minute living organisms. Dr. Labbée treats at length of the experiments of modern observers, who are said to have traced living germs in the products of fermentation, and even in the miasms from the human body, but he neither denies nor confirms their statements, and in the same spirit he refers to the views entertained and published as to the connection between the phenomena of disease and the development of the morbific germs. Carbolic acid has also been strongly recommended as a disinfectant, its efficacy in this respect being supposed to be due to the power it has of destroying the life of the minute organisms which are assumed to be the causes of epidemic and infectious diseases; but in this respect, also, Dr. Labbée seems to think that the theory on which its use has been founded is not well established, while in practice its efficacy is still doubtful. The physiological effects of carbolic acid are local and general; when applied to the skin or the mucous membranes, it acts as a caustic; when given internally, the effects vary with the dose, but when this is large the acid acts as a violent poison. Among other effects which it produces, there is a notable diminution in the temperature of the body; and with respect to its elimination from the system, this process seems to take place from the lungs, and perhaps also from the skin. Dr. Labbée relates some experiments made by himself on frogs with carbolic acid, and the general results were that it appeared to act on the nervous system, causing convulsions, which were rather epileptiform than tetanic, and its special influence seemed to be upon the cerebellum and the medulla oblongata. In reference to the employment of carbolic acid in surgery and medicine, the author, while copiously quoting the experience of English and French authorities, expresses his own opinion that in surgery it is not superior in efficacy to other remedial agents, such as alcohol, glycerine, &c., and that it has no decided influence in preventing complications in the healing of wounds. At Sedan, where Dr. Labbée made use of the acid very extensively, he did not find that it prevented death from purulent infection after amputation, or that it neutralized the fetor of the wounds. In comminuted and compound fractures, however, he found that dressings of carbolic acid were very useful. In medical practice, Dr. Labbée passes in review the various applications of the acid in various forms of disease, and while he does not confirm the reports as to its efficacy in intermittents, typhus, or the eruptive fevers, he considers it very useful in many kinds of skin diseases; and from its slightly caustic and astringent properties, he thinks it may be useful when given in some forms of atonic dyspepsia. He also thinks that from its power of reducing the animal temperature it might be given advantageously in febrile conditions attended with excessive calorification. On the whole, Dr. Labbée arrives at the conclusion that, in spite of many illusions in regard to the acid and some exaggerated reports as to its utility, it is really an important article of the materia medica.—*Archives Générales de Médecine*, October, 1871.

On the Use of Oxygen in Diseases of the Lungs. By Dr. H. N. READ, of Long Island College Hospital. And *Sugli Effetti Terapeutici della Inalazione d'Ossigeno* (*On the Therapeutical Effects of the Inhalation of Oxygen*). By Prof. P. BURRESI, of Siena.—Dr. Read publishes a series of cases taken from observations extended over about a year, during which time oxygen gas was used in the Long-Island College Hospital in the treatment of pulmonary phthisis, acute and chronic pneumonia, and chronic bronchitis. He publishes only those cases in which the gas was used regularly and for a sufficient time to warrant him in drawing conclusions. The cases reported are twenty-one in number, and they comprise persons of different nationalities, and it is to be observed that other treatment was always adopted in addition to the inhalation of oxygen, cod-liver oil being given in the phthisical cases, and a tonic and supporting diet being always prescribed. The results appear generally to have been very satisfactory, the patients gaining in weight by the treatment, the rational symptoms diminishing or ceasing altogether, and the physical signs undergoing favorable change. In some cases, however, the results were unfavorable, but even in these the general health is said to have been improved, and the lives of the patients to have been probably prolonged. The advanced state of disease at the commencement of treatment and complication with other maladies were unfavorable elements in the instances of failure. Dr. Read submits his cases to the profession for the purpose of showing the action of oxygen gas, *in conjunction with cod-liver oil*, in diseases of the lungs. In the cases of acute pneumonia in which it was given it invariably had the effect of relieving the laboured respiration in the early stages, and of promoting resolution and absorption in the later.

Prof. Burresi relates a single case in which the inhalation of oxygen was employed in conjunction with other remedies. The patient was a young Italian woman, who was suffering from chronic leucocythæmia, chlorosis, hyperplasia of the spleen, insufficiency of the mitral valves, and hypertrophy of the heart. The treatment recommended consisted in a meat diet with wine; the administration of carbonate of iron and manganese; the inhalation of pure oxygen, in the daily dose of 20 litres at first, and then of 34 litres, which were consumed in fifteen or twenty minutes every morning. The duration of the treatment was from the 18th of April to the 16th of June, and at the latter period there was not much change in the general condition of the patient, except that she had gained some appetite, slept more quietly and without oppression of the breathing, felt a little stronger, and had a little more colour. The case does not prove the therapeutical efficacy of the inhalation of oxygen, nor does Prof. Burresi adduce it for that purpose, but he has carefully recorded the immediate effects produced by the gas, and also the state of the respiration, circulation, and calorification, as observed day by day during the treatment. The immediate effects appear to have been some increase in the frequency of the pulse and of the respiration, and some augmentation (though this was not always observed) of the temperature; but Dr. Burresi did not find any *immediate* change in the quality of the pulse, as shown by the sphygmograph. As for the more remote effects of the inhalation it is difficult to estimate them, because the treatment by meat diet, and by the iron and manganese, must have had great influence on the results.—*New York Medical Journal*, October, 1871; and *Rivista Scientifica Siena*, 1871.

On the Treatment of Diabetes by Lactic Acid (*Cantani's Method*). By Dr. G. W. Balfour, of Edinburgh.—The pathology of diabetes, although still obscure, is now pretty generally admitted to consist in some perversion of the glycogenic function in the body, and the treatment has hitherto been mainly directed to the object of depriving the organism of the pabulum from which the glucose is mainly derived, and such remedies are employed as may alter the nervous energy of the organs at fault. Cantani, however, considers that in diabetes there is not merely an increased production of sugar, but an imperfect combustion of that principle, and he believes that this imperfect combustion depends on the production of a morbid form of glucose, which he calls *paraglucose*, a substance incapable of being transformed into lactic acid, and which therefore cannot be burned, and consequently passes unchanged into the urine. As the sugar and starch of the food cannot now be burned, the heat of the body is maintained at the expense of the albumen and fats, the combustion of the former leading to excess of urea which adds to the density of the urine in diabetes. The albumen and fats received as food being now insufficient for the requirements of the body, the tissues are employed also for that purpose, and hence the rapid emaciation in the disease. In the early stage of diabetes the quantity of sugar in the urine varies with the diet, but in the latter stages Cantani believes that the inosite of the muscles, and even the gelatinous tissues, are converted into paraglucose. He considers the liver to be the chief seat of disease, but whether this be the case or not, he proposes to give as complete rest as possible to this organ by withdrawing sugar-producing substances and subjecting the patient to a rigorous meat diet. But as this is only a temporary expedient, inasmuch as meat itself may ultimately be converted into paraglucose, he further proposes to prevent waste by supplying a combustible agent in a quantity sufficient for the wants of the body, so that the albuminates and fats may be spared, Now, in the conversion of glucose into carbonic acid, lactic acid appears to be the intermediate product, and lactic acid is the combustible agent which Cantani recommends. The quantity of lactic acid administered by this physician is from 77 to 154 grains daily, diluted in from 8 to 10 ounces of water, and his meat diet is exclusively one of plain meat, roast or boiled, without any sauces of milk or eggs, and without any bread, flour, or any vegetable matter whatever, the only seasoning permitted being salt, oil, and a little vinegar. The drink allowed is water, either plain or with a little of the purest alcohol ; coffee, tea, and wine being prohibited. The results are said to be surprisingly successful, and Dr. Balfour remarks that the latest treatment adopted in Great Britain, namely, that by skim-milk, bears out Cantani's views so far as it is a strictly animal diet, free from amylaceous matter, and containing three to six per cent. of lactin, which, under the influence of the caseous matter, became transformed into lactic acid. Dr. Balfour has tried both these systems, but he finds that of Cantani the more successful. He relates the details of seven cases in most of which the treatment was attended with favorable results, and he invites a further trial of Cantani's plan at the hands of the profession. The lactic acid employed by Dr. Balfour is fluid, not syrupy, of the sp. gr. 1·027, and with the ordinary musty smell of sour milk, and three to four drachms a day appear to him sufficient for all practical purposes. Koumiss would probably be a useful article of diet for diabetic patients.—*Edinburgh Medical Journal,* December, 1871.

