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PART FIRST.

Analytical and Critical Reviews.

Review I.

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Hospitals considered with reference to their Construction, &c. By Armand Husson.

2. *Rapport sur les Hôpitaux Civils de la Ville de Londres, à point de Vue de la comparaison de ces Etablissements avec les Hôpitaux de la Ville de Paris.* Par MM. Blondel et Ser. —*Paris*, 1862. pp. 238.
Report on the Civil Hospitals of London as compared with those of Paris. By MM. Blondel and Ser.

3. *Notice sur l'Hôpital Saint-Jean. Etude sur la Meilleure Manière de Construire et d'Organiser un Hôpital de Malades.* Par André Uytterhoeven. Deuxième édition.—*Bruxelles*, 1862. pp. 290.
Notice of the Hospital of St. John. On the best Mode of constructing and Organizing a Hospital for the Sick. By Andrew Uytterhoeven.

4. *Discussion sur l'Hygiène et la Salubrité des Hôpitaux, à la Société de Chirurgie de Paris, Octobre, 1864.—Paris*, 1865. pp. 137.
Discussion on the Hygiène and Salubrity of Hospitals, at the Surgical Society of Paris.

5. *Notes on Hospitals.* By Florence Nightingale. Third Edition.—*London*, 1863. pp. 187.

6. *On Hospitals.* By Sir J. Ranald Martin, M.D. Being an Article in 'A System of Surgery,' edited by T. Holmes, M.A. —*London*, 1864.
7. *A Manual of Practical Hygiène.* By Edmund Parkes, M.D. —*London*, 1864. pp. 612.
8. *Report by Dr. John S. Bristowe and Mr. T. Holmes on the Hospitals of the United Kingdom.—London*, 1864. pp. 281. Contained in the Sixth Report of the Medical Officer of the Privy Council.
9. *The Builder* (an Illustrated Weekly Magazine). Various articles on Hospital Construction.—*London*, 1858.
10. *Polizia degli Spedali.* Scritta dal Dottor Giovanni Pozzi. —*Livorno*, 1839. pp. 378.

On Hospitals. By Dr. John Pozzi.

The origin of hospitals has commonly been assigned to the influence of Christianity, and even if this opinion be not entirely correct, there can be no doubt that the divine commands of the Author of Christianity, to love our neighbours and to succour the poor, were influential in promoting the construction of places of refuge and relief for the sick and destitute in every country that received the Gospel. As early as the year 370 after Christ, we read of a large hospital being established in Cæsarea, in Cappadocia, by the good Bishop Basil, who at that time governed the Church in that city. Similar institutions speedily arose in Constantinople and other cities of the Roman empire, and obtained imperial notice and patronage, as shown in the Institutes of Justinian, in the year 529.

These early Christian establishments were under the immediate control and direction of the bishops of the towns in which they were situated—a circumstance partly due to the fact that the bishops acted as the almoners of the collections made in behalf of the poor, in whose interests the hospitals were founded. However, these institutions were primarily not solely devoted to the reception and treatment of the sick, but served also as refuges for the old and infirm, for the destitute and for the wayfarer, and sometimes, likewise, for the bringing up and instruction of orphans. The sick obtained a place within them, inasmuch as they belonged to the category of those who had a claim upon Christian sympathy and benevolence; and it was, without doubt, their miserable and forlorn state, as outcasts of society, that induced Basil and other Christian bishops to make special provision for lepers in the hospitia they founded. It may, moreover, be assumed that the erection of the hospitium outside the city was, to a certain extent at least, dictated by its object as a refuge for those wretched beings who were, by custom and rule, interdicted from towns.

Hospitals, infirmaries, and refuges of the poor may, in general, so far as Europe is concerned, be therefore considered Christian monuments. They constituted, originally, a part of the machinery of the Church, as the centre of philanthropic action, and in most countries of the Continent their connexion with the Church is maintained, to a considerable extent, at the present day. Certain members of the religious fraternity who governed the hospital were at the same time the administrators of medical relief; the offices of priest and physician were united in the same person, and it was not until the sixteenth century that those offices became dissociated, that the art of physic acquired a distinct social position, and assumed that individuality which has ever since been assigned to it in the working of sick hospitals.

In England, especially, of all the countries of Europe, have the hospitals for the sick become detached from ecclesiastical associations and control. In those institutions elsewhere, particularly in Roman Catholic countries, the ancient religious element retains a much more prominent and officious character. Even in Paris, the principal hospitals are served by two or more resident priests, and the offices and ceremonies of religion are much more amply provided for and attended to than in England. Moreover, the nursing of the sick has always been, in those countries, retained in the hands of religious orders, together with much of the internal economy of the hospitals. Indeed, there are institutions for the care of the sick, more especially of the insane, to be seen in France, Italy, and elsewhere, entirely belonging to religious confraternities, and either served medically by physicians and surgeons from without, or even, as in the case of the hospital and asylum of St. Servolo, at Venice, by brethren of the order trained in the practice of medicine, and living within the establishment.

Within the last few years the entirely lay character and management of British hospitals has, in some degree, been broken down by the introduction into several of their number of ladies and others trained as nurses, and at the same time members of private associations, banded together by religious ties, amenable to particular rules, and governed by a responsible head. In Dublin, the establishment of the "Mater Misericordiæ" hospital by the "Sisters of Charity," is an instance of a revival of the ancient connexion between the convent and the house for the sick poor. The prevailing tendency of the present time in England seems to be to re-invoke the religious element in the nursing of the sick, both in public and in private; the conviction obtaining, and being supported also by experience (now that prejudices are becoming weaker), that nursing is more efficient and humane when carried out under religious sanction by individuals devoted to it as a work of love and mercy.

In the above remarks credit has been taken for the influence of that primary principle of Christianity—charity, in the institution of receptacles for the nurture and tending of the sick poor; and, so far as that principle has operated, Christianity may be fairly deemed instrumental in the establishment of hospitals. Yet there is no question that, prior to the dawn of Christianity, institutions for the benefit of the sick were to be found in every country that had attained the higher degrees of civilization then existing. Wherever the religion of Greece and Rome extended temples arose dedicated to Æsculapius, and health-giving fountains were discovered and consecrated to Hygeia, to which sick folk might resort, and where they might receive aid and advice from the attendant priests and ministers. Certain observances and ceremonies were to be gone through, which, though identified with religion, were at the same time calculated to benefit the health, being derived undoubtedly in a great measure from the teachings of experience. Indeed, much knowledge of disease must have been acquired by the temple attendants, both from their own observation of the applicants for relief and from the records of former cases, which were, as we know, preserved; and, besides this, their interests were more or less identified with the reputation and success of the sacred place they served.

In Rome, again, after the establishment of imperial rule, the public baths or thermæ assumed a development and importance never since approached, and afforded the citizens not only enjoyment but also energetic means of curing disease; so far, therefore, they fulfilled the purpose of hospitals. However, both these thermæ and the Æsculapian temples approached in kind nearer to the modern water-cure establishments than to our sick hospitals as at present constituted. In the case of the temples great wisdom was displayed in the choice of their sites. The most renowned stood on hill sides, overlooking the sea, surrounded by the finest scenery, and amply supplied with water for the ablutions and baths prescribed for their votaries. They therefore possessed the principal and most important requirements of hospitals.

In estimating precisely how far Christianity was concerned in bringing about the establishment of hospitals, the political and social system of Rome, and the mighty social influences at work in the later periods of the empire, must be borne in mind. The population of ancient Rome consisted of freemen and slaves; the latter possessed in large numbers by the nobles and wealthy classes. These slaves, as valuable chattels, had their health, without doubt, looked after at the cost of their owners, and history tells us that the medical practitioners at first belonged especially to the slave caste. There was, consequently, in this social system no place for the operation of sick hospitals as such, and it was not until various political causes so broke down and modified it, that public provision became necessary for the medical relief of a class of sick poor. This provision it was the privilege of Christianity to supply.

This subject of the original foundation and character of hospitals might advantageously be investigated; to examine it here would be beside the subject we have in hand. The saying has of late become popularized, that hospitals are institutions proper to an imperfect civilization; but we look upon it as one of those smart pseudo-philosophic dicta for which the present day is remarkable. It is possible, indeed, to imagine hospitals unnecessary and of a bygone character in a Utopian society where no sick poor, or, at least, none unable to obtain efficient medical aid and nursing should exist; where all medical practitioners should be equally enlightened and skilled, and where the field of experience, observation, and research afforded by large medical institutions would be worthless in advancing medical science, in creating a high class of physicians and surgeons, and also in training those who should adopt the medical profession as their calling. A state of society admitting of all this, and implying such an equality of talent, and with it such an equality in the appreciation by the public of all practitioners in medicine, is one that we cannot regard as likely to be realized whilst human nature is what it is. Consequently we infer that hospitals, however degrading as marks of imperfect civilization, will ever be necessary in the most civilized society to be developed; and we trust that philosophers, equally with meaner folk, will not think their efforts lost in endeavouring to make such institutions more worthy than they at present are of the civilization of the age.

It would be much more of a truth to say that hospitals have not, either in their construction or management, kept pace with advancing civilization. Their primary end and aim, that of curing their patients as speedily and completely as possible, has not been kept prominently enough in view. Efforts have been expended rather in favour of multiplying patients within their wards, than in making hospitals instruments of treatment; facility of access has been more studied than facilities of cure; and the administration of medicines and brilliant or ingenious operations have received much more attention than the provision of good nursing, of pure air and free ventilation.

The heathen of ancient Greece and Rome had the sagacity to construct their Æsculapian temples on the most healthy and beautiful spots; and the earliest hospitals instituted by Christians were placed outside towns in a purer atmosphere. At later periods, in the middle ages, the conventual hospitals, though, from various circumstances attaching to the time, built within the walls of towns, were, as a rule, surrounded by gardens, and occupied along with their enclosed courts a large space of ground, whereby the health of their inmates was better preserved. But as the present century approached, and as the secular element replaced the religious in the ancient hospitals, the aggrandizement of the buildings and of the revenue became the leading object; and whilst the wards were added to, the space around the hospital was gradually curtailed as the city population augmented, and as the land in consequence became more valuable, and by its sale furnished the means of increasing the funds. This state of things has culminated within the present century; hospitals have been erected and enlarged regardless of the fitness of site, and of the necessity of pure air to their inmates; they have assumed the character rather of receptacles for sickness than of places of cure, and the wants of a densely-populated poor district have been urged as the one and sufficient ground for their establishment in the crowded locality. Within the last few years, however, the purposes and the utility of hospitals have been more considered and better comprehended. Their primary object is now recognised to be the reception of the sick with a view to their recovery by the agency of hygienic as well as of medicinal treatment, under the care of medical men and nurses of extended experience, in buildings affording every possible sanitary condition, subject to conditions conducive of economy in relation to the results attained.

The first essential condition of a good hospital is, that it should be so situated as to be surrounded and freely permeated in every corner by pure air. A country site, on a healthy hill-side, at once suggests itself as fulfilling this condition; but the difficulty of transfer of the sick applicants for relief, who are living more or less remote from its healthy wards in the confined streets and lanes of the neighbouring town or towns, at once occurs as constituting an insuperable objection to such an extra-urban locality. The objections to placing hospitals outside the towns which they are chiefly destined to serve, have a very different weight attached to them by different physicians. Some regard them as so great that the gain promised by the removal does not at all compensate for the disadvantages entailed; whilst others are of opinion that large general hospitals should be in the country, and that only small infirmaries for urgent cases and accidents, and for clinical instruction, be constructed in towns. This latter opinion was held by the majority of speakers in the recent discussions of the Surgical Society of Paris. M. Léon le Fort entered largely on this topic, and examined in detail the several circumstances adjudged adverse to extra-urban sites. He argued that facility of access is comparatively unimportant to the large proportion of hospital patients; that in scarcely a tenth of their number would the removal to the necessary distance be attended by any difficulty or danger. To meet the requirements of this minority, he suggested the erection of small infirmaries, containing from 80 to 100 beds, or exceptionally as many as 150, in populous quarters of the town. With proper ambulances the difficulty of removal would be well-nigh overcome, and railway communication might be made available to lessen it still further. The deprivation of the patients, to a greater or less extent, of the visits of their friends, would be certainly an evil, but it would be more than compensated for by their more speedy recovery, and the numerous advantages of a cheerful and spacious residence during the period of their illness. To the objection, that a suburban hospital would impose increased labour, and involve loss of time on the visiting medical staff, he replied, that the hospital is constructed for the sick, and not for the doctors; and he contemplated the probability of such outside establishments becoming the abode of medical officers, who, as residents, might devote themselves especially to medical research, and so more fully utilize the opportunities therein furnished, than occasional medical visitors engaged in private practice can possibly do. Lastly, to meet the remaining objection urged against extra-urban hospitals—viz., that medical students would be deprived of the benefit of clinical instruction, he proposed the institution of a special clinical hospital sufficiently near the city to be readily accessible. These views were supported by M. Larrey, whose opinion was evidently highly valued by the other members of the society; and at the termination of the several sittings, the conclusion was accepted as that of the members at large, that only small hospitals for urgent cases, and those required for clinical instruction, should exist within towns, and that not only would the salubrity of the larger hospitals be improved by their extra-urban position, but that also their construction would be rendered more economical by reason of the less cost of the land required for their site.

Miss Nightingale is equally opposed to the erection of hospitals in towns. In her remarkably terse and vigorous style, she writes:

"If the recovery of the sick *simply* is to be the object of hospitals, they will not be built among dense, unhealthy populations." (p. 26.)

"Land in towns is too expensive for hospitals to be so built as to secure the conditions of ventilation and of light, and of spreading the inmates over a large surface-area—conditions now known to be essential to speedy recovery—instead of piling them up three or four stories high, in regions contaminated with coal-smoke and nuisances." (p. 27.)

Again, farther on (pp. 31, 32,) when discussing the question of accessibility, she observes, that were it possible to remove the existing large metropolitan hospitals "to the farthest accessible healthy points from their present positions, there cannot be a doubt that the sick would gain immensely by the change, except accidents or cases of severe and sudden illness. For these cases special wards would have to be provided at the points where they would be most required." The writer of the articles on hospitals in 'The Builder' (1858, p. 577) coincides in the same opinions and suggestions. And Sir Ranald Martin lays down (op. cit., p. 989) the following rules of hospital construction:

"Never erect a general hospital within the precincts of a town, or in suburbs likely to be built upon. Remove all general hospital establishments out of town, and from populous suburbs, as soon as circumstances may permit; or when such removal is not practicable, let all the available ground around the institution be purchased. Build all general hospitals in the country on areas of ground sufficient to admit of extensions of buildings, and to prevent other buildings from being erected within such a distance as shall interfere with a free circulation of air and with quiet."

Rightly cognisant, however, of certain necessities arising from social conditions, particularly in reference to the poor, Sir R. Martin proposes the erection of "certain hospitals of immediate necessity within crowded cities," to which should be attached convalescent hospitals in the country, "supplementary to and cooperative with the central institutions."

Besides these very recent writers who testify against the erection of hospitals in large towns, and lament the sins of their forefathers in so doing, we may also refer to the Italian author, Pozzi, who wrote a very good treatise on hospital administration in 1839. He discovers an additional argument against such sites from a consideration of the ill effects of hospitals upon the inhabitants in their vicinity. This is reversing the point of view adopted by the preceding authors quoted. Pozzi finds the neighbours living around a town hospital injuriously affected by its bad air and exhalations; the other writers, on the contrary, declare that its inmates suffer from the contamination of the air by the townfolk around. There is a measure of truth in both propositions, but it may safely be held to preponderate in the latter of the two; still it is only fair to Pozzi to state, that when advocating the location of hospitals away from towns, he insists upon the evils of a town atmosphere as noxious to the sick.

Dr. Bristowe and Mr. Holmes, in their excellent Report (op. cit., p. 478) take opposite ground to the foregoing authorities respecting this question of site. The good effects of the removal into the country of any of the large London hospitals upon their patients would, in their opinion, be, if any at all, very trifling:

"That there might be some slight variation in the mortality may or may not be probable, but that the prevalence of hospital disease would be much decreased, that operations on given cases would be much more likely to succeed, or that the period of recovery of given cases would be much abridged, we do, judging from the evidence before us of the state of things in hospitals variously circumstanced as far as situation goes, disbelieve. In fact, we have no evidence which shows that any change at all would be wrought in any one of these three particulars.