On the Use of Carbonate of Lithia in Gout and Uric Acid Gravel. By Prof. DITTERICH, of Munich.—Carbonate of lithia, according to Prof. Ditterich, must always be considered as the most powerful remedy in gout and the morbid conditions depending upon excess of uric acid, and the salt has fallen somewhat into disrepute of late only in consequence of being unsuitably administered. The doses of five to ten grains, recommended by Aschenbrenner, generally produce very unpleasant symptoms, as dyspepsia, catarrh of the stomach and bowels, with vomiting, &c., which require the discontinuance of the remedy. These doses, according to Prof. Ditterich, are much too large to act beneficially, and the single dose should never exceed twelve centigrammes (a centigramme is the hundredth part of a gramme, which is about 15 grains) and in twenty-four hours not more than a gramme should be given altogether. The next question to be asked in the treatment of gout, is whether the case presents itself in the acute or chronic form, for in the former lithia is unsuitable, but in the latter the carbonate may be given in the proportion of half-a-gramme (about 7½ grains) in a hundred and fifty grammes of distilled water, one or two teaspoonfuls to be taken every two hours. Thus administered, the lithia causes no inconvenience and generally affords relief in from eight to fifteen days, during which the painful parts are covered with socks or linen coverings. According to Prof. Ditterich, the gouty swellings which have become hardened are not affected by the lithia circulating in the blood, until the adjacent parts of the limb have been brought into a state of congestion by stimulating embrocations.—*Schmidt's Jahrbücher der Gesammten Medicin*, October, 1871.

On the Use of Cold Water as an Oxytocic. By Dr. GARVIN, of Kentucky.—The efficacy of cold water in exciting contraction of the uterus in post-partum hæmorrhage has induced Dr. Garvin to employ the same agent in promoting the action of the organ in cases of tedious labour. He gives the history of four cases, selected from a number of others successfully treated by this agent. In all the cases, the uterus was inactive, and presented the conditions in which ergot of rye or the use of the forceps is usually recommended, but they all did well by the application of iced water to the abdomen. Dr. Garvin compares the value of cold water with that of ergot; and, while thinking that the latter drug has been overestimated, he believes that only experience is needed to prove the efficacy of cold water, by the use of which, moreover, none of the dangers are encountered which often attend the use of ergot. Dr. Garvin argues that cold, when applied to the surface of the body, though locally depressing, acts as a stimulant to the whole system, as is shown by its effect in restoration from syncope, and in its stimulating influence on the brain in cases of narcotic poisoning. It does not exert a toxical power on the nervous system as ergot does, but it merely awakens and revives the dormant or flagging energies, and re-establishes a normal condition. The only objection which can be urged against the use of cold water as an oxytocic is the liability of the patients to take cold under its use, but this inconvenience may be obviated by a few simple precautions. When applied to the surface in the manner recommended, cold water excites the uterus to contraction by reflex action.—*American Journal of Medical Sciences*, October, 1871.

On some New Properties of Quinine, as employed in Obstetric and Ophthalmic Practice.—*L'Union Médicale* remarks that, in addition to the well known anteperiodic action of quinine, various other properties have been assigned to this alkaloid, namely, stimulant, disinfecting, parasiticide, &c.; but, in addition, Dr. Monteverdi, of Cremona, has lately declared it to be superior even to ergot of rye as a special excitor of the contractions of the uterus, being more rapid in its action, and also being harmless both to mother and child. In a medium dose of 1 gramme (about 15 grains), taken in three or four doses in two hours, the drug will produce contractions in cases of atony and inertia of the uterus, whether in cases of labour or uterine hæmorrhage. Dr. Monteverdi was induced to suspect this effect of quinine from the aversion manifested by the pregnant women of Cremona to take the alkaloid from the fear of abortion; and he says that he has verified this action by a series of facts proving the power of quinine to accelerate labour and to promote the expulsion of the placenta. But another application has been made of quinine in ophthalmic practice, especially in affections of the cornea and conjunctiva. Dr. Gotti, of Bologna, has employed a quinine wash, composed of 25 centigrammes of hydrochlorate of quinine to 30 grammes of water, in a great number of private and hospital cases affected with maladies of the cornea and conjunctiva, and he has found the treatment very successful. The wash is especially applicable to inflammation of the cornea and conjunctiva; but Mr. Prout, of Brooklyn, has employed insufflation of dry quinine in palpebral granulations. Two cases are recorded where this treatment was attended with partial success. In other affections of the eye, however, the results of the treatment have not been equally satisfactory.—*L'Union Médicale*, September, 1871.

On the Treatment of Pneumonia by the Acetate of Lead. By Dr. Strohl, of Strasburg.—Dr. Strohl strongly recommends the use of acetate of lead in pneumonia; and, discussing the whole subject of the treatment of that disease, he comes to the conclusion that lead is the safest and best remedial agent. He compares the results obtained by different modes of treatment, such as the expectant plan, bleeding, emetics, mixed treatment of bleeding and tartar emetic, &c.; and he claims for his own plan the most satisfactory results, for the ratio of mortality in the cases he treated was only 1 in 9·66; but, even if the mortality were the same, he claims for lead certain advantages which it possesses over other remedies, and especially its innocuity and the rapidity of its curative action. Tartar emetic causes vomiting, gastro-intestinal irritation, and prostration, but acetate of lead does nothing of the kind, and Dr. Strohl found that it did not even cause a threatening of colic. Again, the objections to other modes of treatment are rather numerous; bleeding is inapplicable in the extreme of age and in bad constitutions; tartar emetic is contra-indicated in cases accompanied by inflammatory or other serious affections of the stomach and intestines, and in diarrhæa and phthisis; but in all these cases lead may be given. Dr. Strohl sums up his views as to the efficacy of acetate of lead in the treatment of pneumonia to the following effect: It is preferable to tartar emetic, digitalis, and veratria, because its action is more certain, more rapid, and more free from inconvenience, and it has never caused any bad symptoms. It never constipates, but sometimes causes diarrhœa, and it may be given to persons of all ages. Under its use the pulse is lowered, and the heat and the fever are reduced, and convalescence is very rapid when there are no persistent complications.—*L'Union Médicale*, October 12, 1871.