"But if the effect of the removal were to change the supply of patients, whether as to the nature of the cases or as to the class of persons admitted—if, instead of a large proportion of dangerous accidents and acute diseases, the practice of the hospital were to lie chiefly among chronic invalids and convalescents—or if, instead of the worn-out victims of want and debauchery, who are the principal subjects of injury and acute diseases in towns, the healthier inhabitants of a country district were to become the inmates of the hospital—then there can be little doubt that the success of treatment would be considerably increased . . . . We believe it would be impossible to treat the sick poor of a great city anywhere except near their homes, at least those of them who are most seriously ill, and who are more especially the objects of treatment in our large hospitals. The experience of St. Thomas's Hospital is in point on this matter. The removal of the hospital from the Borough to the Surrey Zoological Gardens, in Walworth, was a much less extensive change than the one we have supposed. The new hospital was within walking distance of the situation of the old one, so that no country journey and railway fare prevented the patients being visited by their friends; yet the change in the class of cases is manifest, as is also the increased pressure upon the beds of Guy's Hospital. Now, in any scheme for a country hospital to receive the patients of a city, there must always be left in the city a depôt for the acute cases and grave accidents not admitting of the transit. The pressure upon the resources of the latter would therefore increase; and, above all, if fever were received, there would be much risk of its becoming a fever-house only, or only a fever-house with a separate department for accidents. The country department would then become what our country infirmaries now too often are—viz., a receptacle for chronic invalids, and the acute non-febrile cases would be left without hospital accommodation. This would be a very grave evil; nor is it easy to be satisfied, from our investigations, that it would be compensated by any corresponding benefit. If it seems probable that hospital diseases are relatively as common, and recovery neither more certain nor more speedy in the country than in the town, to what purpose should the hospital be moved, and its existence as a school of medicine and surgery be thereby endangered?"

Passing on to examine the scheme of having "country departments" to large city hospitals (p. 480), Messrs. Bristowe and Holmes arrive at the inference that the establishment of such subsidiary and convalescent hospitals would be in effect "not only different from, but even opposite to that of removing the bulk of the hospital into the country and leaving only a small receiving-house in town; for patients, knowing that they would have the option of remaining in town if they chose, would, of course, be attracted by the offered alternative of a gratuitous sojourn in the country." Moreover, the plan would offer the great advantage of preventing overcrowding and its attendant evils in the urban institution.

In confirmation of these views, the writers named quote with approbation "a report issued by a committee of the governors of St. George's Hospital, appointed to consider the question of founding a convalescent, or, perhaps we ought rather to say, a subsidiary hospital in the country, in connexion with that hospital." The quotation contains the following statement:

"It is quite unnecessary in the present day to put forward any arguments as to the expediency of a certain class of cases, or rather of diseases and accidents in certain stages, being treated in the country rather than in London, however well a hospital may be situated. Even in St. George's Hospital, unrivalled for its situation in this or any other city, in a ward well placed for air and ventilation, one of the scourges of surgical disease, phagedæna (sloughing of sores and wounds), has been exceedingly prevalent during the past season. To be enabled to send away patients for the further treatment they may require when such a disease makes its appearance would certainly result in the saving of many lives."

According to Messrs. Bristowe and Holmes, the above statement relative to the prevalence of phagedæna in one particular ward is incorrect, the disease having spread through most, if not all, the surgical wards, though chiefly in "that which received the greatest number of operation cases. But phagedæna at the same time also attacked a far larger number of persons (forty in all) outside the hospital, who were afterwards admitted on account of the disease, and thirty-three who were treated as out-patients." (op. cit. p. 481, foot-note.)

Having now before us the principal reasons urged *pro* and *con.* the erection of hospitals in the country, a few words by way of comment may here be introduced. If it be allowed that "subsidiary hospitals" in the country are desirable, the whole question becomes one of the proportionate development those institutions should assume relatively to the town establishments. In other words, it becomes a question of selection of cases admitting removal, of the relative number of such cases, and of the available means of transit. Dr. Bristowe and Mr. Holmes would, for their part, in the case of London, to which their opinions refer, retain the present large hospitals and erect auxiliary institutions in the country, whilst the members of the Surgical Society of Paris, Sir Ranald Martin and others, on the contrary, would adopt rural sites for the principal hospitals, and construct smaller buildings, as supplementary to them, within the town precincts, for casual and severe cases of disease and for clinical instruction. By the one scheme, town hospitals, would constitute the principal places of treatment; by the other, country hospitals.

To admit that subsidiary and convalescent hospitals in the country are needed to supplement the town establishments as means for the better and more rapidly restoring the sick, is tantamount to admitting that some disadvantages do attend town sites, and to a recognition of the benefits of fresh uncontaminated air and of ample surrounding space. Consequently, it is to little purpose to contend, in the face of such an admission, that no change "would be wrought in any one of these three particulars:"—the prevalence of hospital disease, the success of operations, or the duration of illness, by the removal of the great London hospitals into the country.

It is well to examine the evidence upon which Messrs. Bristowe and Holmes base this opinion. The evidence relied upon is that of the statistics of the results of medical and surgical practice in London and in provincial hospitals, which, on comparison, are made to appear to be as satisfactory in the former as in the latter. In the comments accompanying them, we cannot help thinking that we trace an endeavour to explain away results when unfavourable to the metropolitan establishments, or, in other words, to maintain a foregone conclusion that rural hospitals *quoad* their sites possess no superiority as places for recovery over those located in London. However, though the Government reporters have deduced this conclusion from their statistics, the Paris surgeons, on the contrary, appear to have satisfied themselves that those same statistics do demonstrate the advantages of rural sites for hospitals, for they appealed to them in evidence of this in their discussion. In our opinion, they are of little value in support of either of the two propositions. The figures collected do not represent facts of the same character and value capable of comparison. Indeed, the running commentary accompanying the statistical tables produced, indicates how little weight can be assigned them in deciding the question in debate; nay, more, it shows that their compilers themselves have a poor opinion of their worth.

In short, the circumstances and conditions, the ages and character of patients, and the class of diseases admitted, the prevailing state of the wards, whether full or nearly empty, the dietary, the nursing and the treatment, &c., vary almost infinitely between one hospital and another, even in the same place, and between town and country hospitals in a still higher degree; and it is impossible to express or to estimate these by figures. Scarcely any even of the provincial hospitals are, strictly speaking, rural hospitals, but are for the most part built in cities of more or less magnitude, and consequently are surrounded by hygienic conditions similar to, and often not superior, to those existing in London. Very few of them, therefore, can be adduced as exemplifying in any degree the advantages possibly derivable from a country site. Moreover, most of the provincial hospitals were erected when the principles of hospital construction were ill-understood, and exhibit the consequences of this in structural defects in a higher degree than do the metropolitan establishments. Consequently, they present no criteria for judging what benefits may accrue from any superiority of site they may possess, inasmuch as this, whatever it be, is forfeited by their faulty sanitary arrangements.

Further, the conditions and circumstances of different hospitals differ so much in relation to the rules of admission, to the demands upon the accommodation furnished, and to various matters of administration and management, that no conclusions of value can be drawn from their statistics of mortality, or of the average duration of any of the forms of disease admitted. Much might be written in illustration of this statement, but we shall restrict ourselves to one example, viz., that the length of time during which patients are retained in the wards is as much dependent upon the demands for accommodation as upon the degree of progress towards recovery. If there be pressure on the ward space, the slighter cases and those approaching convalescence will be discharged ; or, *vice-versâ,* their residence in the hospital will be protracted. Hence arguments derived from the statistics of duration of treatment are well nigh worthless on this ground only; leaving out of consideration that cases even of the same disease are not comparable when represented by units, by reason of their varying intensity, the different stages to which they have advanced at the time of admission, and of those many particulars which impart an individuality to every case of disease.

M. Legouest presented to the members of the Paris Surgical Society (' Discussion,' p. 66) a collection of statistics from the Military Hospitals of Val-de-Grâce, Le Gros-Caillou, and Vincennes (the two former establishments being urban, and the last-named extra-urban), with the view of elucidating the much-debated question of the influence of site upon the inmates of hospitals. Statistics gathered from such sources he rightly estimated as of greater worth, inasmuch as their "hospital conditions" are more uniform; "they only receive a homogeneous population—namely, patients of the same sex, whose ages range from twenty to thirty-five at most, exercising the same profession, and consequently subjected to similar causes of disease, and being, so far as concerns their physical condition, the pick of the young men of the country." In such establishments, he contended, the oscillations in the rate of mortality must be more contingent on their sanitary conditions than aught else.

In further aid towards arriving at a just inference, M. Legouest gave a description of the three hospitals compared, from which it appears that though the Val-de-Grâce and the Gros-Caillou be within the walls of Paris, they both, particularly the former, are surrounded by ample open ground, occupied as gardens and exercising-courts, and therefore largely partake of the advantages accruing from a rural site. A calculation of the superficial area shows that 245 square mètres are afforded to each patient at the Val-de-Grâce, 87 at the Gros-Caillou, and 250 at the Vincennes hospital.

The statistics of the two older institutions extend over a period of thirty-four years, and those of the Vincennes hospital over the six years only during which it has been in operation. The general result is, that the mortality of Val-de-Grâce has averaged 4·445 per cent., that of Gros-Caillou 4·880 per cent., and that of Vincennes 2·110 per cent. An examination of the mortality returns of the two former hospitals demonstrates a reduction in the percentage, traceable to the structural improvements carried out of late years; so that the mortality of Val-de-Grâce in the course of thirty years has declined 1·169 per cent., and that of the Gros-Caillou as much as 1.424 within the last twenty years.

An explanation of this divergence in the extent of amelioration in these two establishments is given by M. Legouest; but, as it does not concern the immediate subject in hand, it need not be here produced. But taking the rate per cent. of the last ten years, it stands at 3·932 at the Val-de-Grâce, and at 3·234 at the Gros-Caillou, as compared with 2·110 at the Vincennes hospital.

From these results, M. Legouest considered himself justified in concluding that "hospitals external to towns are more salubrious than urban hospitals; that those which are so situated as to be surrounded by extensive grounds, and are built in a single line, as is the case with Val-de-Grâce, are in the best conditions; but where such conditions cannot be realized, the defect may be partially remedied by the construction of 'exchange wards,' as has been done at the Gros-Caillou, by raising the new building." (p. 73.)

Exception was taken to these inferences, in the course of the discussion, by M. Gosselin, who attributed the different rate of mortality at the two older military hospitals as compared with that at Vincennes, to the greater gravity of the cases entering the former, and treated the statistics adduced as unsatisfactory from their dealing with maladies in mass. Instead of this plan, he advocated the selection of certain definite lesions for comparison. However, M. Gosselin offered no evidence of his assertion relative to the greater gravity of the cases admitted into the urban military hospitals, nor was it treated by M. Trélat in his concluding summary as of any import. The last-named physician, moreover, referred to the data furnished by M. Verneuil and others, as furnishing the materials for comparison demanded by M. Gosselin, and as confirmatory of the general statistical results arrived at by M. Legouest.

Assenting to these conclusions, we also agree with Miss Nightingale (op. cit. p. 27), that, "according to all analogy, the duration of cases, the chances against complete recovery, and the rate of mortality must be greater in town than in country hospitals." And this inference, we maintain, is not invalidated by the array of figures Dr. Bristowe and Mr. Holmes have adduced in their elaborate Report.

Nor do we think those gentlemen have satisfactorily established their objections to extra-urban hospitals as being attended by greater inconveniences than can be counterbalanced by their admitted hygienic superiority over town institutions. They assert that it is impossible to treat serious cases of illness among the "poor of a great city anywhere except near their homes;" but they do not prove this statement. The altered condition of things consequent upon the removal of St. Thomas's Hospital nearly two miles from its ancient site, is advanced, indeed, in support of it. They state that the removal produced a manifest change in the class of cases admitted, and an increased pressure upon the beds of Guy's Hospital. All this might have been predicated; but as an argument in favour of their assertion, it is of small worth. The removal of St. Thomas's to the Surrey Gardens was merely a temporary expedient—it was no part of a plan of making London hospitals extra-urban; it caused a dislocation of an ancient condition of things to which the public had become used, and no inducement was offered to the denizens of "the Borough" to resort to a makeshift hospital transplanted to a neighbouring site. On the contrary, there still remained with them a large and wealthy hospital to supply their wants, and to it they therefore had recourse.

Moreover, the reporters make no allusion to the considerable number of cases, particularly surgical, received at the former St. Thomas's Hospital from the country, and from the large towns on the river side below London Bridge. That hospital, and its neighbour Guy's, served very large districts in Kent and Surrey;

and this very fact itself controverts the statement, that serious cases of sickness cannot be treated excepting near the places where they originate. The practice pursued with regard to the admission of patients at the Paris hospitals, may be further adduced in opposition to that assertion. Except urgent cases of accident, all others have first to apply at the Central Bureau of admission, and are transferred, at the direction of the staff attending there, to some one of the several hospitals of the city; it may be to the remotest from their habitations. We do not commend this arrangement, but mention it as setting at nought the impossibility imagined by Messrs. Bristowe and Holmes, of treating patients anywhere except near their homes.

Further, the other difficulties felt by these gentlemen in the way of transplanting large hospitals into the country, we cannot consider as of much account. That there must be in a city "a depôt for the acute cases and grave accidents not admitting of the transit" to the country establishment, is clear enough; but that the pressure upon its resources "would therefore increase," is an induction we do not recognise. Nor can we acquiesce in the conclusion that, "if fever were received, there would be much risk of its becoming a fever-house only, or only a fever-house with a separate department for accidents." On the contrary, it should be made a special object to transfer fever patients into the country, and, unless in a sinking state, such patients are readily removable. Lastly, we do not share in the apprehension expressed, nor in the implied regret, that "the country department would become what our county infirmaries now too often are—viz., a receptacle for chronic invalids; and the acute non-febrile cases would be left without hospital accommodation." In arguing thus, the writers in question seem to forget that, should the supposed removal of a hospital take place, certain regulations and arrangements for the transfer of patients, and for rendering it an efficient hospital, would assuredly be made, and that both non-febrile acute as well as febrile diseases would be provided for within its walls. But it would not discredit the charitable end and aim of a metropolitan hospital, that, by its translation to the country, it, in some measure, served as a receptacle for invalids; or, in other terms, that it combined the advantages of a convalescent establishment with those it had heretofore offered to the sick poor. This double purpose is, in truth, served by some of the county infirmaries, and hence the larger proportion of chronic invalids in their wards than in those of the London Hospital. They may be, and are, indeed, too much encumbered by trifling and also by chronic cases, which have but slight claim for in-door treatment; but this circumstance is to a certain extent due to the vicious practice of recommendation by governors without proper revision of the cases admitted by the medical staff.

This subject of country sites for hospitals we must now quit after making a few remarks on the following paragraph in the official Report: "The effect of this (i.e., a subsidiary or convalescent hospital in the country) would probably be not only different from, but even opposite to, that of removing the bulk of the hospital into the country, and leaving only a small receiving house in town; for patients knowing they would have the option of remaining in town if they chose, would of course be attracted by the offered alternative of a gratuitous sojourn in the country." (p. 480.) We are at a loss to understand the last clause as affording any reason for the supposed difference and contrariety in effect expressed in the former portion of the paragraph. However, letting this logical obscurity pass, we meet in this quotation with the admission of a circumstance which should be kept prominently in view in a discussion concerning the relative value of town and country sites. The sick poor would, we are rightly told, "be attracted by the offered alternative of a gratuitous sojourn in the country." This attraction felt by the persons to be benefited towards a particular scheme surely should be kept in view by those who profess to confer the benefit. And, so far as our experience goes, the poor are never backward in appreciating the advantages and enjoyments of country air and country scenery. The simple removal from the noisy narrow streets and close courts which they occupied to the open country, could not fail to have a cheering and beneficial influence upon the poor sufferers from disease; and doubtless, a hospital so situated would be sufficiently attractive to avert the dreaded evil of increased pressure upon the auxiliary town establishment.

From a review of the whole question, we come to the conclusion that, as a general fact, the open country offers the best site for a sick hospital; but that, inasmuch as certain cases or classes of patients cannot advantageously be removed from their town homes to a country hospital, situated more than two or three miles distant, it is necessary to provide for such in smaller town establishments, until their condition may justify removal. These town hospitals, moreover, would constitute excellent schools for clinical instruction. The proportion of cases not admitting of removal, and the difficulties in the way of transport, we hold to be much exaggerated. Lastly, we are not in possession of satisfactory data collected in country hospitals having good sites, good air, good construction, and good regulations, to enable us to measure and to express in figures the advantages which they possess over urban ones considered as institutions for the recovery of the sick.