On the Physiological and Therapeutical Effects of the Chlorides of Potassium and Magnesium. By Dr. Rabuteau.—Dr. Rabuteau, in a series of experiments made upon himself, found that the chloride of potassium produced two distinct effects, namely, as a chloride it increased oxidation, and as a potassium salt it lowered the pulse. The proof of its oxidising action was that the excretion of urea was notably increased. The salt has been hitherto very little used in medicine, but it is a purgative, and has been sometimes employed externally in a concentrated solution or in powder in cancerous affections. It has been also prescribed instead of the bromide of potassium, in epilepsy, and has been said to be in some respects superior to the latter salt. On theoretical grounds, the chloride of potassium might, perhaps, be substituted for digitalis in some cases. The chloride of magnesium, as is well known, is contained in sea-water. Experiments made upon the lower animals showed that when injected into the veins it caused constipation, but when introduced into the alimentary canal it acted as a purgative, and when employed as a medicine it was found that its aperient properties were well marked. Dr. Rabuteau considers it to be as efficacious in this respect as the sulphate of magnesia, and in some of its properties to be preferable; for it is not so disagreeable in its taste, it produces no griping, and no consecutive constipation, and its purgative effects are secured by a comparatively small dose; nevertheless, Dr. Rabuteau does not specially recommend the chloride of magnesium as a purgative, for he objects to all magnesian purgatives; but he prefers it to the sulphate. The chloride is deliquescent, and, if employed, the best form is in solution; and Dr. Rabuteau gives formulæ for what he calls *magnesian waters*, to be employed like Seidlitz water.—*L'Union Médicale*, September 14 and 30, 1871.

On the Therapeutical Use of the Lacto-Phosphate of Lime. By Professor B. W. McCready.—There is reason to believe that phosphate of lime, besides entering into the composition of bone, has some influence in cell formation, and according to Lehmann it is found in appreciable quantity wherever cells or fibres are formed. The phosphate has been recommended in cases of rickets, and the experiments of Milne-Edwards seem to show that, under its use, fractured bones in dogs and rabbits produce more abundant callus. But in a recent series of articles, published in the *Archives Générales de Médecine*, Dr. L. Dusart examines the whole subject, and attributing the somewhat unsatisfactory results hitherto obtained to the great insolubility of the ordinary phosphate, he recommends the use of a new preparation, which he calls the lacto-phosphate of lime, in which the lime-salt is dissolved in free lactic acid. Dr. Dusart made experiments both on the lower animals and on man, and he found that the union of bone, in cases of fracture, was promoted by the use of this preparation, which was also useful in rickets, and some other diseases, as diarrhœa and indigestion, In the United States, at Dr. McCready's request, a syrup of the lacto-phosphate of lime was prepared by the pharmaceutists, and he found the drug useful in cases of defective nutrition, especially in the cases of prematurely weaned children, in rachitis, and in atonic dyspepsia. Dr. W. A. Hammond found it of very great value in cases of nervous derangement. In forming the syrup of the lacto-phosphate, bone-earth is dissolved in hydrochloric acid, then precipitated by ammonia, and the recent precipitate is treated with concentrated lactic acid; the clear solution is then mixed with syrup, and flavoured with orange-flower water.—*New York Medical Journal*, June, 1871.

On the Use of Chlorine Water in the Treatment of Diphtheria. By Mr. W. G. Balfour, of Montrose.—Chlorine water, which is prepared by adding hydrochloric acid to chlorate of potash and adding water, has been successfully employed in the treatment of scarlet fever, its efficacy having been supposed to depend upon its disinfecting properties; but Dr. Matthew Gairdner, of Crieff, introduced its use in diphtheria, and Mr. Balfour, while acting as his assistant, became aware of its remedial powers in that disease. He adduces several cases in support of his recommendation of chlorine water, and particularly mentions the case of a family living near Crieff, where four of the children suffered from diphtheria, and were all treated with chlorine water, stimulants, and milk; three recovered, and one died; and the unsuccessful result in this last instance is attributed by Mr. Balfour to the neglect of the mother in not giving the remedy. In another case, which was that of a child three years of age, the symptoms of diphtheria were well marked, and a little ipecacuanha wine was given with temporary relief, but as the symptoms soon returned, the chlorine water was administered in two-drachm doses every two hours. After several vicissitudes, the child had a violent paroxysm of coughing, followed by expectoration of something, which was probably the false membrane, but which was swallowed, and immediately afterwards the child was relieved, and ultimately recovered. Mr. Balfour thinks that the remedial action of chlorine in diphtheria and scarlet fever is more general than local, and that, when taken internally, it is absorbed into the blood, and there neutralises the morbid poison; but, whatever the theory may be, Mr. Balfour has found the treatment very successful in practice. —*Edinburgh Medical Journal*, December, 1871.

On the Physiological and Therapeutical Effects of Hemlock and its Alkaloid. By MM. Martin-Damourette and Pelvet, of Paris.—The *Gazette Hebdomadaire* gives a *résumé* of some recent researches on the physiological and therapeutical properties of hemlock and its alkaloid. Four kinds of hemlock are known, but they belong to four different genera; they are, the common hemlock (*conium maculatum*), another kind of hemlock (*cicuta virosa*), the water hemlock (*phellandrium aquaticum*), and the fool's parsley (*æthusa cynapium*). These plants are all dangerous, and contain an alkaloid, called by Brandes, *conia*, but afterwards named by Giesecke, *cicutia;* and this principle has been carefully studied by different chemists, whose researches, however, have been chiefly directed to the properties of the *conium maculatum*. The work in which MM. Martin-Damourette and Pelvet have recorded their experiments is divided into three parts, which comprise respectively the examination of the physiological properties of *cicutism* in the lower animals, the effects of cicutia on the different systems and organs of the body, and a synthesis of the physiology and therapeutics of cicutia. The general effects of hemlock are—1. Excitement, and even convulsions, if a sufficient quantity of the poison is introduced suddenly into the blood. 2. Paralysis of the voluntary movements at first, and then involuntary movements and diminution of sensibility. 3. Return of convulsions when the *cicutism* is disappearing, in cases where the dose is not fatal. When death occurs, it takes place from arrest of respiration, with or without ultimate convulsive movements. The symptoms exhibited by Socrates, as described by Plato, present, according to MM. Martin-Damourette and Pelvet, the characteristic features of poisoning by hemlock. According to Plato's description, Socrates was directed by his gaoler to walk about, after taking the poison, till he felt a heaviness in his legs; the philosopher did so, and when he felt his legs heavy he lay on his back. The person who had given him the poison, after a certain time pressed his feet, and asked if he felt any pain, and he said that he did not; then the upper parts of the body were successively shown to be cold and stiff, and eventually Socrates spoke a few words, then had a convulsion and remained with his eyes fixed, and then died. Thus, it appears that the first symptom was the weakness of the lower limbs, afterwards coldness and insensibility, extending from the periphery to the centre, and lastly there was a terminal convulsion. It is therefore argued that the doubts entertained by some authors, as to the similitude existing between the Κωνειον of the Athenians and the *conium* of the present day are unfounded, or at any rate may be explained away. In reference to the therapeutical uses of conium, it is unnecessary to refer to the empirical eulogies of this plant as a cure for several organic diseases; and it must be admitted that its efficacy as a remedy is not very great. It has been found experimentally that conium, without much increasing the excitability of the spinal cord, paralyses the extremities of the motor nerves, and hence it might be serviceable in tetanus. Asthma and spasmodic cough and hooping-cough have been relieved by this drug, and neuralgic pains have been caused to disappear by injections of a weak solution of cicutia.—*Gazette Hebdomadaire*, October, 1871.

On the Use of Ergot of Rye in Dysentery. By M. Luton, ot Rheims.—During an epidemic of dysentery which lately prevailed at Rheims, M. Luton employed in the cure of the disease most of the remedies which are considered efficacious, and met with various degrees of success. The epidemic was not a very severe one, and most of the patients recovered; but, in the majority of the cases, it appeared to M. Luton that the therapeutical action was not very evident nor the relief rapid; and moreover some of the patients, especially among the most aged, died. It was therefore desirable that some new method of treatment should be found, which might give mory satisfactory results, and the opportunity of doing so was offered by the case of a female patient, who was suffering at the same time from uterine hæmorrhage and dysentery. The ergot of rye was prescribed successfully for the former malady, and it was found that the latter was likewise benefited by the remedy; and, in fact, as soon as the first doses had been given, a condition of constipation was induced, which lasted for four or five days. This first experiment led to the use of the ergot in simple dysentery, and it was found that an improvement in the symptoms, and eventually a complete cure, followed the new plan of treatment. M. Luton gave the ergot in powder, in the dose of 3 grammes (about 45 grains) a day, divided into doses of 50 centigrammes (about 7½ grains); and two or three days generally sufficed for a cure in ordinary cases. The ergot appeared to attack not only the hæmorrhagic element of dysentery, but the whole disease; and the mucous secretions, the griping, colic, and fever, were equally relieved at the very commencement of the treatment.—*Gazette Hebdomadaire*, October, 1871.

On the Therapeutic Value of the Nitrite of Amyl. By Dr. H. C. Wood, jun., of Philadelphia.—Dr. Wood has arrived at the conclusion that the nitrite of amyl, in its action on the lower animals, is an almost universal sedative on the nervous system, while on the circulation its uniform action is to lessen arterial blood-pressure; and he thinks that it also catalytically arrests oxidation. Dr. Wood's views, however, are not in accordance with those of some other writers, who believe that the nitrite acts on man as a powerful stimulant. It is true that the immediate effects of this agent on man are fulness of the head, flushing of the face, and violent action of the heart, but these symptoms are due to the dilatation of the capillaries, and are associated with lessened arterial pressure. In answer to the question as to the practical value of the nitrite of amyl, Dr. Wood suggests, that as it checks oxidation and lowers temperature, it may possibly be of use in some fevers, but he has no clinical evidence to adduce in support of this view. In tetanus it ought to be theoretically of great value, because in this disease there is a condition of exalted functional activity of the reflex motor centres, and of these centres the nitrite is a powerful depressant, and there is some evidence that it has been serviceable in this affection. In angina pectoris the nitrite is of very great value in affording rapid and permanent relief, and not only in true angina pectoris, but also in those cases where there is well-marked valvular disease of the heart. Dr. Wood has had an opportunity of using the nitrite in a case of valvular disease attended with severe suffering, and its effect in relieving the pain, after the failure of other remedies, was astonishing. With regard to the mode of administration, it has always hitherto been given by inhalation, and its insoluble and highly volatile nature renders it unfit for exhibition either in solution or mixture, but Dr. Wood thinks that it might be given when dropped upon a piece of sugar; when it is to be inhaled, five drops should be placed upon a handkerchief and held close to the nostrils, the pulse being closely watched and taken as a guide as to the continuance or withdrawal of the drug.—*American Journal of the Medical Sciences*, October, 1871.

REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

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On Degenerations occurring in Acute Maladies.—M. A. Laveran observes that the question of the degeneration of certain anatomical elements (muscular fibres, small blood-vessels, &c.), in acute maladies has hitherto been only studied from an histological point of view. He proposes to examine the influence exercised by these degenerations on the progress and termination of acute diseases. He thinks that Zenker's distinction between the granular and waxy forms of degeneration of muscle in typhoid fever is untenable. The granular form is the first degree of the waxy degeneration, *i. e.* the vitreous degeneration of O. Weber, the granulo-vitreous of M. G. Hayem. He describes three degrees in the alteration of muscle. In the first the muscles are red, hard, brittle, the fibres swollen, their contents granular, at points the striæ have disappeared, and here and there they present incomplete fractures. In the second degree, the muscles are pale, dry, and friable, the fibres are unequal, expanded at certain points, narrowed at others. In the expanded parts are transparent masses (the *masses vitreuses* of O. Weber) or more frequently they are transparent at some points, granular at others. Between the swellings the fibres are granular and present numerous fractures. In the third degree the muscle is quite pale, of a yellowish dead-leaf or waxy colour. It is more friable than in the second degree. Its histological structure is completely modified. The greater number of fibres enclose granulo-vitreous masses in juxtaposition; some are completely empty and reduced to the mere enveloping sheath. The nuclei of the fibres and the cells of the perimysium are in course of proliferation. Alteration of muscles is met with in a great number of acute diseases. Hayem has noted it in 22 out of 24 cases of variola, once in scarlatina, twice out of 3 cases of measles, in 3 cases of acute miliary tuberculosis, in 2 cases of grave jaundice, once in erysipelas with meningitis, once in 2 cases of tubercular meningitis, once in puerperal fever with metastatic abscesses, once in phlegmonous inflammation of the parotid. Hoffmann has met waxy degeneration of the muscles of the thoracic and abdominal walls frequently in pneumonia. The author has observed alteration of muscles in 19 out of 21 cases of typhoid fever, in 6 out of 10 of variola, in 3 cases of scarlatina, and in two out of 3 of acute tubercolosis. The change is most marked in typhoid; in variola and scarlatina death supervenes more rapidly, and the degeneration less frequently has advanced beyond the first degree. Degeneration of the muscles is not the cause of the great weakness which accompanies typhoid at its onset, for the time has not been sufficient to produce it; but muscular lesion is probably the cause of the great weakness which accompanies the long convalescence from typhoid. Alteration of the respiratory muscles, although by itself it may not cause death, yet favours a fatal termination by embarassing respiration, hindering expectoration, and favouring hypostasis. Zenker considered alteration of the fibres of the heart rare in typhoid. Hayem has met it more frequently in patients who died suddenly in the course of typhoid, and in whom there was no other lesion to explain death. In four out of fourteen cases of typhoid the author has found the heart in course of granular degeneration. This condition may predispose to death by embarassing the pulmonary circulation and favouring bronchitis and pulmonary œdema, and it may predispose to sudden death. The author compares sudden death during the convalescence of typhoid to the sudden death which sometimes overtakes anæmic and leucocythæmic patients. The author, with Hoffmann and Hayem, has observed granular degeneration of small blood-vessels in a considerable number of cases of typhoid, of variola, and of scarlatina. Not only are the vessels of the heart, but those of the skin and kidneys attacked The alteration of the vessels is a granular degeneration. The blood-vessels become granular, break easily, and give rise to hæmorrhages—hence subcutaneous and submucous ecchymoses.—*Archives Gén de Médecine*, Juillet, Août, 1871.