Next after the question of site in consideration, and equal, if not superior to it in importance, is that of the best mode of construction. The surrounding atmosphere of the hospital is, as M. Trélat called it, the common reservoir for the building and its inmates, and not only is it of primary consequence that the supply of air be good and ample, but that it also freely permeate every part of the interior, impinge upon every wall, and purify every corner. Furthermore, light must accompany the air and penetrate into all the rooms and passages to make bright and cheerful, and by its chemical properties to contribute to the well-being of the inhabitants. Subordinate to these requirements are those of rendering the building as convenient as practicable in all that concerns internal management, and of securing in its construction the greatest economy compatible with efficiency.

How not to fulfil the primary requirements in construction is illustrated in English hospitals of past periods, with very few exceptions. At the present day, happily, there is much concord between authorities on hospital architecture. The defects in existing buildings have been pointed out by Husson, in his comprehensive treatise, 'Étude sur les Hôpitaux;' by Uytterhoeven, in his Essay on St. John's Hospital, Brussels; and very cogently by Miss Nightingale, in her far-famed 'Notes on Hospitals.'

Some institutions for the sick, to which the term hospital is attached, are comprised within the limits of an ordinary house, and in no way exemplify the principles of hospital construction. Among buildings specially erected as hospitals, the older ones, particularly those of the Continent, retain many of the features of structure met with in convents, of which, indeed, they may be regarded as the offspring. Among these monastic features is the arrangement of the buildings around three or four sides of a rectangular court, and where the hospital has been much extended, it is common to meet with this disposition of the wards, once or more repeated. All hospitals so constructed "stagnate the air even before it reaches the wards (we quote Miss Nightingale's words, op. cit., p. 32). This defect is one of the most serious that can be committed in hospital architecture. . . . . The air outside the hospital cannot be maintained in a state sufficiently pure to be used for internal ventilation, unless there be entire freedom of movement. Anything which interferes with this is injurious."

In the pavilion system alone does this able writer recognise the correct principles of construction; each pavilion constituting a—

"Separate detached hospital, which has, or ought to have, as little connexion in its ventilation with any other part of the hospital, as if it were really a separate establishment miles away. The essential feature of the pavilion construction is that of breaking up hospitals of any size into a number of separate detached parts, having a common administration, but nothing else in common. And the object sought is that the atmosphere of no one pavilion or ward should diffuse itself to any other pavilion or ward, but should escape into the open air as speedily as possible, whilst its place is supplied by the purest obtainable air from the outside." (p. 56.)

Again, "pavilions may be placed side by side, or in line;" the latter arrangement "is most suitable for small hospitals with fewer than 120 beds."

This pavilion plan is advocated also by Uytterhoeven, by the writer in 'The Builder,' and by Sir Ranald Martin. From Dr. Parkes we further learn that it has been accepted as a rule for military hospitals ('Practical Hygiene,' p. 298). It is seen in its fullest development in the celebrated Lariboisière Hospital, in Paris, and, with a nearer approach to perfection, in the newly-erected Herbert Hospital, near Woolwich. It was carried out in the rebuilding of St. John's Hospital, Brussels, between 1837 and 1848; it has been adopted for the new Leeds Hospital, for the new St. Thomas's Hospital, London, and for several county infirmaries, including those for Bucks and for North Staffordshire.

The adaptability of the plan to small hospitals is exemplified n the new Bucks Infirmary, intended for fifty-two beds; the first-floor plan and the south elevation of which are engraved in Miss Nightingale's volume. Apart from the thorough ventilation obtained, the plan recommends itself for large hospitals by the opportunities it affords to classify patients according to the special requirements of their maladies by securing complete isolation, and by the ample area it demands, whereby free exposure for all parts to light, and air, and space for exercise are fully attained.

The disposition of the pavilions side by side, at intervals of at least twice their height, and connected only by a corridor on the ground floor, leaves them exposed to the full play of the surrounding air, and permits the placing of opposite windows along their entire length, so as to effectually light and ventilate the wards. "Having windows only on one side, or having a closed corridor," appears as one among the leading defects pointed out by Miss Nightingale in hospital construction.

"Every ward," she writes, "must have direct communication with the external air by means of a sufficient number of windows on its opposite sides, and every ward must have its own ventilation distinct and separate from that of every other ward; [hence] it follows that to have a dead wall on one side, or to cover one of the sides by a corridor, is directly to interfere with the natural ventilation of the ward. To join all the ward doors and windows on one side by means of a corridor is much more objectionable than even to have a dead wall, because the foul air of all the wards must necessarily pass into the corridor, and from the corridor into the wards indiscriminately. The whole hospital becomes in this way a complicated ward. . . . . A similar objection exists against connecting two wards by a door." (p. 40.)

At St. John's Hospital, Brussels, the corridor uniting the pavilions is attached not only to the ground, but also to the first floor, and therefore operates injuriously in intercepting the free circulation of air between them. Uytterhoeven, moreover, asserts that it is so built against the pavilions as to form a sort of "common conduit where the vitiated air from the several wards may intermingle and suffer detention in its passage externally." So far, therefore, the advantages of the pavilion plan on which this institution is constructed, are sacrificed. M. Trélat, in his concluding discourse ('Discussion,' p. 122), referred to the Lariboisière Hospital unfavourably, and criticised the pavilion system as represented in its construction. To quote his words:

"Isolated pavilions, as at Lariboisière, were but lately considered the best type, the *ne plus ultra* of hospital architecture. But this type has not found a single advocate amongst us. On the contrary, all have been impressed with the necessity of an ample ventilation for the courts; we give the preference to simple buildings extended in a single line, or in parallel lines widely separated. MM. Giraldès, Le Fort, Broca, and Gosselin, have expressed themselves to this effect, and, at our last sitting, M. Larrey has given us the support of his opinion."

He further added, that M. Imard has accepted these views in his plan for the new Hôtel Dieu.

A perusal of the above extract from M. Trélat's speech and the context, shows that he does not object to the pavilion plan itself, but simply to that plan as carried out at the Lariboisière Hospital, where unfortunately the intervening courts between the several sections (themselves too large) are found to be considerably too narrow, and to assimilate the whole building to several rows of narrow streets. In fact, in a following paragraph, he referred to the Vincennes Military, and to the Malta New Hospital, both essentially constructed on the pavilion system (as may be seen by the drawings in 'Notes on Hospitals') as illustrations of the arrangements he advocated.

Dr. Bristowe and Mr. Holmes advance, in some degree, views adverse to those quoted from the other writers above noticed. They regard the pavilion plan of construction with no admiring eyes, and are apologists for wards ventilating into corridors, and for the arrangement of hospital buildings around closed squares. Their reasons for differing from other writers on these matters are of the negative class; they have no evidence, no knowledge that the plans of construction objected to are objectionable; for the results of hospital practice, as collected by them, are much alike, whatever plan is followed. Such is the general line of argument put forward; but an examination of some of their statements is desirable. The disposition of wards around the three or the four sides of a square, especially the latter, they write, has "been severely criticised, but the severity appears quite undeserved. No hospital in the world (as far as our knowledge extends) can show better results of practice than Guy's, or treats cases of more gravity, and no hospital appears to fulfil the objects of its foundation better than Haslar." And, touching the question of corridor hospitals, they remark, "We cannot say the inquiries we have made tend at all to support the idea that a hospital with a corridor is, on that account merely, less healthy than one without a corridor, always provided that the corridor itself is well ventilated, that the wards have direct ventilation into it, and that the latter are well constructed." Hence they "venture to believe that the objection to a hospital constructed with corridors has been somewhat exaggerated." At the same time, they allow that, "if the corridor be placed in the centre of the building, it is more difficult to establish a satisfactory ventilation. . . . . It is much more desirable to place the corridor at one side of the building, so that only one set of wards open out of one side of it." (Report, p. 494.) They further admit that, "where the wards are small and close, and the corridors are long, narrow, irregular in direction or shape, and ill ventilated, we believe it to be true that they do oppose a very serious obstacle to the free ventilation of the wards, and have a direct tendency to the production of hospital diseases, and these defects of construction may, we believe, be traced to have had some such influence in hospitals like those of Leeds, Lincoln, Manchester, &c." (p. 494.)

There is little force of argument in all this to prove corridors not objectionable. If too long and irregular, their ventilation is said to be difficult, and their presence admitted to be prejudicial. The question then arises whether these evils can ever be entirely got rid of when such corridors exist. Reflection suggests, in reply, that wards with such appurtenances can never be so thoroughly ventilated as those without them. They necessarily cut off more or less light from one side the wards, and lessen their cheerfulness; they must act as common shafts for the intermixture of the air of different wards, and any accidental nuisance arising in one ward is conveyed, by means of the connecting corridor, to every other.

The practical conclusion is that, unless some very cogent reasons can be found to the contrary, corridors of the character in question should be avoided. Some such reasons Messrs. Bristowe and Holmes believe may be found in certain advantages they presume them to possess:

"The plan of building (they say) is suitable either to large or small wards, whilst that which insists on each ward having opposite windows and no communication with the corridor, is only possible for comparatively large wards; it adapts itself to all varieties of shape, while the former (?) is adapted almost exclusively to the two shapes of which examples have been just given, and the various parts of the hospital can be arranged in any proximity to each other that may be considered desirable, while in the other plans the chief parts of the hospital must be separated by the whole length of the connecting part, if the principle is rigidly carried out." (p. 493.)

This extract repeats what, in a preceding page (p. 491), is urged as one of the two material objections to the pavilion plan—viz., "the distance of parts of the hospital from each other;" the other one being "the costliness of the construction."

Now it must be granted that the adoption of the corridor system of construction, leave being given to double the line of buildings on itself, so as to form a square, or three sides, or even only two sides of a court, would much concentrate the hospital population and bring it within easy access. But would the gain in this one particular counterbalance the evils of concentration and of ward corridors? In our opinion, which happily coincides with that of most recent authorities on hospital architecture, it would not, but, on the contrary, would be dearly bought. Again, if instead of doubling the building with its corridor on itself, the wards were all arranged in line, as at the Victoria Hospital, Netley, the distance to be traversed in order to visit the same number of patients would actually be greater than if the pavilion plan were adopted. Thus, at Netley, we find nine contiguous wards, each forty feet by twenty-five feet, containing nine beds apiece, on a floor opening into a corridor 340 feet in length, exclusive of sixty feet representing the space occupied by the medical officer's room, the principal ward master's apartments, and the waiting-room. Now, supposing the pavilion plan to be substituted, a corridor of communication 280 feet long would suffice for four pavilions of the regulation dimensions (Parkes, p. 299)—viz., eighty feet by twenty-five feet, separated by spaces sixty feet in width, each to contain twenty beds, on the same floor.

In this calculation we have supposed the wards placed at right angles to the corridor only on one side; but were the plan pursued in the Herbert Hospital followed, and the wards constructed on each side the corridor of communication, double the number of patients would be brought within reach by the length of corridor named. On the other hand, a similar disposition of wards on both sides a corridor like that at Netley, would be in contravention of acknowledged sanitary principles. The same may be averred of any scheme to enlarge the existing wards of the Victoria Hospital, for these constitute a continuous suite of rooms, deriving light and air only at their opposite ends, and this only indirectly at one end, on account of its intersection by the corridor.

In fine, we regard the objection to the pavilion plan, that its several sections are inconveniently remote, to be as invalid as it would be deficient in practical importance, even could it be substantiated. It is a plan that will, we are persuaded, bear favourable comparison, even in this minor particular, with any other that duly arranges for the perfect ventilation and satisfactory lighting of a hospital in all its parts.

The supposed inapplicability of the pavilion plan to small hospitals is shown to be a groundless objection by such examples as theBuckinghamshire Infirmary. Its non-adaptability to small wards is another objection of no greater validity. In the first place, small wards are objectionable, and their number should be reduced as far as consistent with the requirements of a hospital. Dr. Bristowe and Mr. Holmes write on this subject thus: "In hospitals divided into a large number of wards, containing a few beds each, there can be no efficient supervision of the patients, since the number of the nurses is usually below that of the wards." (p. 500.) A few pages further on (p. 505) they proceed to say: "This construction is, we think, almost fatal to efficient ventilation. These small rooms can never be kept free from draughts, except by excluding all free ventilation . . . . (p. 506) and if the hospital be constructed to contain acute cases, each ward must have its own water-closet and scullery in its immediate neighbourhood." The only small wards they propose as desirable are those "for delirious patients, for surgical operations of peculiar gravity, and other special cases. But apart from these it is, we think, an almost universal opinion that, for large city hospitals, large wards are better than small." However, in county infirmaries, where the population occasionally falls very low, they suggest that wards of the usual size would look cheerless, and become cold for the few inmates—a contingency, by the way, readily obviated by a proceeding in itself highly advantageous in every hospital—viz., by disusing for a time certain wards. From these extracts it may be fairly assumed that the writers in question are adverse to small wards, and would keep their number at a minimum, just as other writers recommend. At the same time, they hold "that separation wards, if not absolutely a necessity, are at least extremely useful, and the advantages of having them close to the main wards are great" (p. 507); such wards containing "one, or at the most two separate beds" (p. 508), being mainly eligible "for the separation of cases of erysipelas and others, which might probably infect the wards." Now this class of small wards, or rather chambers attached as offsets to the principal wards, are certainly as adaptable to pavilion as to corridor wards. There would, likewise, be no difficulty in arranging for those other small wards indicated as desirable for certain surgical operations, and for delirious cases, on the pavilion principle of construction. For supposing no better place to present itself to the architect in making his design, the staircase of a pavilion offers an eligible point in regard to ensuring isolation and ventilation for the attachment of a small special ward. So, also, does the corridor of communication where the pavilions are situate on one side, since a small ward could most advantageously be built on the opposite side at any point where its presence might be thought best.

Miss Nightingale did not lose sight of the occasional need for small accessory wards. She remarks:

"Where small wards for special cases are required, they can always, as a point of construction in small hospitals, be attached to the pavilion . . . . In good existing examples, small wards have been thrown out separately from the staircase . . . . . . But in large hospitals the smaller class of special wards should always be grouped together, and completely separated from the other wards, because they are intended to contain either the most dangerous or important cases, or noisy cases, or cases with offensive discharges, which it is always safest to remove from the general wards; besides, small wards require, if possible, purer air than larger wards, and therefore more care in construction; and in order to insure those cases which really require most nursing from neglect, they should always be placed under a completely appointed staff of their own, and *not* attached one to each large ward, which renders proper attendance extremely difficult." (pp. 63, 64.)

Sufficient, we think, has been said in reply to the assertion, that the pavilion plan "is only possible for comparatively large wards." We have shown, likewise, that it offers no impediment to the construction of accessory small wards for special cases; yet we must frankly admit that it cannot "adapt itself to all varieties of shapes" of wards and other hospital buildings, as we are told the corridor system can do, for the pavilion plan loses its distinctive merit when the arrangement in right lines is departed from, and must cease to exist when blocks of building are massed together in squares, or form a congeries of intercommunicating rooms.

The costliness of construction is the only remaining objection preferred by the official reporters against the pavilion plan. Miss Nightingale treats the question of cost, when raised in discussing plans of hospital architecture proposed for the good of the sick, as a well-nigh irrelevant one. She remarks:

"I submit that this is not the question before us, which is, how to construct a hospital with the requisite facilities for ventilation, administration, nursing and health. The mode of construction of hospitals is, it is presumed, to be determined by that which is best for the recovery of the sick. If any other consideration is taken, such or such a per-centage of mortality is to be sacrificed to that other consideration. But it so happens that the safest for the sick is in reality the most economical mode of construction." (p. 59.)

But as all have not like faith, especially with respect to the direct and definite benefits accruing to hospital patients from attention to structural details, so unflinchingly asserted by Miss Nightingale, it is desirable to take lower ground than that writer has done, and to discuss the question of the relative cost of pavilion and other hospitals. In so doing, we shall find valuable aid in Husson's work, and in Blondel and Ser's Report (op. cit. p. 135 et seq.). The subjects of comparison in Messrs. Bristowe and Holmes' Report are St. George's Hospital and the Lariboisière Hospital. The cost of the former for 320 beds is put at 50,000*l.*; that of the latter, for 606 beds, at 285,000*l.*, "so that (they proceed to remark) we may say that two such hospitals as St. George's could have been erected to accommodate 700 patients, at less than half the cost of Lariboisière to accommodate 600. This is the expense of the pavilion plan as compared with the H shape." Now, were these figures accurate, and did they apply to buildings similar in character, purpose, and constitution, they would carry the conviction with them, as they appear to have done to the English reporters' minds, that the pavilion plan is so costly as to render it inapplicable to hospitals, except such as are overburdened with wealth.

But we submit that the case of the two establishments is not accurately stated. A comparison is instituted between the two as alike in their general constitution and in the circumstances of their construction, when, in truth, there is little else in common between them than that they both bear the name of hospital. The sum of 6,655,215 francs put down as the cost of the Lariboisière includes the outlay for a multitude of structures and fittings not to be found in St. George's Hospital; and, on the other hand, it is very certain that the cost of the whole of the buildings and fittings of the latter, in its present condition—in that, that is to say, in which it is contrasted with the former establishment in its finished state—is not included in the original estimate of 50,000*l.* (i.e., 1,250,000 francs.)