Leucocythæmia accompanied by Alteration of the Marrow of the Bones.—E. Neumann relates the following case:—The patient, æt. 30, presented during life all the symptoms of splenic leucocythæmia; he died from repeated hæmorrhages. After death there was found considerable hypertrophy of the spleen and liver, and the usual alterations of the blood. The parenchyma of the spleen and liver presented all the characters of leukæmic hyperplasia; the liver also enclosed a whitish nucleus of some size, in which a collection of lymphatic cells had taken the place of the hepatic cells. The marrow of the bones presented very curious lesions, especially observed in the ribs, the sternum, a vertebra, the diaphysis of the humerus, and the diploe of the cranium. At all these points the marrow presented a uniform greenish yellow colour, traversed by a few small red veins; it had a ropy consistence and resembled creamy pus. The microscope revealed white corpuscles of the blood, of varied form and in countless numbers—perhaps, in course of transformation into red corpuscles. These elements were enclosed in a filamentous substance, containing mucine and very slightly vascular. The blood-vessels were represented by some isolated arterioles. The walls of the larger of these vessels were infiltrated with lymphatic corpuscles; those of the little arterioles were formed by slender fusiform cells, elongated as in the spleen. These vessels were almost exclusively filled with red globules. The author proposes that a myelogenic leucocythæmia should be admitted in addition to a splenic and lymphatic. The capillary network being wanting the blood is brought by the arteries and thrown directly into the pulp of the marrow, which is rich in cells, and it is probably returned into the venous canals mixed with the elements of this tissue. The presence in the blood of red globules incompletely developed, only met in the marrow of the bones in health, is also explained.—*Arch. der Heilkunde*, xi, 1,—15; *Archives Gén. de Méd.*, Fevrier, 1872.

The Chemistry of the Urine and Blood in Leukæmia.—Salkowski has collected seven observations of splenic leukæmia in which an increased proportion of uric acid was remarked. He analysed the urine of a patient suffering from splenic leukæmia for thirty consecutive days. The mean proportion of uric acid to urea was 1 : 16·3. There was a constant augmentation in the proportion of uric acid. With regard to the existence of hypoxanthine in the urine of patients attacked with splenic leukæmia, the author is unable to confirm the observations of Mosler. He has analysed large quantities of the urine without finding it, and in any case the occurrence of hypoxanthine in the urine is not diagnostic. Lactic acid, formic acid, acetic and oxalic acids, products of incomplete oxidation may be found. The three first of these exist in healthy splenic pulp, it is therefore not difficult to suppose that they may occur in the urine of patients attacked with hypertrophy of the spleen. The author has never found a trace of allantoine. In the blood after death he has found a substance presenting reactions analogous to those of glutine; this body, however, treated by sulphuric acid only furnished doubtful traces of leucine, and not of glycocol. Besides, he found in the blood hypoxanthine, formic, acetic, and lactic acids, and an organic acid containing phosphorus. Of these the principal abnormal constituents were the glutine and hypoxanthine. Hoppe-Seyler has shown that formic acid is found at the moment of coagulation of hæmoglobine. Glutine has been found in some cases of leukæmia, but not in all. Light may be thrown on its occurrence by the observation of Neumann quoted above on the lesions of the marrow of bones, in certain cases of leukæmia.—*Virchow's Archiv,* 1870; and *Archives Gén. de Médecine*, Feb., 1872.

On the Relations of Leucocythæmia and Pseudoleukæmia.—Dr. Horace C. Wood, jun., in a valuable paper on this subject, defines pseudoleukæmia to be a disease closely simulating lympathic leucocythæmia, so as to be indistinguishable from it, save only by the examination of the blood, which contains no excess of white blood-corpuscles. There is the same indolent enlargement of the glands, the same tendency to diarrhœa and hæmorrhages, the same apparently causeless yet ever deepening anæmia, the same remorseless march towards death; and post-mortem examinations reveal no differences in the anatomical lesions of the glands. The formation of masses of lymphatic tissue in the various viscera, which is frequently seen in leucocythæmia, however, is rarely met with in pseudoleukæmia, perhaps because it is not looked for. Pseudoleukæmia is the disease known under the names "adénie," Hodgkin's disease, &c. In true leukæmia, the spleen generally finally becomes involved; so, in all the described cases of adénie, enlargement of the spleen has finally occurred, except where death has taken place early, from pressure of enlarged glands on trachea. Dr. Wilks has described cases of Hodgkin's disease, in which the enlargement of the spleen was a prominent feature. The author believes there is a splenic variety of pseudoleukæmia, and relates a case of splenic enlargement and disease with intense anæmia, but no increase of white corpuscles, as an example. He then enters on the question of the relation of hyperplasia of the leucocytes of the medulla of bones to leucocythæmia, and adds a carefully observed case to the literature on the subject. From this we extract the description of the appearances in the bones. The patient was a German, and his blood exhibited a marked increase in the number of white corpuscles, with enlarged spleen:—"*Lumbar vertebra*, on section, bright carmine red. Microscopical constituents of this juice as follows—1. Irregular, granular, distinctly nucleated cells, the largest having a diameter of $\frac{1}{2000}$ of an inch. 2. Similar cells or corpuscles, often not nucleated, and less distinctly granular. 3. Cells, granular externally, clear in the central portions. 4. Cells distinctly nucleated, nucleus surrounded by a clear hyaline portion; these cells are irregular, or nearly globular in shape, and between $\frac{3}{12000}$ and $\frac{4}{12000}$ inch in diameter. 5. Irregular, granular, not distinctly nucleated cells. 6. Cells, very abundant, exactly resembling lymph-corpuscles, varying from $\frac{4}{12000}$ to $\frac{5}{12000}$ in diameter. 7. A very few red blood-corpuscles. *Right femur*, sawn open longitudinally; bone, as in other long bones, remarkably dense and thick. Marrow somewhat pulverulent; lower $\frac{1}{6}$ bright carmine red, mottled with yellowish, shading into the next $\frac{1}{3}$, which is of an intense, very dark, almost blackish carmine; this shades into the bright Indian red of the next $\frac{1}{3}$, which above shades into bright carmine, gradually giving place to the yellowish trabeculæ above. *Left femur* very similar to the right, save that the deep carmine portion is shorter, and the Indian red correspondingly longer, and that the marrow of the lower $\frac{1}{6}$ has a much more transparent gelatinous look. *Microscopical examination.*—Medulla containing very little oil, and very few red blood-corpuscles, made up of an immense number of cells, which are most irregularly globose, sometimes larger, and altogether irregular in form, minutely granular, mostly distinctly nucleated, very rarely binucleated; nucleus with a distinct nucleolus; size of globular cells $\frac{7}{12000}$ to $\frac{4}{12000}$ inch. Besides these, there are some smaller globular cells entirely free from granules, and perfectly transparent, but furnished with a distinctly granular nucleus." The medulla of the left tibia was, in part, nearly natural in appearance; microscopically, it was largely made up of normal fat-cells and free fat, but with a good many cells similar to those seen in femora. The author gives two cases of pseudoleukæmia, in which the marrow of the bones exhibited strikingly similar lesions. He concludes—1. Clinically, the so-called true and false leukæmia are the same, save only in the matter of the white blood-corpuscles. 2. All varieties of leukæmia are represented in pseudoleukæmia. 3. Hyperplasia of the marrow of the long bones is a more or less characteristic lesion of leucocythæmia; and this lesion, and all the other lesions of the solid tissues known as characteristics of leucocythæmia, are equally characteristic of pseudoleukæmia. The author asks, is it well to consider these two dyscrasia distinct? He thinks not—1. Because the increase of white corpuscles is not peculiar to leucocythæmia, but occurs in very different diseases, such as lymphomia and pyæmia, and as the result of malarial poisoning. 2. That in these diseases, the increase of white corpuscles is not constant. 3. The amount of increase varies indefinitely in leucocythæmia itself. 4. There are cases which at one part of their course represent pseudoleukæmia, at another leukæmia.—*American Journal of Medical Sciences*, October, 1871.