Before committing to print the note on the comparative cost of the two hospitals, and drawing an inference from a particular institution well known to have been extravagantly built, so unfavourable to a special plan of construction, Messrs. Bristowe and Holmes were undoubtedly bound to have noticed the wide differences obtaining between the two institutions. Had they read the accompanying remarks of Husson (whose work they refer to for their figures) the necessity of their so doing would have been impressed upon them ; or even had they looked through the items presented in detail by that writer, the exceptional nature of a large number of these would have convinced them of the unfairness of their taking the cost of the Lariboisière Hospital as the standard of reference for estimating the cost of the pavilion plan of construction.

Husson writes thus:

"But it must not be lost sight of that the total cost comprises, besides that of the pavilions specially intended for sick, that also of the erection and fitting of numerous buildings constructed for the general services of the hospital; and offices of administration, chapel, apartments for the nursing sisters (religieuses de l'ordre de St. Augustin), pharmacy shops and laboratories, kitchen, laundry, wash-house, amphitheatres, dead-houses. The difference accruing from these various services may, as must be conceived, be great or small, according to the dimensions of the buildings, the nature of the materials employed, the architectural decorations, and the degree of completeness and finish aimed at. We make this remark, because it often happens that in comparisons made concerning the cost of hospitals, this essential element in the evaluation is neglected, and hospitals provided very largely with 'general services' are contrasted with others in which those services are very restricted."

A glimpse at the items of expenditure on the Lariboisière Hospital shows the justice of Husson's remarks, and also how many of those items represent architectural details forming no essential or necessary elements of the pavilion plan. A notice of some of these is required to make good our assertion. Now, the first item in the list—viz., foundations and masonry, is marked by very exceptional features, particularly when the cost of St. George's Hospital is brought into comparison. In the first place, the site of the hospital was over an ancient quarry, filled in with rubbish, and its preparation and consolidation for building upon and the extra foundations requisite for the stability of the edifice, caused a very heavy expenditure. Again, as M. Blondel informs us (p. 136), according to the original design, only 400 beds were contemplated, and when it was decided that the number should be raised to 600, the foundations of the pavilions had to be altered, and an expense of 500,000 francs thereby incurred. In the course of erection, the surrounding space for exercise was found too small, and it became necessary to pull down and to re-erect the boundary-wall at a cost of 87,373 francs. The whole building is constructed upon arches, forming vaults beneath, and is built of hewn stone, with great attention to "finish" and architectural adornment. Sculpture figures in the list of costs for 20,509 francs, and stained glass for the chapel, for 10,206 francs.

If we next examine the apartments provided, we shall find how widely this grand institution differs from the London west-end hospital with which comparison is challenged. There is a magnificent chapel with costly fittings and decoration for Divine worship, such as St. George's Hospital does not, unfortunately, possess; two spacious amphitheatres for clinical lectures and operations, and two large rooms for autopsies; two large bath-houses, fitted with all sorts of baths, each house having baths for some twenty patients at once; a large "community house" for lodging the sisters of St. Augustin; stores for clothes, stables and coach-houses; an extensive laundry and wash-house, with their expensive fittings, drying closets, &c., sufficient to wash the clothes not only of this great hospital, but of other establishments also; and lastly, eight very spacious rooms interposed between the several pavilions, of which six are used as dining or day-rooms, and two as libraries. Besides all these, it must not be forgotten that the buildings forming the façade of the hospital, 530 feet wide, with projecting pavilions on either side the entrance court, and altogether well-nigh equal in dimensions to St. George's Hospital, are occupied by the residences of the officers and servants of the establishment, by the kitchen, dispensary, and pharmaceutical laboratories, with their accessory apartments, and by admission and out-patients' rooms. How many, then, are the apartments having nothing akin in St. George's Hospital, but yet of heavy cost in construction? And even among those not peculiar to this French hospital, how different is their relative development found when a comparison is drawn between the London and the Paris hospital in question!

The scant proportions of the apartments in English hospitals set apart for the "general services," is particularly remarked upon by M. Blondel, in his comparative review of London and Parisian hospitals. He was particularly struck with the simple and inexpensive arrangements and smallness of the kitchens and dispensing departments; with the absence of those splendid and well-kept linen and clothing stores which arrest the attention of every visitor of foreign hospitals, and with the great deficiency of baths considered as important means of treatment. (pp. 114, et seq.)

These remarks on some of the accidental expenses in construction of the Lariboisière Hospital, and on the great disparity in kind and degree of the accommodation furnished in it and that to be found in St. George's, or indeed in any English hospital, suffice to show how groundless is the inference which the English official reporters would draw respecting the expense of the pavilion system, and how unfairly they have put the case before their readers. And we might, indeed, strengthen our assertions by a further examination of the items of expenditure of the Lariboisière Hospital, by showing that the total sum mentioned includes the cost of planting and laying out the courts and gardens and paths—above 2132*l.*; of warming and ventilating apparatus and ovens, 17,060*l.*; laundry, 2320*l.*; commission, 10,680*l.*; heavy sums, which would not be found to figure in the 50,000*l.* at which the contract for St. George's was taken.

Blondel (p. 137) has gone into the question of the cost of the Lariboisière very fully, and calculates that each pavilion, considered apart from the several offices, costs at the rate of 4000 francs, or 160*l.* per bed. Taking the figures as they stand in the English Blue Book, St. George's Hospital contains 320 beds, and cost 50,000*l.*; consequently, each bed cost 156*l.*,—a mere trifle less than in the Paris hospital it is contrasted with. We do not insist upon the comparative estimates as in either case accurate; for it is probable that, could the cost of a ward and its accessories at St. George's be discovered, it would prove less than the sum per bed as stated, in which is included the expenditure on the general offices of the institution. However this may be, M. Blondel demonstrates at how greatly less an expenditure wards on the pavilion plan may be constructed, by citing those recently built on that system at the Hôpital St. Antoine, for 2200 francs per bed; those at Necker Hospital, for 1800 francs; and, lastly, those at the Hôpital Beaujon, for 1500 francs, or 60*l.* per bed. On the other hand, he has adduced, by way of contrast, the expense per bed of English hospitals, stating that at Guy's it amounts to 200*l.*, and at the new King's College Hospital to 400*l.* per bed. Referring to the cost of the last-named charity in reference to that of the Lariboisière, he sarcastically remarks, "we do not think any person would desire to compare these two establishments in what relates to their general sanitary conditions, the grandeur of their internal arrangements and provisions (*le grandiose de l'installation*), and the magnitude of their accessory services." Lastly, he arrives at the conclusions—that the English hospitals, as a whole, cost as much in construction as the French, and that those parts of them exclusively set apart for the sick are cheaper in French than in English institutions.

The result of this discussion concerning the cost of the pavilion plan as compared with other principles of hospital construction, is, that Dr. Bristowe and Mr. Holmes have failed to prove it much greater, and that, on the other side, from the French examples quoted it may, to say the least, be accepted as equally cheap. But should it be made out distinctly that the pavilion plan is a more expensive one than any other hitherto carried out, still the very superior sanitary advantages of that plan should, in the interests of the sick, suffice to recommend it. That such advantages are secured by it is not denied even by those official reporters; and, on the other hand, no valid argument against it is advanced by them.

Their reasoning on the merits of other hospitals as compared with those erected on the pavilion plan, appears to us to be one-sided and fallacious. They contend, generally, that because our old hospitals have done good service, though occupying town sites and not pavilion structures, therefore country sites for them and improved construction are not required; the advantages, moreover, of such a change not being to them apparent from the statistical inquiries they have instituted. But of these statistical inquiries, it must be observed, they are inconclusive and of little value in determining the point at issue: the circumstances represented by figures are not sufficiently analogous, and no standard is offered for judging of the good results an improved construction and a country site might produce. The maxim they approve is, "Rest and be thankful." They appear to dread innovation upon the existing state of things for the admitted shortcomings of which they are apologetic; they view changes esteemed by others improvements, with suspicion and doubt, and find numberless impediments to realizing them; and, lastly, they often take refuge in the extraordinary plea, that the prevalence of a plan or system, though condemned by others, is evidence *per se* of its excellence.

Review II.

1. *Quelques Mots sur l'Acide Lactique et les Lactates Alcalins et Terreux, sur leurs rôles dans l'Economie Animale et Specialement dans l'Acte de la Digestion.* Par M. Burin du Buisson.

A Brief Notice of Lactic Acid and of the Alkaline and Earthy Lactates, and of their Action on the Animal Economy. By M. Burin du Buisson. (From the 'Gazette Hebdomadaire de Méd. de Paris,' Juin, 1862.) pp. 8.

2. *De l'Emploi Therapeutique des Lactates Alcalins dans les Maladies Fonctionnelles de l'Appareil Digestion.* Par J. E. Pétrequin, Professeur à l'Ecole de Médecine de Lyon, &c. Deuxième édition.

On the Medicinal Use of the Alkaline Lactates in Functional Derangements of the Digestive Organs. By J. E. Pétrequin, Professor, &c.—*Paris,* 1864. pp. 23.

These two pamphlets are designed by their authors to make better known the use of the lactate, chiefly the alkaline, in morbid affections of the digestive organs. Their views of the efficacy of these salts are founded on physiological principles, and are confirmed and verified, they confidently assert, by a large clinical experience. As the subject is comparatively novel, and one of much promise, we shall give the principal conclusions which the author of the first pamphlet thinks himself justified in making in favour of the medication which he advocates. He promises in another work a full account of that which he now gives summarily:

1. That in principle all the functions of the animal economy may be considered of two kinds—one acting by excess of acid, the other by excess of alkali, two acids only concurring to the maintenance of life—the lactic secreted in the economy, the phosphoric derived from the food.

2. That the quantity of lactic acid normally secreted in the twenty-four hours in the stomach of the adult man of the average weight of 75 kilogrammes, supposing this acid to be of 30° of Baumé, and in the proportion of 1 per cent. of the gastric juice, is about 75 grammes—a quantity to which additions are made by the acid which is formed in the duodenum, jejunum, and small intestine.

3. That in health this acid is the *normal* one in the gastric juice, to the absolute exclusion of the hydrochloric, or any other acid, whether organic or mineral.

4. That the lactic acid occurs alone in the gastric apparatus, whilst in the solid and organised parts of the economy it is found conjointly with free phosphoric acid; whence it may be concluded that the presence of free lactic acid in our organs is indispensable for the production of the mixed chemico-physical phenomena essential to vital action.

5. That the young mammal has in milk the source of this acid, so indispensable to its economy and development, and in sufficient quantity; and that later, when weaned, it finds the same acid in the albuminoïd matter which should form the chief portion of its nutriment, and which, like milk and feculents, is equally liable to undergo the lactic fermentation.

6. That the lactic acid procured from flesh and its juices, is identical in its properties with that contained in the gastric juice, and yet differs as regards its salts, whence we are led to believe with M. C. G. Lehmann that the free acid of muscles, and of their juices, is derived from the muscular fibre itself, under the influence of the physical functions of these organs.

7. That the free acid of the stomach, besides the action which it exerts on albuminous matters, facilitates in a remarkable manner, by endosmosis, the absorption of chyme, and its passage into the alkaline blood and lymph.

8. That by the notable quantity of lactates, alkaline and earthy, with a base of soda, potassa, ammonia, magnesia, and lime, which it gives rise to, and which are found in all the active fluids of the economy—the saliva, the gastric juice, the chyle, lymph, blood, the humours of the eye, &c., it facilitates by a special action the selection and separation of the four orders of aliments[1] one from the other; and that, when taken into the blood-current, these lactates become a powerful source of heat by the combustion of their acid, whilst the alkaline bases, soda, and potash, contribute to the alkaline quality of the same fluid.

9. That the bicarbonate of soda and the carbonate of magnesia, as given in small doses in aid of digestion, are inoperative until they have passed into the state of lactates of soda and magnesia.

10. And that even magnesia, when administered as a purgative, does not, according to the observation of M. Mialhe, take effect until converted in the stomach into a lactate.

Such are the conclusions from which M. du Buisson considers himself justified in expressing his belief that the introduction of the lactates constitutes real progress in therapeutics, and that the healing art possesses in them a precious means of relieving a vast

[1] 1. Proteic, so termed (albumen, fibrin, casein, &c.)
2. Amylaceous (starch, gum, sugar).
3. Fatty (oils, fats, butter, &c.)
4. Mineral (earthy phosphate, alkaline chlorides, &c.)

variety of ailments which too often are viewed with despair equally by the patient and practitioner.

We shall now briefly notice Professor Pétrequin's publication. His experience of the mode of treatment which he has introduced extends, he assures us, over twelve years, and that with every increase of it his confidence in its efficacy has strengthened, as has also, he says, that of his *confrères*, who have given it a trial.

We need not enter into the particulars which he recounts of the trials made by him of the different lactates, or further notice the general principles on which he employed them, the latter, with slight modifications, being much the same as those which we have extracted from M. du Buisson's pamphlet; who, indeed, seems to have been indebted for them to Professor Pétrequin He (Professor Pétrequin), however, it may be right to mention, is not so exclusive in considering one acid only as concerned in digestion. He admits that the hydrochloric may also take part in it. The lactates to which he finally gives the preference are those of soda and magnesia, or rather a double salt, composed of the two, which at his desire M. du Buisson succeeded in forming. It possesses the valuable property of not deliquescing like the lactate of soda, on exposure to the air, or of undergoing any change from exposure, and is fit to be given in the form of pastil, or powder. In its dried and pulverized state it is white, of extreme tenacity, with only a just perceptible odour of lactic acid, and a slight saline alkaline taste, with a faint bitter after-taste — qualities which our author thinks of high value, and strongly recommending it for use.

Before treating of the ailments for which he prescribes this salt, he dwells on the peculiar advantages which the lactic acid possesses over every other acid; and first and chief, that the stomach and intestines are able to elaborate it from the materials essential to life—namely, the aliments themselves; next, that as an organic acid, it is readily decomposable and eminently combustible. He observes that to appreciate duly these qualities we should keep in mind the enormous quantity of the gastric juice which is indispensable for digestion, and how, in this process, the acid, or rather its alkaline salts, are decomposed, the excess of alkali being excreted by the kidneys and skin, and the acid itself, except that portion of it which is retained in the muscles, furnishing combustible elements, carbon and hydrogen, to the blood, and becoming a source of animal heat.

The ailments connected with digestion he does not, as of old, view as originating solely in the stomach. He treats of them under buccal, gastric, and intestinal derangements.

Under the first he notices, 1st, an acid state of the saliva, co-existing with difficult digestion, alteration of the teeth, and bad breath. In cases of this kind he has prescribed with advantage from one to two or three pastils of the lactate of soda and magnesia, directing that they should be left to dissolve in the mouth, and not broken by the teeth. 2ndly, he adverts not to a vitiated quality of the saliva, but to a deficiency of its quantity, producing what may be called a dry dyspepsia from insufficient salivary secretion, and atttended with laborious and imperfect digestion, a dryness of the mouth and fauces, with eructations, &c. In such cases he gives from one to two or three pastils of the same salt before each meal, with the same instructions, so as to promote the flow of saliva, and orders the repetition of them after food. He assures us that this mode of treatment has often been so beneficial and in so short a time as to have exceeded even his expectations.

The gastric digestive arrangements are less limited; he specifies four: 1. Acid dyspepsia, an aggravation of the former, often accompanied by pyrosis. For this he prescribes a dose of lactate of magnesia (thirty centig.) and a saccharine lactate of soda (twenty centig.) to be taken before a meal, presently followed by one or two pastils of the lactate of soda and magnesia. 2. Flatulent dyspepsia, with eructations, sometimes inodorous, sometimes acid, occasionally fœtid; the first two commonly connected with imperfect digestion of flatulent food, especially farinaceous. The treatment advised is much the same as the preceding, with this difference, that if the flatulency is experienced some hours after eating, the lactate should be used, not before, but after, the meals. 3. Dyspepsia with gastralgia or gastrodynia. This variety of the malady is also efficiently treated by the lactates; they should be administered whenever the stomach becomes the seat of pain after taking food, or in some cases should be continued daily until the malady is relieved. 4. Neutral dyspepsia, so designated by our author, in which there is neither excess of acidity, nor any of the preceding complications, but a state of atony with a feeble and altered digestion. The evacuations are fœtid, and the patient from defective nourishment becomes emaciated and loses strength. In such cases he considers the gastric juice to be in fault, either deficient in quantity, or altered in quality. When of the first kind, stimulation of the secreting organs is requisite, and may be effected by the alkaline lactate given in the form of powders and pastils. When of the second kind he has recourse to pepsine in conjunction with the lactates; these two being helpmates and essential to healthy digestion. He prescribes them in the form of pastils composed of ten centigrammes of the former and five of the lactate of soda and magnesia, and often with the best effects.