On the Clinical Significance of the Presence of Leucin aad Tyrosin.—Dr. James Tyson, in a paper on the chemical characteristics and physiological and pathological relations of leucin and tyrosin, after noticing the various solids and fluids of the body in which leucin is a normal or pathological constituent, observes that it is in certain affections of the liver, attended by impaired function of this organ, as in acute yellow atrophy or chronic softening, that the presence of leucin assumes a clinical significance, being found under these circumstances in the blood and secretions, particularly in the urine, as well as in the substance of the liver, kidney, and spleen. Its abundant presence in the liver, under these circumstances, leads us to suppose that this organ is the seat of its destruction, rather than its formation, as some suppose; and the fact that it only, or chiefly, appears when the function of the liver is deficiently carried out, makes this supposition reasonable; while its elimination at such times by the kidney, analogous to, and coincident with, the supplemental action of this organ in separating the constituents of bile, affords confirmation of the same view. The author has found leucin abundantly in the urine of a case of atrophic disease of the liver, which continued almost a year before it terminated fatally. Beale has found it in the urine in cases of chronic wasting of the liver with jaundice: but Städeler has found it in the urine in typhus and smallpox, where deficient action of the liver is not characteristic, although the action of that organ may have been deranged. Coincident with the presence of leucin and tyrosin in the urine is a great diminution in the urea. With regard to the presence of tyrosin in hepatic affections, the author quotes the observations of Neubauer and Frerichs, as to its abundant presence with leucin in the urine of acute yellow atrophy of the liver. Hoppe-Seyler, however, says that it is only in certain cases of softening of the liver that it, with leucin, is abundantly present in the urine; and that, in the ordinary cases of so-called yellow atrophy, neither tyrosin nor leucin is met in the urine. Hoppe-Seyler denies that it is found in the urine of severe typhus and variola. The author states that in two cases of destructive disease of the liver which have been under his observation, one of which, at least, was a case of true acute yellow atrophy, and both of which afforded ample opportunity for the study of leucin, he was quite unable to find any evidence of the presence of tyrosin. His experience has been confirmed by that of two other observers. He concludes that leucin and tyrosin have only a marked significance when present in the urine, in connection with symptoms of deranged hepatic function. They are, then, of grave import, as indicating destructive diseases of the liver, which have, as far as is known, always terminated fatally. The mode of death, with coma or convulsions, together with the deficiency of urea in the urine containing leucin and tyrosin, points to a condition analogous to, or identical with, uræmic poisoning.—*American Journal of Medical Sciences*, January, 1872.

Convection of Scarlatina.—M. Guerard, in August last year, brought before the Société Médicale des Hôpitaux the following remarkable instance of the transmission of scarlatina. A young governess during a holiday, in the department of the Loire, was attacked with scarlatina. During her convalescence she wrote to a pupil and told her that she was skinning so abundantly that the paper on which she was writing was covered with skin. Five days after receiving her letter, the pupil was seized with scarlatina. Her mother, who nursed her, also took it and died. There was no scarlatina in the neighbourhood where the pupil and her mother were residing.—*L'Union Médicale*, Janvier 23, 1872.

On Delirium Tremens.—Prof. Laycock combats the opinions defended by Prof. Cuming, of Belfast, that a distinct connection exists between the withdrawal of alcohol, in the case of habitual drunkards, and the supervention of delirium tremens. He adheres to the conclusion which he had previously arrived at, "That the withdrawal of alcohol, or, more accurately, of the usual stimulants, has comparatively little influence as a cause of delirium tremens; not so much influence as the want of food." The observation of about 60 methystic cases has confirmed him in this conclusion. From an abstract of twenty-four of the methystic cases, received into the Edinburgh Infirmary last summer, he finds they may be classed under three heads, viz.:—As being drunk on admission; as in the "horrors," or first stage of the delirium; and, as in actual delirium tremens. In every one of these 24 cases there was a withdrawal of alcoholic stimulants, to the extent which Dr. Cuming has found to induce the disease; but none of the drunken cases became horrored or delirious; none of those with "the horrors" advanced to the next stage; and, all in the delirious condition rapidly recovered. Dr. Laycock states that during the three years from 1st October, 1845, to 30th December, 1848, the old procedure of giving stimulants, with or without opium, was followed in the "D. T. Wards" of the Edinburgh Infirmary, and the deaths, in delirium tremens, were at the rate of 35 per cent.; during the eleven years ending 30th September, 1850, the deaths were 26·0 per cent. Since Oct., 1858, the withdrawal system has come into general use in the Edinburgh Infirmary, and in the ten years, 1859 to 1869, the mortality, in cases returned as delirium tremens, has only been 3·89 per cent. On the other hand, in St. George's Hospital, London, from 1850—

1855, the mortality was 14·6 per cent. In the army, in 1853, the mortality from delirium tremens was, for the infantry, 17·6 per cent.; for the cavalry, 13·0 per cent.—*Dublin Quarterly Journal*, Nov., 1871.

Paralysis of the Fifth Cerebral Nerve, and its Effects.—Dr. H. D. Noyes has collected and reported several cases of paralysis of the fifth nerve, which bear on the question of the influence of the fifth nerve in the nutrition of the cornea. He concludes that destructive effects take place in the cornea when a part only of the trifacial is paralysed, probably, if the ophthalmic branch alone be impaired. These effects occur when the paralysis proceeds from a cause existing in the peduncle of the brain, or in the nerve behind the ganglion of Gasser,—this organ not being injured; furthermore, when the ganglion is seriously damaged, and even when it is simply irritated without being destroyed, the cornea is liable to suppuration. Exposure of the cornea to irritating, or depressing causes, favors its destruction, but the essential reason is ascribed to implication of sympathetic nerves incorporated in the trifacial. The cause of paralysis may be peripheral, but it is usually central, and in the greater number of cases is to be found in the growth of a tumour in the middle fossa of the base of the skull, or in the brain.—*New York Medical Journal*, August, 1871.

Glosso-Labio-Laryngeal Paralysis.—In a clinical lecture Dr. W. A. Hammond relates at great length a case of this comparatively rare affection, which was first noticed by Trousseau, but fully decribed by Duchenne. The following is a short abstract:—W. H. S., æt, 32, book-keeper; temperate, no hereditary taint. In the winter of 1867 first noticed altered sensation at angle of mouth, and inner canthus of eye on the left side. This extended, and in the winter of 1869 the left cheek and left temple were numb. There was also loss of sensibility in mucous membrane lining left cheek and gums. He chewed food on right side. In May, 1870, anæsthesia extended across the forehead to the right orbit and malar bone. He had ringing and impairment of hearing in left ear. In September, 1870, the numbness extended to naso-labial fissure on both sides, the eyes became congested, then there was difficulty in deglutition, vertigo on changing position, inability to walk in the dark, trouble in making water, pain in the occiput and vertex, dribling of salvia, loss of virile power, and embarrassment of speech. Dr. Hammond notices that the starting point in this case was the left trifacial nerve; in most cases it is the hypoglossal, as indicated by loss of motility in the tongue. The next nerve involved was the auditory; the disease, which commenced in the nuclei of origin of the fifth nerve, gradually extended until it involved those of the auditory, then the facial. At the time of the report the hypoglossal, the pneumogastric, and spinal accessory had not suffered. In the more typical cases, however, the tongue is first affected. We have simply progressive motor paralysis in muscles innervated by the hypoglossal, the facial, the pneumogastric, and the spinal accessory (partly through the pharyngeal plexus), and lastly, by some of the spinal nerves—for the phrenic, and even the intercostal nerves, seem to be sometimes affected. According to Trousseau, sensibility is wholly intact, and even the reflex irritability of the paralysed muscle is retained. Dr. Hammond's case, however, proves that there is another type of the disease, in which the primary symptom is loss of sensibility, attended sometimes by hyperalgesia, the motor paralysis not appearing until later. He adds that one of Trousseau's cases belongs to this category. In his remarks on the pathology of the affection he adopts Duchenne's theory of distinct sets of centric nerve-cells (sensory, motor, and trophic) having special functions, and traces paralysis of various forms to the disappearance of special centric nerve-cells. Little is known of the causes of glosso-labio-laryngeal paralysis. The prognosis is wholly bad. Treatment is useless.—*New York Journal of Psychological Medicine*, July, 1871.

The Physical Signs of Mitral Stenosis.—Dr. G. W. Balfour maintains that systolic apex-murmur is by no means the most distinctive sign of disease of the mitral valves. Such a murmur may be exocardial or endocardial, even produced by regurgitation, and yet the mitral valve be free from disease. The murmur, commonly known as "presystolic" (although this is not a strictly accurate term) may be considered conclusively pathognomonic of mitral stenosis. The presystolic murmur is most distinctly heard over the mitral area, *i. e.* within a circle of about a inch, described round the point where the apex impinges as a centre. It is not propagated far in any direction, it is rarely heard above the third rib, and in every other direction its distinct propagation is usually equally limited. The presystolic murmur, is really an auriculo-systolic murmur. By timing it with the carotid pulse (with which the first sound of the heart is synchronous) the murmur will be found immediately to precede and to run up to the carotid pulse. It occupies the time of the auricular systole, preceding the ventricular systole. "In timing this murmur it is obvious that we must employ the carotid, and not the radial pulse; for while the former is always synchronous with the ventricular systole, and apex beat, the latter is, even in health, always delayed to an appreciable extent—one-sixth of a second; while in disease, especially such as interferes with the arterial contractility, this delay is notably increased, and sometimes amounts to an entire cardial pulsation." The carotid pulse is a perfectly safe guide, provided our senses are sufficiently educated to appreciate the teachings obtainable by comparing an audible with a tangible phenomenon. The true auriculo-systolic murmur is short, because it sharply coincides with the contraction of the auricles; it is also rough because it is a direct murmr produced by forcible muscular contraction. The rough presystolic murmur, more frequently than any other, gives rise to a distinct sensation of vibration to be felt over the mitral area (*frémissement cataire*). It is capable of being vocalised by the sounds represented by the letters *R-r-r-b* or *Vōōt*. It is separated from the second sound by a more or less lengthened, but always readily appreciable interval, and it distinctly precedes the apex beat and the carotid pulse, usually running quite up to them; but occasionally separated from them by an exceedingly short, though appreciable, interval. Such a murmur is invariably an evidence of mitral deformity, of more or less constriction of the auriculo-ventricular opening. Such a murmur may disappear, but the lesion is permanent.—*Edin. Med. Jour.*, Nov., 1871.

On Narrowing of the Pulmonary Artery, contracted after Birth.—Dr. Constantin Paul sums up an elaborate memoir on this subject with the following conclusions:—1. The pulmonary artery is not only the seat of congenital affections, but may be the seat of affections acquired during extra-uterine life. 2. Amongst these lesions the most important is narrowing acquired after birth. 3. This narrowing is sometimes found at the level of the sigmoid orifice; it is produced by soldering of the valves with diminution of the orifice, and sometimes also of the calibre of the artery at this level. It is generally the result of an endocarditis. 4. The narrowing may be at the level of the arterial cone and be pre-arterial; this most ordinarily results from a myocarditis. 5. The narrowing may have its seat on one of the branches of bifurcation of the artery, or even on both; I have not seen it seated on the trunk of the artery, as is the case with the narrowing produced in the early months of intra-uterine life. 6. Beyond the narrowing, the artery is generally dilated. 7. It is generally associated with consecutive hypertrophy of the right ventricle. 8. Valvular narrowing of the pulmonary artery may accompany insufficiency of the same valves. 9. Lesion of the tricuspid, or of the valves of the left heart may coexist with pulmonary narrowing. 10. The proper symptom of narrowing of the pulmonary artery is a systolic *bruit de souffle*, more or less rasping, which is heard over the cardiac region, but is at its maximum at the level of the pulmonary orifice and along the vessel. 11. Narrowing of the pulmonary artery does not produce cyanosis. 12. In acquired pulmonary narrowing the foramen ovale is closed. 13. Nevertheless a myocarditis, developed during extra-uterine life, may develope at the same time a pulmonary narrowing and a communication between the two sides of the heart. 14. Narrowing of the pulmonary artery with persistance of the foramen ovale is probably, but not necessarily, congenital. 15. The recent character of the lesions would be a proof that a pulmonary narrowing had been contracted during extra-uterine life. 16. A frequent complication of pulmonary narrowing is consecutive tuberculisation.—*L'Union Médicale*, Dec. 23, 1871.

Œdema of the Lungs.—Dr. T. K. Cruse remarks that œdema of the lungs being intercurrent with maladies of the most opposite nature has a varied etiology. From observation of many cases of insolation, he regards it as the cause of a large proportion of deaths from sunstroke. It may complicate and render fatal the second stage of pneumonia. It sometimes supervenes with startling rapidity in Bright's disease, and in the temporary hyperæmia of the kidney of the puerperal state. It is sometimes the result of pressure on the lung by intra-thoracic tumours, pleuritic effusions, pneumothorax. Aneurism of the ascending aorta, hypostatic congestion from prolonged decubitus, obstructions to the systemic circulation, mitral stenosis and insufficiency, especially when complicated with pigment induration of the lungs, hepatic, and other abdominal tumours, and even intense tympanites are enumerated as causes of pulmonary œdema. According to the author, pneumonia is the disease in which œdema of the lung most frequently arises. In cases of sudden death from pneumonia, it is common to find one lung filled with inflammatory exudation, while bloody serum flows from the cut surface of the other. The treatment recommended is by dry cupping-glasses applied over the whole of the chest, to be left on until large serous blebs appear. The author relates the following case as an instance of the efficacy of bleeding. During the month of January, a woman was admitted to the lying-in wards of Bellevue, who had a rapid and easy delivery, but during the night was attacked by convulsions, which persisted during the night and morning of the next day, in spite of chloroform and elaterium. At the latter time her condition was as follows: Feet and legs œdematous; coma profound; urine loaded with albumen, contains fatty and granular casts; respirations, forty per minute; moist crepitation at every point over both lungs; patient foams at the mouth; surface blue. Dr. Barker then took from her forty ounces of black blood, which spurted from the vein as if under great pressure. In this blood the author found one part urea to every 960 other constituents of the serum. The patient had no more convulsions after the venesection, and, in addition, the surface assumed its normal appearance, respirations fell to twenty-six per minute; in five minutes the normal vesicular murmur was heard over the whole chest, and in half-an-hour consciousness returned.—*New York Medical Journal*, June, 1871.