The functional derangements belonging to the third, the intestinal digestive process, are more obscure than the preceding—that is, physiologically considered, though their symptoms are sufficiently manifest. Their origin he attributes for most part to diet, especially of farinaceous articles, liable to accumulate and load the intestines. As the lactates in a large dose are aperients, they are peculiarly suited for this form of ailments.[1]

In his concluding observations the author takes a wider view of dyspepsia, justly remarking that, though he has treated of it locally, and that it is often merely a symptom, or a functional derangement, yet, if long continued, it may terminate in organic lesion. His advice is, that besides local, it requires general treatment, directed to the specialties of each case, with particular attention to all those circumstances likely to conduce to healthy digestion. His first indication is to reform the hygiene when depraved, to regulate the regimen when not in accordance with the precepts of science; when the meals have been irregular, to insist that they be taken at fixed hours; to direct attention to the due mastication of the food; to prohibit the use of tobacco, &c. He adds, moreover, that it is necessary to combat pathological complications by suitable means; for instance, the chloropathic, by preparations of iron and manganese; enervation by quinine; general asthenia by tonics and restoratives; and the rheumatic diathesis by mineral waters, which may be called into aid in a variety of other cases.

As our object is to make our readers acquainted, so far as our limits permit, with this new mode of treatment of a very important and too prevalent class of disorders, we shall pass no judgment on it or on the physiological views on which it is founded, yet we have no hesitation in expressing our opinion that it is deserving of trial: we are cautious of saying more, keeping in mind that, though the physiology of digestion is in some degree advanced, it is far from perfect, especially in what pertains to the acid in question. We have spoken of the treatment as novel; perhaps it would be more correct to designate it a renewal, in a more elaborate and definite form, of that inaugurated by Magendie some thirty years ago, and now almost forgotten.

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[1] The following formula is given for these lozenges as prepared by M. Du Buisson:

℞ Saccharine lactate of soda	8
Lactate of magnesia	2
Amylaceous pepsin	8
Powdered sugar	61
Mucilage of gum tragacanth	q. s.

A gramme to be prepared at a time; it should be quickly dried and kept in a dry place.

Review III.

Essays and Reports on Operative and Conservative Surgery. By Richard G. Butcher, &c. &c. Illustrated by 62 Lithographic Plates, coloured and plain, and several Engravings on Wood.—*Dublin* and *London*, 1865. pp. 933.

Mr. Butcher has earned, by hard work and by valuable services rendered to the theory and practice of surgery, a high place in the profession, perhaps the highest place in the sister island—no mean distinction in a surgical school so famous as that of Dublin has long been. Moreover, the volume from his pen which we have here under review, consists for the most part of well-known matter; of matter which has been stamped with the approval of the mass of surgical readers, and has become authoritative on the subjects of which it treats. On both these grounds, therefore, we approach this volume with minds already made up as to its general merits. Whatever Mr. Butcher thinks worth publishing must be a valuable addition to our literature; and the papers on excision and on plastic surgery, of which this volume in a great measure consists, have already spoken for themselves: they have formed for a long time the chief attraction of the distinguished periodical in which they have appeared, and are now classical in the literature of surgery. If, therefore, we differ in some respects from Mr. Butcher's doctrines, we beg our readers to remark that these differences extend to minor matters only; and if the writer of the present notice has some reason to complain of haste and violence in the language which Mr. Butcher has employed while speaking of opinions expressed by him, which have had the misfortune of differing from Mr. Butcher's conclusions, he would feel heartily ashamed of himself if any such trifling motive could blind him in the least degree to Mr. Butcher's eminence in the profession to which they both belong. The reader shall soon be in a position to judge on which side of this little controversy the right is to be found.

The first topic treated of in this work is the Excision of the Knee. On this subject Mr. Butcher republishes, without any material alteration, the papers in which he first treated of this operation in the 'Dublin Quarterly Journal' more than six years ago. It does not appear that anything which has been written since the first appearance of those papers has induced Mr. Butcher to modify his main conclusions, though possibly some unimportant changes may have been made in the wording of the essays.

These papers contain the results of Mr. Butcher's own experience; but those results are really insignificant, as they extend only to five cases, all of which, however, were perfectly successful. Success in so limited a series of cases is a matter of very trifling importance in considering the comparison between excision and amputation. Many surgeons have had far longer series of successful amputations of the thigh. The main feature in Mr. Butcher's papers is the attempt which he makes to prove that excision of the knee is a milder operation than amputation, and has been more successful. We will quote Mr. Butcher's words:

"Excision of the hip, excision of the elbow, excision of the knee, are likewise formidable operations; but the latter I have proved, by lengthened statistics, and even from Mr. Syme's own showing, not to be so dangerous to life as amputation of the thigh; therefore the term 'formidable' must be taken in a restricted sense as applied to excision of the knee, but in its unlimited meaning in reference to amputation of the thigh." (p. 124.)

This conclusion is opposed to the opinion which the writer of the present article expressed some time ago in this Review,[1] and in an article on excision of bones and joints in a "System of Surgery." Both these productions fell under Mr. Butcher's notice, and he promised to confute the reasoning contained in them, but has hitherto contented himself with abusing the writer for the heresy of differing from him. Nor does Mr. Butcher take any notice of the opinions expressed on this point by Mr. Fergusson, who of all men can hardly be suspected of a desire to decry the operation of excision of the knee. Yet the operation is confessed by Mr. Fergusson to have been exceedingly fatal in his hands, and in speaking of its mortality, as compared with that of amputation of the thigh, he was fain to confess that the question is as yet undecided.[2] But let us see what is the evidence which Mr. Butcher himself adduces. He gives three tables of cases from which to calculate the mortality after excision of the knee. The first of these tables (p. 5) includes the cases operated on prior to Mr. Fergusson's revival of the operation in 1850, and more than half these cases were fatal. However, as these operations hardly come into the category of the surgery of the present day, it seems fairer to exclude them. The other two tables contain between them 82 cases; of which 15 died, and 1 was in a precarious state, with symptoms of pyæmia, at the date of the report; 7 were ampu-

1 See our number for July, 1862.

2 "I have now performed this operation forty times, and of these no less than fifteen have died. . . . . The largest number of collected and original cases with which I am acquainted was made some years ago by my friend and former assistant Mr. Price. The list amounted to nearly 250, and went to show that the fatality was pretty much the same in excision of the knee and amputation of the thigh. . . . . My impression is that excision of the knee is, or should be, by proper treatment, as little destructive to life as amputation of the thigh."—Lancet, 1864, vol. ii. p. 33.

tated, and 17 are left imperfect. These are the data to calculate the mortality of excision. Those for amputation are thus given:—At page 38, Mr. Butcher quotes from an early edition of Erichsen's Surgery the statistics which he gives of the experience of University College Hospital, and which put the mortality of amputations "for disease" at 2½ per cent., and those published by Malgaigne from the Parisian hospitals, in which the same mortality is stated at 60 per cent.

"These tables," says Mr. Butcher, "when contrasted with my second on excision of the joint, set at rest for ever the comparative danger of the two operations." (p. 38.) Now the table of excisions to which Mr. Butcher refers as setting the question at rest for ever—the first of the two summarised above—exhibits 31 cases, 5 of which were fatal, and 7 were left imperfect; but of these 6 are afterwards accounted for—5 recovering, and 1 suffering amputation. Is it possible that Mr. Butcher can wish his readers to believe that so small a number of cases can settle the question of the general mortality of an operation? Then with respect to the cases of amputation, Malgaigne's statistics are not in point, since the circumstances of patients operated on in the hospitals of Paris are quite different from those of English patients.[1] As to Mr. Erichsen's, our readers would hardly believe that his statistics of amputation are almost identically the same as Mr. Butcher's lists of excision. Mr. Erichsen's first list (the one here referred to by Mr. Butcher) gives only 27 cases, with 7 deaths, 20½ per cent.[2]—a result we should have thought pretty nearly tallying with Mr. Butcher's first table. This is in Mr. Erichsen's edition for 1853. But if Mr. Butcher would have given himself the trouble to look at later editions of Mr. Erichsen's work, he would have found that larger experience at the same hospital brings the results into still more close accordance, for in Erichsen's edition for 1863, he gives the number of cases at 49, with 9 deaths—18·4 per cent.; while Mr. Butcher's two tables give 82 cases, with 15 deaths, or 18·2 per cent. This is the minimum mortality in Mr. Butcher's list; for one case is left "in a precarious state," with symptoms of pyæmia, and doubtless died, and 17 cases are left imperfect; and of these some also must in all probability have died. Thus on Mr. Butcher's own showing, the deaths in his table are quite equal in number to those in Mr. Erichsen's; and this does not take any account of other failures not due to death—viz., amputations and useless limbs. But Mr. Butcher ought to know that this comparison between exci-

[1] In the hospitals of Paris excision of the knee has proved so fatal that, as M. Verneuil said in the recent discussion at the Société de Chirurgie, on Hospital Hygiene, it has been pretty nearly abandoned there.

[2] There is some misprint here, for the figures do not give the percentage; but it is a matter of little moment.

sion and amputation for all kinds of disease is a very unfair one, because excision is never done for acute disease, or extensive disease, or cancer, or exhaustion from hæmorrhage, or hectic, or in advanced life; while all these unfavourable causes meet together in a general table of amputations. Again, there are much better data now published for ascertaining the usual death-rate after amputation of the thigh for chronic disease of the knee-joint in London hospitals (which is at any rate not *below* the average death-rate all over the country) than those which are here quoted—viz., Mr. Bryant's statistics from Guy's Hospital, Mr. Callender's from St. Bartholomew's, and those published by Mr. Cooper, in conjunction with the present writer, from St. George's. These statistics will show that in the most favourable cases (such as are alone selected for excision), the average mortality after amputation of the thigh is much below what Mr. Butcher has here given for that after excision of the knee. Mr. Bryant's statistics from Guy's Hospital agree with those from St. George's in showing that at those hospitals amputations for chronic disease of the knee-joint (which alone can be compared with excisions) have, over a long series of years, proved fatal only in one case out of seven.

These statistics have been before the profession now for a long time, Mr. Bryant's having been published in 1859, and those from St. George's Hospital in 1861. It was therefore with much pain that we observed that Mr. Butcher had not been ashamed to republish a most intemperate and unthinking passage reflecting on the surgical practice at St. George's Hospital, which he ought never to have written, and which, having written, he ought at any rate to have taken the first opportunity to expunge. Mr. Butcher quotes (on p. 131) a series of 12 cases of amputation of the thigh performed during a single year (1856) at St. George's Hospital, on account of different kinds of disease; in some of which cases excision might no doubt have been performed, had the judgment of the surgeons led them to recommend the operation, while in other cases that operation would have been impossible. Six of the cases died. Mr. Butcher makes the following remarks:

"How satisfactory this return" (i.e., one of seven amputations after excision, with only one death) "as contrasted with the result of the wholesale lopping-off of limbs practised in one of the largest hospitals in London—St. George's Hospital."

Then follows the table of 12 cases:

"In this table, 12 cases of amputation of the thigh are recorded, and 6 of them died; while one of the mutilated 6 is reported only as convalescent on the sixty-second day. Such a result is very calamitous; and it is painful to look upon this table and see how many cases, apparently well adapted for excision, were subjected to the more formidable operation—amputation—terminating in death. I may well repeat, there is a wholesale lopping-off of limbs for 'abscess,' and 'ulceration of the cartilages' of the knee-joint exemplified here." (p. 132.)

On this head, all that the present writer need say is, that the surgeons of St. George's Hospital have published the results of their amputations for a period of seven years,[1] and that these results show that their practice in amputation of the thigh for chronic disease is much more successful, as far as danger to life goes, than any results of excision of the knee which either Mr. Butcher or any one else has published. They can, therefore, afford to treat Mr. Butcher's criticism with indifference. Every one knows that in a long series of operations unsuccessful cases will often come close together. Mr. Butcher has chosen to publish a fragment of a larger table when the whole has been accessible to him for several years; and he has done this with a view of casting obloquy on surgeons whose practice he has had no opportunity of seeing, on grounds which prove on examination to be fallacious, and in language hardly consistent even with the dignity of his own position. It is difficult, indeed, to reconcile such conduct with the high honour and gentlemanly bearing that should characterise the leaders of our profession.

If now our readers will turn to the article above referred to in our number for July, 1862, and to a review of Dr. Hodges's work on 'Excisions' (January 1863), they will see that the larger collection of cases made by the latter gentleman[2] raises the percentage of deaths to about 1 in $3\frac{1}{2}$ cases, and that of failures of all kinds to about one half; and that about the same proportion seems, as far as can be ascertained, to have prevailed in the London hospitals.

The above facts, and even Mr. Butcher's own statistics, without the above facts which he has chosen to ignore, amply confirm the assertion made in the article on Excision of Joints in the 'System of Surgery,' that "the acceptance which this operation" (viz., excision) "has met with is not due to its lower rate of mortality." In fact, there is little doubt that the rate of mortality has been far higher than it would have been if amputation had been performed in the same cases. At the risk of being tedious the present writer cannot avoid adding that he insists on this fact from no hostility to the operation; on the contrary, in his own practice he has never lost an opportunity of performing it, and in children has found it a very successful operation. But

[1] Med. Times and Gazette, April 6, 1861.

[2] And we may remind our readers that this larger collection of cases is precisely similar to, and rests on precisely the same authority as, Mr. Butcher's shorter series—each being the most complete collection which the authors could make of the cases published in books and journals, and reported chiefly by ardent advocates of the operation.

even in the interests of scientific truth it would be important that surgeons should have a more accurate idea of the risks of the operation than Mr. Butcher's rose-coloured tableau would give them; yet this is not the most important side of the question. It must be distinctly understood, as a matter of practice, that excision of the knee, as it offers greater advantages than amputation, so also it involves greater risks, and therefore must be reserved for the more promising cases of chronic disease only, and mainly for young persons. It would be difficult to understand from Mr. Butcher's writings why excision should not be practised in every incurable case of disease limited to the knee-joint. Yet this is a practice which not even he would recommend. Another drawback to the universal applicability of his favourite operation to which our author persists in shutting his eyes, is the check to the growth of the limb which sometimes follows excision in children. It is amazing, at the present day, to find a surgeon like Mr. Butcher complacently assuring his professional brethren, "In my former essay I proved, in several instances, *that the growth of the limb was not checked by excision of the joint in childhood;* and am happy now again to confirm this most important fact" (p. 125). (The italics are in the original.) Surely every one, except we suppose Mr. Butcher, knows that this depends on the extent of bone which it may be necessary to remove, and that when the incisions implicate the diaphysis of the bone, the growth of the limb is sure to be checked. We must say, however, that our own experience leads us to subscribe to Mr. Fergusson's and Dr. Humphry's recorded opinion, that this is not a matter of such extreme practical importance as has been assumed, for the limb may be very useful even when considerably shortened. Thus in the case of a lad on whom the writer operated several years ago, the shortening has progressively increased since the operation; yet the limb is a most sound and useful one, and the lad would scout the idea of comparing it to a wooden leg. Again, in a case seen by the writer at Jersey, where the operation had been performed by the late Mr. Jones, the bones are bowed outwards at the line of union, and the limb is nine or ten inches shorter than its fellow; yet the patient prefers it to a wooden leg.[1]

[1] In controversies about excision of the knee a great deal has been said as to the wonderful success which attended the late Mr. Jones's operations. Being at Jersey last summer, the writer was anxious to see some of the limbs which had been preserved by conservative surgery. By the kindness of Mr. Godfray (Mr. Jones's successor at the hospital) he was enabled to see two patients, who were the only ones that Mr. Godfray could then find, on whom Mr. Jones had excised the knee. One of these patients, as stated in the text, had a bowed limb, nine inches shorter than its fellow, but still useful. The other had a limb ankylosed at right angles, with the foot suspended in the air, and far less useful than a wooden leg. Mr. Godfray knew of two others of Mr. Jones's patients in whom the operation had turned out better,