Tubercular Peritonitis.—Dr. J. Kaulich has described with great minuteness the pathological appearances in this disease. In conjunction with the deposit of tubercular matter in the peritoneum, there is a copious serous exudation in the peritoneal cavity. Each tubercular deposit is surrounded by a newly developed layer of cellular tissue, with a tendency in this to simulate a layer of pseudo-membrane, with a strong disposition to a hæmorrhagic discharge. In the progress of the disease these masses of abnormal cellular tissue have a tendency to contract; their areolæ, by contracting, often cause the formation of areolated elevations, which may often be felt, like a network of cords, through the walls of the abdomen. The separate tubercular deposits may undergo changes in the progress of the disease of the nature of anatomical involutions. In such cases the peritoneum, in all its extent, exhibits no indication of recent irritation; the abdominal cavity is free from any exudation; the separate tubercles are dry, solid, and surrounded by a darkish pigment; others, especially large conglomerated masses of tubercular matter, are of a soft cheesy consistence, or dry, and of the appearance of mortar, while others of the conglomerated masses present the appearance of uniform masses of fat. In treating of the symptoms and course of the disease, the author notices the occasional formation of a painful œdematous inflammation at the umbilicus. This chronic inflammation usually disappears, seldom forming an abscess. Sometimes, however, an ulcerative opening is formed at the umbilicus, and the fluid in the peritoneal cavity is discharged, to the great relief of the patient. Dr. K. has never seen this occurrence exercise any unfavorable influence on the course of the disease. He warns against paracentesis, when the peritoneum is the seat of inflammation, or when hæmorrhagic exudation has probably taken place.—*Centralblatt f.d. Med. Wissenschaften,* 1871, No. 30, from *Prager Vierteljahr.*, cx. *American Journal of Medical Sciences,* January, 1872.

Hypertrophic Cirrhosis of the Liver.—Dr. Paul Olivier, in a memoir on this subject, believes he has established the fact that, besides the common form, atrophic cirrhosis of the liver, there is a rarer form, which is accompanied by increase of the volume of the organ. He regards hypertrophic cirrhosis as a distinct form of disease, and not one of the stages of ordinary cirrhosis, or a cirrhosis which has not had time for complete development. Like the atrophic form, it is characterised anatomically by a chronic irritation pervading the cellular tissue of the liver, whence proliferation of this tissue and atrophy by compression of the glandular substance; but whereas, in the atrophic form, this production ceases at a given time, in the hypertrophic form it continues; after having invaded the lobes of the liver, it extends to the cellular tissue interposed between the acini; and it is to the incessant accumulation of this tissue the large size of the liver is due. Besides the ordinary symptoms of cirrhosis, hypertrophic cirrhosis presents an augmentation of the volume of the liver, sometimes general, sometimes partial. This is its fundamental character; it is accompanied by an earlier and more profound alteration of the blood, whence various hæmorrhages from the mucous tracts, and more frequent true icterus than in the ordinary form. The author notes, also, a particular condition of the skin, the face being the seat of a papular eruption. The cause of the condition is alcoholic excess.—*L'Union Médicale,* September 26, 1871.

Abscess of the Appendix Vermiformis.—In a paper containing several observed cases of this disease, Dr. Leonard Weber makes the following observations on its pathology. After remarking on the fact that there are numerous cases on record where foreign bodies in the appendix seem to have produced no symptoms or inconvenience during life, he states that fæcal concretions found in the cavity of the appendix, when dry, appear, on section, to consist of a central nucleus, with concentric laminæ around, which are all of the same composition, viz. phosphate and carbonate of magnesia and lime, &c. They look very much like fruit seeds, and have often been mistaken for them, although they consist mostly of nothing but the ingredients of the human fæces. To explain their origin, we must suppose that, by irregular contraction of the cœcum, a small portion of fæces is forced into the appendix, and then hardened to a concretion by gradual absorption of its fluid parts. The calculi so formed become a source of permanent irritation to the mucous lining of the appendix, inflammations gradually develop, and that portion of the mucous membrane particularly pressed upon by the calculus becomes thinner and thinner by ulceration. The muscular coat and peritoneal covering are at length perforated, circumscribed peritonitis takes place around, adhesions are formed with neighbouring parts, especially the cœcum and omentum. By-and-bye, these adhesions also undergo ulceration, and then perforation must occur at one or two points of the appendix corresponding to the two ends of the calculus. Should the ulceration eat around the entire circumference of the process, as has been observed in a few cases, it may even cut off the appendix by gangrene, the calculus remaining fixed in its extremity, or dropping into the abdominal cavity, where it may be easily overlooked in the autopsy. Tubercular, and more rarely cancerous ulceration, may cause perforation of the appendix in a similar manner. The abscess resulting from perforation of the appendix may form and grow to the size of a man's fist, and larger, walled in by the previously adherent neighbouring parts. Its contents may become solid, from the gradual absorption of the fluid parts, leading finally to a firm fibrous union of its walls, in which the calculus is imbedded, with the laminæ of the appendix wholly or partly obliterated; or ulceration may encroach upon the walls of the abscess, when perforation will take place either into the peritoneal cavity, rapidly followed by general peritonitis, or outward through the parietal muscles, or into the intestines when pus is discharged per rectum. The proximity of the diseased appendix to the right iliac vein has been known to cause phlebitis. In a case recorded by Lewis, the coats of the vein were thickened and contracted below the appendix, and the vein filled by a thrombus in a state of decomposition; further up, above the process, it was filled with fresh coagula. The patient's right leg became œdematous, as in phlegmasia dolens. In a case observed by Hennoch, fatal hæmorrhage into the peritoneal cavity took place. When the abscess makes its way into the parietes, fistulæ may result. Pleurisy, pneumonia, pneumo-thorax, and pericarditis have resulted, from the abscess penetrating the diaphragm. When, upon the presence of a foreign body in the appendix, inflammation and gangrene do not supervene; the mucous membrane, distended and thinned by accumulation of its own secretion, is gradually converted into a serous membrane, secreting a thin albuminous fluid, the appendix itself forming an hydropic sac. Foreign bodies, as orange pips, cherry stones, &c., finding their way into the appendix, may produce similar pathological changes to fæcal concretions; but the latter are the cause of ulceration in the vast majority of cases.—*New York Medical Journal*, August, 1871.

Rachitic Pseudo-paraplegia.—Dr. J. S. Parry, in concluding a valuable paper on rickets, calls attention to the occurrence of pseudo-paraplegia as a form of rachitis. This is important, since it is often mistaken for paralysis, and the child is subjected to a course of medication for disease of the brain or spinal cord. "The child is small, has a large head and a weak intellect, with the aged expression common to this affection, but there is often very little bone deformity; indeed, it seems as if the disease had expended itself upon the muscles." Loss of power in the lower extremities is the common form of this variety of rickets (pseudo-paraplegia, Gee). The muscles are atrophied, and the child unable to stand, but it moves its limbs a little when lying or sitting. The muscles respond to electricity. That this condition is due to rickets is proved by the fact that it is preceded by the prodromes of this disorder, is accompanied by bending of the ribs, late dentition, open fontanelles, and it may be followed by bending of the bones of the legs when the child has recovered sufficient muscular power to walk. There is, however, often no serious deformity. The disease most frequently ends in perfect recovery, provided there is no serious defect about the thorax.—*American Journal of Medical Sciences*, January, 1872.

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CORRIGENDA.

No. XCIV (April, 1871), p. 410 (foot-note) *for* "Notes on Nursing," *read* "Notes on Hospitals." P. 411 (foot-note) *for* "a suggestion due to Dr. Ransome," *read* "a suggestion due to Mr. Lewis."

INDEX TO VOL. XLIX

OF THE

BRITISH AND FOREIGN MEDICO-CHIRURGICAL REVIEW.