Another point on which the present writer has expressed an opinion which has had the misfortune to displease Mr. Butcher, is as to the advantages of the saw which goes by Mr. Butcher's name. Where it is necessary (if it ever is) to give to the surface of a sawn bone an angular, polygonal, or curvilinear shape, Mr. Butcher's saw may be a very useful instrument. But in an operation like excision of the knee, where it is wished to oppose two perfectly smooth plane surfaces to each other, most people would prefer a common saw; and such seems to be the practice of the great majority of operators. The matter is surely too trifling to require discussion, and we should have thought too trifling to excite any feeling even in the mind of an inventor. Nor is Mr. Butcher at all insensible to the duty of courtesy in discussion, for he complains of others when they are apparently deficient in this respect towards himself. Thus, speaking of the great Edinburgh professor and his doctrines on the compression-treatment of aneurysm, Mr. Butcher says, "Mr. Syme's views are thus graciously and modestly expressed: 'So long as it is my sincere persuasion that ligature of the artery is preferable to pressure, I shall deem it my duty to pursue this method, *though it may not perhaps be the best suited for the lowest capacity of surgical skill.*'" And again: "Let every man act according to his powers; *but let no one who feels it necessary to choose inferior means throw blame upon those who are able to practise a higher exercise of their art*" (p. 521). Not very modest or gracious on the part of Mr. Syme, we grant; but it should be remembered that Mr. Syme does not speak like Mr. Butcher from a personal experience of five cases. He has tied the femoral artery more than fifty times, and twenty-three times in succession without a single bad symptom, a circumstance which, if it had occurred to Mr. Butcher, would have "set at rest for ever" the question of the relative merits of the two methods of treatment. Let us see the measure of graciousness and modesty which Mr. Butcher thinks sufficient for one who doubts whether "Butcher's saw" is better than a common saw in excision of the knee. The following are Mr. Butcher's gracious and modest expressions:

"In a work recently published, entitled 'A System of Surgery, Theoretical and Practical, in Treatises by various Authors,' I find allusion made to this most useful instrument, in a feeble, meagre

and who had useful limbs; but he believed that he might safely say that not more than one in three of Mr. Jones's cases of excision of the knee was successful. In Mr. Butcher's own language, "It is too much the fashion of the day to describe bold and daring operations performed by dexterous hands, without that searching inquisitor, result, even being introduced or made mention of" (p. 437). The style (for a critic of style as Mr. Butcher professes to be) is singular, but the sentiment is a very useful one, and very applicable to this particular operation.

article, forsooth, termed 'Resection"[1]—such a production, both in style and matter, as would place a second year's student last in his class. I perceive a review of the book in the 'Dublin Medical Press' for Feb. 18th, and I will allow the reviewer to condemn the ignorance and want of information displayed in an article which should at least yield the facts and directions laid down in every text-book, and recognised by all well-educated surgeons. . . . . . The objections urged against the saw are too contemptible for me to dwell upon. Its great advantages for the purposes of resection I have pointed out in my memoirs on the subject of excisions of joints. Every modern work on surgery, and surgeons who have worked in this department, speak loudly in its praises. By a regulation of the Army Board, it must be in every case of instruments. Is not all this additional testimony in favour of my saw? Therefore, I protest against incapacity robbing the instrument of its merits." (p. 160.)

We would beg Mr. Butcher to think for a moment whether this style of language is disgraceful to the person against whom, or to him by whom, it is employed; and whether it can ever avail to settle any difference of opinion. As to this wretched squabble about "the merits of a saw," it is not worth wasting ink on. It was the sincere opinion of the writer after using Mr. Butcher's instrument in a few excisions, that the saws in common use would do as well or better. This opinion has been confirmed by an ample subsequent experience, and we find that he is supported in it by most of the operators of his acquaintance. But in stating this opinion he made use of no discourteous language towards Mr. Butcher, nor ought the expression of a temperate opinion to have been met by vulgar railing. With respect to the more serious charge which Mr. Butcher brings against our production of passing over the facts and directions which are laid down in all text-books and recognised by all well-educated surgeons, we would venture to remark that it is quite unsupported by any instances; but so far as we can see by comparing Mr. Butcher's long essays with our own necessarily very short one, the only "fact" that we have omitted is, that excision of the knee is less dangerous than amputation, which we have given strong reasons for disbelieving, and do disbelieve; and the only "direction" is that in this operation the bones should be sawn with Butcher's saw, which we regarded as a matter of complete indifference, and do still so regard. Nor does the critic whom Mr. Butcher quotes point out any such omission, merely saying that the article is "remarkable only for the suppression of everything Irish that has been done in this branch of operative surgery."

[1] There appears to be some satire intended here on the title of the article, which Mr. Butcher appears to think is "Resection." If so, however, he has been in too great a hurry even to look at the heading, which is "On Excision of Bones and Joints."

But really nothing original has been done in this department of surgery by the Irish surgeons which required special notice in a short article for a text-book, where the far more important labours of Syme, Fergusson, Hancock, and others, were necessarily passed over in a very few words.

On a patient review of these papers on excision of the knee, no one would feel disposed to deny that they form a very valuable contribution to the literature of the subject, comprising as they do full directions for the performance of the operation, and for the after-treatment of the case, as well as the detailed histories of five carefully-related and carefully-treated cases. At the same time, our admiration of these qualities of Mr. Butcher's writing must be tempered by regret at the profuse praise which he bestows upon himself, and the violent abuse which he directs against those who differ from him, even in trifles.

Turning from the subject of excision of the knee, we find many others on which Mr. Butcher has given valuable contributions to surgical literature in this volume. We will enumerate only some of them. Thus, we have an acceptable communication on excision of the wrist, a new subject at the present time, and one which can only be properly elucidated by the united experience of those surgeons who practise this operation. Mr. Butcher gives one case of complete excision which proved fatal, and one of excision of portions of the right carpus and metacarpus, which proved quite successful, a specimen of the man's handwriting after the operation being given. Mr. Butcher, however, does not notice the very remarkable labours of Mr. Lister in this operation; probably the essay had been printed before they came under his notice. He advocates the transverse incision across the extensor tendons, preserving only those of the thumb—an operation, to our thinking, inferior to that by longitudinal incisions, as practised by Mr. Lister, though easier and more rapid. Nor does he, of course, enter into the question raised by that surgeon, whether the ill success, which cannot be denied to have attended this operation hitherto, has not depended on the fact that most or all of the excisions have really been partial, and whether it might not have been avoided by removing all the articular surfaces of the wrist, carpus, and metacarpus.[1]

This paper is followed by two of much interest, on the surgical operations which Mr. Butcher has practised in the removal of tumours of the upper and lower jaw. Though there does not seem anything which can strictly be called original in these proceedings, yet the account of them will fully repay perusal, on account of the boldness of the operations in many of the cases, and the great success which seems to have attended

[1] See Lancet, March 25th, 1865.

them, even in very discouraging circumstances. We would especially direct the reader's attention to a case related on page 287, in which the entire upper jaw and palate bone were removed, without chloroform, from a woman aged eighty, and in which the subsequent hæmorrhage was so great that it was necessary to keep up digital compression by relays of pupils for fifty-eight hours, at the end of which time the flaps were pared and united. The patient recovered completely. We may mention that, in these operations on the mouth, Mr. Butcher follows those who shrink from the use of chloroform, and that he prefers Mr. Fergusson's plan of operating with a single incision in the removal of the upper jaw.

This is followed by some detached cases of excision of portions of bones; but though several of the cases are of interest, and show Mr. Butcher's dexterity in the treatment of his patients, yet, as they do not seem to illustrate any controverted principle in surgery, we shall not go more minutely into their details, merely stating that several are good examples of the preservation of limbs in circumstances apparently desperate.

The next subject which engages Mr. Butcher's attention is one more suited than any other to the method on which the greater part of this book is written—viz., the relation of isolated cases. It is the old, but always fresh, topic of wounds of arteries, and their treatment. In the ever-varying circumstances of these complicated and difficult cases it is always useful to have the mind stored with the histories of those which have gone before, and of the success or failure of the measures adopted. We will, therefore, indicate to the reader the main contents of this chapter. In the first place, we have a very interesting, but, it must be confessed, not a very conclusive case of a wound of the profunda femoris artery, in which Mr. Butcher found it impracticable to tie the wounded vessel, and accordingly placed a ligature on the femoral in the groin. As the man died eleven hours after the operation from the effects of previous bleeding, it is impossible to say whether this practice was, or was not, better than its alternative; which, we presume, after the failure to find the bleeding artery, would have been amputation. Then follow two cases of wounded arteries, one supposed to be the posterior tibial, the other the ulnar, successfully treated by pressure on the wounded vessel, assisted by compression of the trunk arteries above. In both these cases bleeding had ceased before the patient's admission, and they are intended to illustrate a judicious precept, on which Mr. Butcher lays stress—viz., the propriety of not searching, by operation, for the wounded artery, unless bleeding is absolutely going on; to which precept, however, Mr. Butcher makes an equally judicious exception, as follows:

"When hæmorrhage takes place from arteries divided in the neck, it is always rapid, profuse, and generally continuous to syncope, and often unto death. Should the carotids escape, the fact still maintains owing to the close proximity of the wounded vessel to the parent trunk . . . . . . At once search should be made for the wounded artery; not for a moment should the patient be lost sight of; every requisition must be had recourse to; the internal administration of stimulants, the repeated applications of warm sponges to the wound, until, by a fresh flow of blood, the source of its origin becomes obvious. Should either of the main trunks be wounded, the artery should be ligatured above and below the wound; if large collateral branches be cut, the same rule should be followed, *if practicable.* I have marked the words 'if practicable' in italics, because sometimes the best efforts of the surgeon will be frustrated in endeavouring to secure the vessels at the wounded part; in such a dilemma he should at once proceed to tie the common carotid; and the records of surgery, if dispassionately viewed, irrespective of theory, will warrant him in the propriety of the practice, and cheer him with the most sanguine expectations of success." (p. 372.)

These precepts are illustrated by two cases of wound of arteries in the neck; then follows a case in which an artery in the buttock, believed to be the gluteal, was tied for wound, and another of the subscapular; and then two cases, to show still further the efficacy of compression of the main trunk above the wounded artery as an auxiliary to direct pressure. Next follows the most interesting and most important part of the whole paper —that on the treatment of wounds of the palmar arteries. If the wound be recent, Mr. Butcher directs the surgeon to apply pressure carefully on the wound with the palmar fascia relaxed, and the fingers bandaged, and to assist this by compression of the arteries of the forearm, and by position of the limb. If in a short time bleeding is seen to be still going on, the compresses being tinged with blood, they should be removed, and an attempt made to secure both ends of the wounded vessel. In wounds which have been inflicted for some time, and where the cavity is full of clot, Mr. Butcher directs that the cavity should be laid open, the clot removed, and the wound sponged, in order to find the bleeding vessel. If this be impracticable, then moderate pressure is to be tried (for the limb will not now bear the same amount of pressure as when the wound was recent) assisted by the application of cold. In either case, if these milder means fail, the brachial is to be tied—a plan which Mr. Butcher prefers to that of securing the arteries of the forearm, on theoretical grounds; but he gives no cases where so severe a measure proved necessary.

We must pass rapidly over papers on a case of ligature of the femoral artery for elephantiasis; on one of extirpation of the eye for primary cancer of that organ ; on a very interesting operation for a large fibro-cellular tumour of the arm ; and on some cases of Syme's and Pirogoff's amputations ; not because the cases are not themselves very interesting, but from the sheer impossibility of commenting on everything contained in this large volume. The next article describes a case of amputation at the knee—as Mr. Butcher calls the amputation through the lower end of the femur, to distinguish it from the amputation " at the knee-joint"—in which the femur is left entire. This is accompanied by a drawing of the stump from a photograph, and it certainly appears to have been a very good one. The operation consists in preserving a very short anterior flap from the skin which covers the patella, and a very long posterior musculo-cutaneous flap from the calf, the femur being sawn just above the condyles. Mr. Butcher attaches much importance to a direction which he gave in the year 1851—to saw the bone " in a curved manner from before backwards, thus securing even a longer stump, more of the bone free of cartilage, and exempt from sharp and irritating edges," and believes that, for this purpose, the saw, which goes by his name, is peculiarly adapted. Mr. Butcher also believes that the removal of the cartilaginous surfaces diminishes the risk to life ; but he has not told us on what experience this opinion is founded. The cases which we have ourselves witnessed of amputation at the knee, preserving the cartilages (only two in number, however) recovered far more rapidly than an ordinary amputation of the thigh.

The next of these papers which calls for mention is one on the treatment of fractures of the thigh-bone. This essay aims more at completeness than most of the others, being not the mere relation of an isolated case or two, with observations on the treatment pursued, but a selection from Mr. Butcher's extensive practice of a most interesting series of cases of fracture of the femur in all parts of that bone, some of them very complicated. These cases are accompanied by a commentary, the object of which is to recommend what certainly appears a very useful modification of Liston's splint, consisting " in the addition of a piece of wood placed in a transverse direction beneath the lower edge of the splint, and upon which its edge rests. The splint is steadied in this position by means of a long screw conveyed through a socket riveted vertically on the side of the splint" (see p. 514, where a drawing of the apparatus is given, and its use fully explained). The object of Mr. Butcher's paper is to show the importance of continuous and permanent extension in the treatment of fractures of the femur in all its parts. In the course of the discussion he has occasion to combat the well-known teaching of Sir A. Cooper, as to the constant forward displacement of the upper fragment in fractures near the trochanter, which, on the contrary, Mr. Butcher believes to occur seldom, and to be easily rectified at the time of reduction. (See pp. 504, et sqq., p. 531.) He also speaks with much reprobation of Mr. Skey's hinted preference for Abernethy's practice "of placing patients with fractured thighs on the side, according to the practice of Mr. Pott, bending the thigh on the pelvis, and the leg on the thigh." But his chief antagonist in this paper is Mr. Syme, from whom he quotes the three following aphorisms: "1. That the great requisites for treating fractures successfully are coaptation and immobility. 2. That extension, or a struggle between the two opposing forces of muscular contraction and a mechanical power, is not consistent with either of these conditions. 3. That therefore extension should be abandoned in the treatment of fractures" (p. 536). It is in this paper that Mr. Butcher reproves Mr. Syme's method of controversy by ironical praise of his "modesty and graciousness" in the passage above quoted; and so, by way of showing Mr. Syme an example of the politeness which the heads of the profession should show to each other in discussion, he proceeds in curt language to qualify Mr. Syme's second aphorism as "absurd" and his third as "ridiculous." Now, with respect to this, we must take leave to say, that whether Mr. Syme is right or wrong, his maxims contain nothing in the least degree absurd, and that they are followed by surgeons of eminence who claim much success for the practice. We ourselves incline to the belief that Mr. Butcher is right in the controversy; but we should be still more disposed to believe so if he could argue in a more becoming style.

We have left ourselves little space to review the remaining contents of this volume. They are of the most varied and most interesting nature, but we must select only some of the most prominent. In the first place, the well-known treatise on Harelip, hardly needs further mention, having been reviewed in our number for Oct. 1856. There is a paper on lithotomy in childhood, in which Mr. Butcher advocates the use of the probe-pointed knife for the completion of the incision into the bladder, and the blunt gorget as a guide to the forceps; but we do not see that he gives any reason for preferring this more complicated method of operating to that with the simple scalpel, which is now usual in London. Very dexterous and experienced lithotomists may no doubt operate successfully though they have frequently to change their instruments, but it seems to us that for those that are less accomplished the simpler plans are much the safer. One of the most interesting cases in the volume is that which describes a new operation on a contracted cicatrix. The novelty consisted in a free subcutaneous division of the bands at the root of the cicatrix, so that the latter could be unfolded at its base. We could hardly give an accurate idea of the proceeding without quoting the entire account of the operation, and must therefore refer our readers to the original for a description of the process, which, if we are not mistaken, may often be found of great service in these difficult and frequently disappointing operations.

We can notice only two other cases in Mr. Butcher's book. One is the account of an attempt to tie the right subclavian artery in the first part of its course, for an aneurysm situated between the scaleni muscles and in the posterior triangle of the neck. The artery was exposed at the desired spot, but found in too diseased a condition to bear the ligature. The same was also the case with the innominate artery. Under these circumstances, Mr. Butcher contented himself with tying the lower part of the carotid, believing that this measure would check the current of blood in the innominate artery. The pulsations of the tumour disappeared on the third day after the operation, and the pulse on the affected side was much weakened, showing obstruction of the artery, but the man died 88 hours after the operation, adding one more to the list of uniformly fatal results of operative measures undertaken in disease of this part of the arterial system.[1] We will quote Mr. Butcher's remarks on this point:

"Deeply, advisedly, long and thoughtfully, this case was considered; and its conditions were weighed, balanced and dwelt upon; the uncertainty of all efforts to cut off the current of the subclavian in its early course rising up as a stern monitor, enforcing thought, consideration and prudent caution. All things considered, I determined on tying the subclavian artery in its first stage; I could detect no valid objection to prohibit such a measure; and, as I do not subscribe to the tenet that, because success has not occurred in other instances, the operation should be abandoned; I will never assent to such a doctrine, (*sic*) because practical experience in other severe operative measures contradicts such a ridiculous postulate . . . . I arranged, if, after cutting down upon the subclavian in its first stage, the vessel was found implicated in the tumour or enlarged, to tie the arteria innominata; and, if departure from a healthy state existed in it, then to ligature the right carotid in its first stage; and, in a few days after, if necessary, the axillary artery." (p. 859.)

The issue of Mr. Butcher's case is not likely to remove the objections of those who doubt the propriety of operative interference in these aneurysms; since the man died, apparently of the mere shock of the operation, before he had been called on to encounter the special risks of softening of the brain and hæmorrhage from the ligature of the root of the carotid artery, still less

[1] A case is said to have lately occurred in America, where the innominate, carotid, and vertebral, were all tied with success; but the very meagre notice of it in the 'Lancet' for Jan. 14th of the present year, does not state whether the tumour was the result of disease or of injury.

the further dangers of operation on the axillary. But we do not think that Mr. Butcher has quite appreciated the opinions of these objectors, at least, if we understand his somewhat confused language. He appears to think that it is taught that because the ligature of the innominate and first part of the subclavian has never succeeded when practised in arterial disease, these operations are to be banished from surgical practice, and this we suppose is what he means to call a "ridiculous postulate." But the number of cases in which the operation has been performed is too small for any such inference to be drawn by cautious reasoners, especially considering the desperate circumstances of most of the cases. Accordingly, all that is taught positively on this subject is that there are very few cases indeed in which the chance of survival from these desperate operations is equal to the chance of spontaneous cure, under judicious medical treatment, and that it is hardly justifiable to resort to operation till the latter method has been tried. In Mr. Butcher's case, he attributes the partial obliteration of the aneurysm to the effect of the distal ligature, but it may be at least as probably referred to inflammation excited by the exposure and handling of the parts. The same effect followed in Porter's celebrated case, where no vessel at all was tied, but the operation entirely abandoned, and yet the aneurysm became filled up and the patient was cured. This case of Mr. Butcher is well worthy of record, and is a new proof of the operative dexterity which he is so fond of claiming; but even at the risk of being set down as a "ridiculous postulator," we cannot see that it advances our knowledge of the treatment of the disease, or renders our impressions of the operation more favourable.

The only other case which we shall notice is one of removal of a large and deep tumour of the neck—a very bold and extensive operation, apparently carried out with great judgment and dexterity, but which proved fatal: as Mr. Butcher believes, from pyæmia. The operation, though remarkable, was by no means unique. A case was lately published by Mr. Spence in which a still larger and more extensive tumour of the neck was removed with success, not to speak of the operations of Langenbeck and older surgeons. From the following passage, however, in which Mr. Butcher is speaking of his own performance, we should suppose that he has not met with any previous cases of the same kind:

"The details of this case portray, in a clear and distinct manner, the innumerable terrors that surrounded it from the first; the alarming and painful death which hung over the individual, and the dangers day by day, closing in and threatening, in a short time, the strangling of his life; the perfect recognition, from the first, of the many difficulties that encompassed the operation, upon the dexterous performance of which the only hope for his safety rested. The execution of the operation, to all concerned about it, only revealed the truthfulness of the diagnosis formed and confirmed in consultation, and the successful accomplishment of it, as recorded, affords grounds for even greater confidence in similar cases. All the dangers from the operation so wide-spread (*sic*), conducted through such hazardous positions, were avoided and escaped from ; and for seven days, day after day, the prospects of recovery seemed more certain, success almost to be achieved, when the symptoms of pyæmia crept in, seized upon the victim, and destroyed life. Yet, by this all-important case a great practical lesson, I conceive, has been taught, very nearly insurmountable difficulties were, by cautious dissection, overcome, and a candid conception and unbiassed judgment of all the particulars in relation to it, wisely weighed, can only elevate the noble art of surgery, and give greater confidence in the almost unlimited resources of its operative range." (p. 879.)

After this (and this is only a sample of many passages of similar tenor scattered over the book) our readers will spare us the superfluous trouble of praising Mr. Butcher ; they will find, if they will peruse his volume, that he has been beforehand with his critics in that particular. Still, although the attractions of this volume are diminished by the extreme self-laudation in which its author indulges, no one can deny that it forms a monument of successful and judicious practice of which any surgeon might justly feel proud. There is little proof in it of the originality which introduces great changes in the practice of our art, for the only matters in which the author claims any original rights are in such comparative trifles as the devising of instruments, or the variation of the details of operations; but the volume gives ample testimony to Mr. Butcher's carefulness in diagnosis, his unwearied attention to the details of treatment, and his energy and dexterity in carrying out the most formidable operations. If Mr. Butcher had left his cases to speak his praises for themselves, as they are so well qualified to do, and had been somewhat more tolerant of differences of opinion, the book would have been well-nigh perfect, as far as the matter is concerned. The language will often strike the reader as singular, but this is perhaps hardly worth mention. Our surgical readers can hardly fail to derive much profit from an attentive study of the volume.

Review IV.

Transactions of the Obstetrical Society of London. Vol. VI. For the Year 1864.—*London*, 1865. pp. 322.

This volume indicates a steady activity on the part of the Obstetrical Society, and contains nearly thirty distinct communications as well as the Annual Address delivered by Dr. Oldham, the President. It is illustrated by a considerable number of woodcuts and nine lithographic plates. As in former years, these 'Transactions' embody the gist of such discussions as arose among the Fellows of the Society out of the various papers or cases communicated, with which we now proceed to make our readers acquainted. *First* of all we have recorded a case of supposed rupture of the placenta, by Dr. Madge.

II. *Extra-Uterine Fœtation, followed by Intra-Uterine Pregnancy. Induction of Premature Labour.*—This case is by Edwin E. Day, M.R.C.S., and is illustrated by a lithograph. The patient died of sudden hæmorrhage after having been progressing satisfactorily. Premature labour was induced by the introduction of an india-rubber bag into the uterus, which was from time to time inflated with air, thus producing alternate dilatation and relaxation of the cervix. Mr. Day, on examination after death, found, in the affected Fallopian tube, what he termed the remains of a deciduous membrane. This Fallopian tube was attached to the base whilst the other one was attached to the fundus of the uterus.

III. *Description of the "Guide Hook," a new Obstetric Instrument.* By Samuel Newham, F.R.C.S.—This paper is illustrated by a woodcut of the newly-invented instrument, which is described as a blunt-hook, with the addition of a prow or guide "for the purpose of favouring the adjustment of the hook into many positions in which a hook is required." The inventor observes, "I feel great satisfaction myself in possessing so useful an addition to my *armamentum obstetricum.*"

IV. *On Fibrous Tumours of the Uterus, treated by Surgical Means.* By I. Baker Brown, F.R.C.S.—This paper contains the details of fourteen cases (related in connexion with others of a similar kind previously described to the Society) of intra-uterine non-pedunculated fibrous tumours growing from the inside of the uterus by a broad base. On former occasions the author had advocated their treatment by a preliminary incision of the os and cervix uteri, and then, by gouging or breaking up the tumour. He now describes the first or preliminary step as being frequently all-sufficient, and states that even when gouging is necessary, a less severe operation is effectual, the slightest destruction of the capsule of the tumour by a pair of blunt-pointed scissors being enough to ensure its destruction.

Mr. Brown minutely describes the various steps of the operation. He speaks of the incision of the os and cervix as not only stopping the hæmorrhage, but as arresting the development, causing the decrease, and sometimes, when it is small, the disappearance of the tumour, especially under the influence of the bichloride of mercury and bromide of ammonium taken internally. Of the fourteen cases here related ten were cured of the hæmorrhage by incision alone, and one relieved. In two only was it necessary to perform the second step of operation, both resulting in cure of the tumour. In six cases the tumour either quite disappeared or materially decreased after incision alone.

V. *On some Cases of Ovariotomy, with Remarks.* By THOMAS BRYANT, F.R.C.S. These cases (ten in number) were brought forward, says the author of the paper, for the purpose of helping to "clear away the prejudices or confirm the convictions with which the question of ovariotomy is still regarded by two opposite parties in the profession," and also of proving "the fallacy of the assertion which has been so boldly made, that it is not possible to secure, in the large and general hospitals of this metropolis, the hygienic and special conditions which are deemed necessary to secure success in such operations." Of the ten cases six were successful, the others not so. Mr. Bryant in his concluding remarks considers the "condition of the patient," observing that the better the general health of the patient, the greater is the probability of success; "the preliminary treatment;" "the warding of the patients, which were all placed in a private room with their own nurses;" "the temperature of the room during the operation," and finally "the operation itself," in this latter consideration commenting on the position of the patient, the extent of the incision, the examination of the abdomen for adhesions, the treatment of the pedicle, the sponging out the pelvis, and the treatment of the wound. The recumbent or sitting position is described as being the best, as being comfortable to the patient, convenient to the operator, and facilitating the falling forwards of the tumour and escape of its fluid contents, and preventing the escape of the ovarian fluid into the peritoneal cavity. Mr. Bryant recommends a short abdominal incision to be made, which, if requisite, may be elongated; and in treating the pedicle, used the clamp, if it was long, or the ligature, if it was short or very broad. The pelvis was not sponged out after the operation, as he considers it a questionable proceeding. In the treatment of the wound, it appears that in all his cases but one he used silver sutures (including the peritoneum in the suture), as they allow a more accurate adaptation of the parts than other kinds of suture do. In nearly all the cases an opium suppository was used after the operation.

Dr. Graily Hewitt then relates a case of fatal umbilical hæmorrhage in an infant.

VI. *Eight Additional Cases of Ovariotomy.* By Dr. Tyler Smith.—The author furnishes this paper as a sequel to the history of twelve cases already related by him to the Society on a previous occasion, all the poor patients having been operated on in private dwellings. In ten cases Dr. Tyler Smith had returned the pedicle into the pelvis after securing it by ligature.

VII. *On the Operation of Cephalotripsy as performed at Vienna by Professor Braun.* By C. G. Ritchie, M.D.—In this short paper descriptions are given, illustrated by a lithograph of the cephalotribe and the perforator, as used by Dr. Braun. The cephalotribe requires two persons to use it. The lithograph also represents a cephalotribe used by Dr. Pagot, of Paris, and exhibited by Dr. Graily Hewitt, of a much simpler kind than the one above noticed.

VIII. *On Spondylolisthesis, with an Account of a Case of Pelvic Contraction from this Affection, in which Premature Labour was induced by the Author's Method.* By R. Barnes, M.D.—This communication, illustrated by several lithographs, is a compendious history of the deformity to which Kilian in 1853 and 1854 gave the name of spondylolisthesis, and of which the most interesting character is the sliding downwards of the fourth or fifth lumbar vertebræ into the true pelvis, so as to contract the space at the brim and in the cavity; and which Kilian distinguished from cases of distortion from rickets or osteomalacia affecting the pelvic bones. Dr. Barnes has collected no less than eleven of these cases placed on record by German observers, and adds one which occurred under his own observation. After giving these cases in detail, the author considers "the nature and modes of origin" of the affection, and subsequently "the obstetric relations of spondylolisthesis and other forms of spinal distortion." After criticising the various views entertained by Kilian, Lambe, Breslau, &c., Dr. Barnes concludes, from the study of these cases, that they are too few to justify any absolute or even exclusive theory of the mode of origin of the affection; that reasoning upon them and upon the surgical history of dislocation of the vertebræ in the dorsal and cervical regions, it cannot be denied that vertebral dislocation may occur from other than intra-uterine causes; and that admitting the possibility of spondylolisthesis occurring as a consequence of spina bifida (as Lambe describes), it must also be admitted that it may arise as a consequence of osteomalacic, scrofulous, and inflammatory diseases of the bodies of the vertebræ, and from direct injury.

Dr. Barnes reviews the characteristics of the four principal causes by which the vertebral column may be distorted—viz., rickets, osteomalacia, shortening of one leg, and spondylolisthesis. Each of which causes produces its own peculiar form of distortion; and he alludes to the intercalation of a supernumerary vertebra as an occasional cause of curvature of the spine, and possibly of spondylolisthesis.

IX. To the above papers Dr. Barnes affixes an appendix containing the details of a new case of the affection published by Dr. Olshausen, of Halle, in March, 1864; and following this appendix we have the description by Dr. Barnes of a small fibroïd tumour which had been expelled by the uterus spontaneously.

X. *Case of Cauliflower Excrescence of the Uterus.* By E. F. Fussell, M.B.—The tumour was removed by means of Dr. Braxton Hicks' écraseur, and is delineated in a lithograph.

XI. *On the Application of Extreme Cold as an Anodyne in the Pain attendant on Parturition.* By J. M. Granville, L.R.C.P.—The cold must be extreme and persistent, and must be applied to the seat of pain, which is supposed to be neuralgic. The author uses a flat tin box filled with a freezing mixture.

XII. *On a New Mode of Treating Cases of Vesico-Vaginal Fistula.* By A. Meadows, M.D.—The author suggests (and by the relation of cases justifies his suggestions) that in all operations whatever of this kind, the old custom of keeping the patient in bed or in the recumbent position afterwards is quite unnecessary and undesirable; and that, on the contrary, the patient should be allowed the same day to go about her ordinary duties.

XIII. *A Case of Hydro-Encephalocele.* By A. Harris, M.D. —In this case the sac was punctured and fluid removed. The child wasted, but lived from the 26th of July to the 15th of November.

XIV. *Case of Destructive Inflammation of the Hip-Joint in a Puerperal Woman.* By T. W. Nunn, F.R.C.S.—This case was apparently quite independent of uterine phlebitis, purulent infection, or of puerperal fever as ordinarily manifested. The patient died on the forty-eighth day after delivery. Mr. Nunn appends an illustrative case described to him by Dr. Fenwick, and concludes with the following question regarding the affection : " Would not the intense pain, swelling, and high pulse, with symptoms of exhaustion, without waiting for fluctuation and other positive symptoms of suppuration, constitute sufficient justification of a deep incision even into such a joint as that of the hip ?"

After Dr. Hall Davis' description of a new craniotomy forceps, and Dr. Meadows' description of a new form of bandage to be used after labour, follows, as paper No. XV., Dr. Aveling's communication *On Immediate Transfusion.*—The author points out the history of the operation, and of the various instruments used in effecting it, and describes a simple apparatus with which he has made several successful experiments (in conjunction with Mr. B. Cartledge) on horses, though he has never tried it on the human subject. This " consists of two small silver tubes to enter the vessels, and of an india-rubber pipe by which they are united, and which has in its centre an elastic receptacle holding about two drachms. It is without valves, and is simply a continuous pipe with an expanded portion in its middle. By its means the vessels are, as it were, extended from one to the other, and a supplementary heart is added to regulate the circulation." An illustration shows well the mode of using the apparatus, described as effective, simple, portable, and inexpensive; and the operation by its means is characterized as safe, easy, uninterrupted, and a clever imitation of nature.

Communication No. XVI. is also a description of an *Apparatus for the Performance of Transfusion*, by GRAILY HEWITT, M.D.— This consists of a glass syringe, into which the blood is directly received from the person who supplies it, and from which it is expelled into the recipient vein by means of a curved canula.

XVII. *Practical Remarks upon the Treatment of Placenta Prævia, with illustrative Cases.* By R. GREENHALGH, M.D.— This is a lengthy paper, and contains the details of 27 original cases. Moreover, it is illustrated by several elaborate tables of statistics connected with the subject, which are far too complicated for us to give any intelligible summary of in this place. Dr. Greenhalgh draws the following conclusions :

"That in any given case of hæmorrhage due to placenta prævia, occurring after seven and a-half months of utero-gestation, when the child is viable, it is expedient, both for the safety of the mother and child, to expedite labour—unless the condition of the patient, from exhaustion, be such as to preclude this step—and if so, then as soon as possible after she has recovered from the shock by every means in our power; that in so doing we should arouse the uterus to vigorous action; and while, on the one hand, we take due precaution to arrest all external flow of blood from the vagina, we should, on the other, adopt such means as will effectually prevent any accumulation of blood in the cavity of the uterus, at the same time that we endeavour to effect a gradual dilatation of the parts. These ends are to be attained by means of a vaginal plug, an elastic abdominal bandage, and ergot of rye, with the occasional additional aid of stimulating enemata frictions over the abdomen, and every now and then rupture of the membranes; turning only being had recourse to in cases of presentations of the upper extremities; the forceps where, from inefficient action of the uterus, or slight mechanical impediments, arrest takes place in the passage of the head through the pelvis; and craniotomy in more serious cases of disparagement in size between the presenting part and pelvis." (p. 175.)

XVIII. *Fibrous Tumour of the Cervix Uteri.* By G. C. P. Murray, M.D.—The author observes, that enucleation might have been performed had it been possible to ascertain the exact position of the tumour, and how easily separated it was from its sides. A lithographic illustration accompanies the description of the case.

XIX. *Case of Cæsarean Section.* Taken from the posthumous papers of the late T. E. Bryant, Esq.—This case occurred in 1857, and is communicated by T. Bryant, Esq. The patient died; the child lived nearly a month. After death, the pelvis of the patient was found to measure from pubes to sacrum $2\frac{1}{4}$ inches; from ileum to ileum $4\frac{1}{2}$ inches; and the depth of the pubes was $1\frac{3}{8}$ inch.

Following the above paper is a description, by Dr. Eastlake, of an "Obstetric Binder," intended to supersede those which lace up like stays.

XX. *Description of a Deformity of Arms and Hands.* By Dr. J. Shortt.—The subject was a young man, aged twenty, a native of Southern India. Every part, bones and tissues, of the fore-arm and wrists on both sides were absent, and the thumb and little finger of the hands are also wanting. The man is otherwise well made.

XXI. *Missed Labour.* By A. W. Williams, M.D.—The author relates three cases so described which occurred in his own practice.

XXII. *Post-mortem Examination of a Woman at the full Period of Gestation, and in the First Stage of Natural Labour. Death by accidental Hæmorrhage, chiefly internally.* By F. J. GANT, F.R.C.S.

XXIII. *Distension of Ureters, Pelvis, Kidney, &c., in an Infant.* By H. GERVIS, M.D.—The child was only five weeks old. No cause of obstruction was found. A plate of the diseased organs is given.

XXIV. *Double Monstrosity.* By F. F. JAY, Esq.

XXV. *A simple Instrument for Vaginal Injections.* By A. RASCH, M.D.—This apparatus is merely an ingenious application of the syphon.

XXVI. *On the Management of the Third Stage of Labour.* By Dr. EASTLAKE.—The author reviews the literature of the subject. He expresses an opinion that it is necessary, during the expulsion of the fœtus, to lay the hand firmly on the fundus of the uterus, and thus to help its contraction, traction on the funis being for the most part uncalled for: points which Dr. Eastlake considers as not sufficiently dwelt upon. As regards retention of the placenta, owing to morbid adhesions, the author thinks this condition of things may be diagnosed by the stethoscope, and describes a modification of the uterine souffle contingent upon the death of the fœtus, a muffled sawing noise replacing the gentle blowing natural murmur; but he is unable to say how soon this change occurs. After insisting on the *in*advisability of giving ergot in cases of retained placenta, until we know that no morbid adhesions or irregular contractions exist, and the desirability of giving chloroform in case of spasm of the os uteri when the placenta has become encysted, he alludes to the possibility of the presence of a supplementary after-birth, the presence of which, after the expulsion of the true placenta, tends to give rise to secondary hæmorrhage.

XXVII. *Case of complete Extirpation of the Uterus and Ovaries with large Fibrous Tumours.* By I. B. BROWN, F.R.C.S. —The patient died a few hours after the operation, after an attack of vomiting and collapse. On post-mortem examination, a large mass of coagulated blood was found in the left iliac fossa, which had escaped from the left broad ligament, from which the ligature had slipped during the operation. The organs of the body presented nothing uncommon. The operator observes, that had he to repeat the operation, "he should either divide the broad ligaments with the actual cautery, or apply a double ligature by piercing the ligament, and thus render it impossible to slip." A plate of the removed parts is furnished.

XXVIII. *On Puerperal Embolism.* By W. F. WADE, M.D.—The author, after reviewing the views on embolism set forth by Virchow, details the case of a woman, aged twenty-four, who, some days after delivery, became affected with dyspnœa and a sense of impending death. The left thigh and leg were swollen and white. After examination physical and general a clot in the pulmonary artery was diagnosed. The patient died after lingering ten days in the same condition. After death, the pulmonary artery was found occupied, from its commencement to the third or fourth division, by a very dense, firm, whitish clot, similar to that which lines an old aneurysmal sac. This was adherent to the artery all round, and when peeled off left the lining membrane pale and, to all appearance, perfectly healthy. The coagulum was perforated by a small, irregular, tortuous channel. Isolated patches of pneumonia existed, and both iliac veins were inflamed, and contained clots.

XXIX. *An Inquiry into the best mode of Delivering the Fœtal Head after Perforation.* By J. BRAXTON HICKS, F.R.S.—This important paper is one of considerable length, and is illustrated by several cases, and by a series of experiments made upon fourteen full-term children, undertaken with the view of ascertaining the amount of reduction of the diameter of the fœtal head under varying degrees of perforation, evacuation, and breaking up of the calvarium, the diameters which oppose under the different presentations being compared. The author seeks "to develop some new points which will serve to improve our practice, and which will place the operation upon a more satisfactory basis." He observes:

"The question itself may be put more practically before us thus—In a given case of severe distortion of the pelvis, is it necessary to perform Cæsarean operation? This can only be answered by first disposing of the question—What is the smallest antero-posterior diameter of the pelvis through which the head can be brought by any means in our power? Upon this a third then arises—What is the mode of reducing the measurements of the fœtal head, and of altering the relations of the now altered diameters, so as to produce the least obstruction?" (p. 264.)

The above analysis, which we have made of the various contributions to this volume of Transactions, will show that the Fellows of the Obstetrical Society well maintain the scientific and workmanlike character of their labours.

Review V.

Outlines of the Chief Camp Diseases of the United States Army as observed during the Present War. A Practical Contribution to Military Science. By Joseph Jamieson Woodward, M.D., Assistant-Surgeon, U.S.A., &c.—*Philadelphia,* 1863. pp. 364.

Great wars may be considered as great experiments both in relation to man and his surroundings; on the one hand testing what he is capable of doing and enduring; on the other hand testing the climatic influences to which he is more unguardedly exposed than during the piping times of peace. If this be true of war generally, it is emphatically true of the civil war now carried on during four years in America, and on a scale of unprecedented magnitude. Never in the Old World were there larger armies in the field; never before were armies so rapidly formed, we might say extemporized; and hardly ever has the scene of action been more extended and varied.

The history of this war (which we now trust is brought nearly to a conclusion) will be interesting in the highest degree, and cannot fail to be proportionally instructing; its elements of interest are so many, and its circumstances so peculiar. We are glad to learn that even from its beginning the Federal government have been collecting materials for it: let us hope that an historian will be found worthy of the occasion, and capable of doing it justice.

The work before us, by a medical officer of the United States army, engaged, as he informs us in its preface, "for many years in preparing for the Medical Department 'the Medical History of the Rebellion,'" is, as regards that history, very promising both as a specimen of what may be expected, and as a practical contribution to military medicine.

Dr. Woodward has judiciously confined himself to the chief camp diseases which have prevailed during the early period of the struggle, and which, indeed, are the chief diseases of armies in the field, whenever engaged in active service and in a protracted campaign, namely, fever and bowel complaints, diarrhœa and dysentery: these, at least, are the most conspicuous and fatal.

What strongly marked their character, comparing these diseases with those occurring in civil life, was in the great majority of cases either a malarious or a scorbutic taint, or a blending of the two, associated more or less with an adynamic condition. These peculiarities, we may remark, are no more than might be expected, and are well explained by the author by reference to climate and to the various noxious influences to which troops in the field are exposed, especially new levies, such as those of which the vast armies were formed at the breaking out of the war, when, from a force hardly amounting to twenty thousand men, it was augmented on a sudden to several hundred thousands.

Further, as might be expected, according to all experience, the fatality of the prevailing diseases was proportionally much greater than in the regular army when not actively engaged. In the early period of the war—viz., up to the 30th June, 1862, it appears that the general rate of deaths was double that of the old standing army during the eighteen previous years of peace. We shall be much surprised if, when the entire history of the war is given, the rate is not found to be considerably greater; for it is a rule, that the longer a war lasts, or a campaign is protracted with energy, the larger is the proportional sickness, and the greater the proportional mortality, the wear and waste of an army being, as in an inanimate machine, very much in the ratio of expenditure of strength.

Amongst the diseases which have been little prevalent in the United States armies, there have been two from which our troops have always suffered much, and greatly to their discredit, syphilis and delirium tremens. The proportion of the former, including all venereal complaints, is returned as very small, only 23,779, of a mean annual strength of 281,177; and that of the latter, including all affections arising from the abuse of alcoholic liquors, was so inconsiderable as to be dismissed with the general remark that they were comparatively rare, and "presented no peculiarities in their phenomena." Malingering, however, we learn, was more than commonly frequent, attributable to the high bounties offered to recruits, the facility of re-enlisting without much risk of detection, and the inexperience, at least, in the early part of the war, of the medical officers in detecting imposition.

Of the localities, the great fields of action, three are specially distinguished by the author,—these, the Atlantic border, the central region, and the Pacific border, each of which is marked by peculiarities as to the diseases occurring in them. Of the three, the Pacific border has proved the most healthy, least productive of malaria; the central region most unhealthy, most malarious; the Atlantic border less than the central region, but more than the Pacific. On this subject, the peculiarities of climate of the different parts of the United States, and the diseases most prevalent referable to climate, we would refer our readers to a former number of our periodical, that of April 1858, in which is reviewed "The Statistical Reports of the Sickness and Mortality of the United States Army, embracing a period of sixteen years."

The chapter, the second of the volume, in which Dr. Woodward treats of the conditions determining the character of camp diseases, will be read with interest by the army surgeon. Besides the two influences already mentioned, malaria and a scorbutic taint, he very properly insists on a third, that which arises from overcrowding, which he designates emphatically "crowd-poisoning," and to which, as their chief cause, he traces fevers of the typhoïd kind.

On malaria he offers many remarks, most of them judicious, though there is little in them that is original. Like the best writers on the subject, he admits that it is only known by its effects—i.e. (and he so defines it), as "the unknown cause of intermittent and remittent disease;" and yet he assigns marsh miasma as its synonym, and considers it productive of diarrhœa and dysentery, and various other ailments, especially *gastric* and *hepatic disturbances.* This, its implied source, and its inferred wide extension of operation, are not in accordance with his definition, and as regards correctness are open to question. That malaria may by its action modify other diseases, even to the extent, as he believes, of creating a malarial cachexia, we are willing to admit. It is in the complications it may induce, which should be marked by periodicity, that, practically viewed, the subject of malaria is of the first importance; and never probably before has its influence in affecting other diseases been more strikingly shown than in the United States armies, as described by the author.

To crowd-poisoning (a happily selected term, as we are still ignorant of that which may be called its *vera causa,* that by which it operates) he assigns not only the adynamic character of diseases, not only typhoïd fever and typhus, but also oriental plague. That the former may owe their origin to it, there is no want of proof; but we cannot say the same of oriental plague. As to its genesis, and that of yellow fever and cholera, there is much mystery; and we fear it must be confessed that our knowledge is extremely defective, confined rather to what is negative than positive. Fortunately, during this great civil war the armies in the field have been exempt not only from the plague (indeed, that disease has never appeared in America), but also from the other two formidable epidemics, although one of them, yellow fever, we are informed, was relied on by the Confederates as an efficient ally in the Southern States when they began their great struggle for independence. Even typhus and typhoïd fever, we are assured, were of rare occurrence in the United States army prior to the present war. Previously about 5 cases per 1000 of mean strength were their proportion; that has increased to 81, as many as 22,801 cases having been reported during the year ending June 30th, 1862, in an annual mean strength of 281,177 men. These cases were rarely simple and uncomplicated, such as occur in civil life, but, according to the statement already made, were variously modified, and exhibited phenomena of a malarial or scorbutic kind, or of both, and in some instances to such a degree, as, in Dr. Woodward's opinion, to justify the recognition of "a mixed type of disease, deserving even a new name." Their origin was due, besides overcrowding, to its common concomitants, very imperfect ventilation and a sad neglect of cleanliness; and in proof we are assured—though the assurance is hardly needed—that fevers of the type adverted to were most rife where these unsanitary conditions were most conspicuous, whether in tents or barracks. Fortunate were those men, we are told, who in winter, in the latter, "got between two and three hundred cubic feet of air-space." Speaking of the condition of the men: he says, "How often, especially in the winter time, are they unwashed, their clothes filthy, their bodies full of vermin, and heaps of garbage piled about the camp and its neighbourhood." We must refer to his pages for details on this repulsive subject. Little can those who think of the pomp and glitter of "glorious war" realize the scenes which are more truly characteristic of it. We must refer, too, to our author for the measures which were taken to mitigate the evils—noticing only one or two of them. One was the extemporizing a tent of the soldiers' ponchos (square pieces of oil-cloth, with a slit in the centre, through which the head can be thrust, thus serving as a mantle); of these, two, properly put together, formed a skeleton tent for the men to whom they belonged. Another was the "Californian furnace"—a simple contrivance, for warming a tent or a row of tents, and, whilst warming, drying the ground. It is described as a pit made outside for a wood fire, with a trench covered with sheet iron on flat stones, passing under and terminating in a chimney beyond the last tent. It is stated that this cheap and simple arrangement has been found adequate to warm, imparting an agreeable temperature, and that readily maintained, to even three or four hospital tents placed in a row.

Dr. Woodward, holding that crowd-poison is adequate to account for fevers of a typhoïd type, considers it unnecessary to discuss the question whether they are propagated by contagion or not. The idea of their being contagious is abandoned, he says, by the leading practitioners in America; and he adds that no facts have come to his knowledge during the war "to justify a belief in the possibility of their contagion."

Relating to scurvy, and more especially its taint, many interesting particulars are related. Dr. Woodward, whilst he claims for the United States armies a greater exemption from the disease than in armies generally, owing to the means taken to prevent it, admits its presence, especially amongst the troops serving in the eastern districts. In the simplest cases, he describes the symptoms as those of "great general debility, with a deficient or morbid appetite, impaired digestion, disinclination to exertion, and intellectual torpor and apathy, occurring in patients apparently free from organic disease." Large numbers of men, he states, have been discharged the service, and, as he thinks, improperly, on surgeons' certificates of disability, who, had they been properly treated, might have been rendered efficient. The diagnostic symptoms of the disease are well described by him. Fresh vegetables, he maintains, are, as is now generally admitted, the requisites for its cure, and not fresh meat,—cases of it indeed having occurred amongst troops on fresh-meat rations. As to the pathology of the malady, he expresses the opinion that it has yet to be discovered, and that the potash-theory—the view that a deficiency of this alkali in the blood is its cause—is not tenable, nor the cure of the disease practicable by the administration merely of alkaline salts.

A large space in the volume is given up to camp fevers and to chronic malarial poisoning. Of the former, typho-malarial fever has been by far the most predominant. From a table which he has drawn up, formed from the army returns of sick, it appears that during the year ending June 30th, 1862, the ratio of fever cases per 1000 of mean strength was, for the Atlantic border 23·899, for the central region 31·994, for the Pacific border 60·95.

His description of this fever and of its many varieties, as modified by the several morbific influences, is ably written, and is deserving of careful perusal. Our limits do not permit our entering into its details. Periodicity marks the malarial taint; while the abdominal conditions, those of enteric fever with adynamic symptoms and mental and bodily prostration preceding and following the attack, with hæmorrhagic tendencies, &c., indicate the scorbutic diathesis.

The pathological anatomy of the fever is ably discussed, and is enriched by original observations, especially microscopical, made by the author. He maintains, in opposition to preceding pathologists, that the peculiar morbid state of the intestinal follicles, as witnessed in fatal cases of typho-malarial fever, is not owing to a peculiar exudation, but to cell-multiplication.

Though about one-third of the total deaths have been owing to this fever, its proportional mortality, it would appear, has been less than that of camp fevers of European armies. It has varied in the different regions: on the Atlantic coast, it is stated as 7·19 per thousand cases; in the great central basin between the Appalachian and Rocky Mountains, as 10·18; and on the Pacific slope, as low as 4·52.

The treatment described as most successful has been that of a mild kind, varied according to the complications, with great attention to diet and nursing. The most active medicines used have been quinine and arsenic, to the exclusion of bloodletting and the employment of mercurials and antimonials, which he considers as altogether contra-indicated, as too debilitating, and this not only in fever cases, but in all the other diseases to which the soldier in the field is liable—an opinion that seems to be generally acted on by the medical officers of the United States army, and, considering how the lancet and these medicines have been abused, especially calomel, we are disposed to think with propriety.

Where treating of fever-diet, he very properly points out that in all the forms of beef-tea and beef-essence there are present merely the collaginous elements of the meat derived from the connecting tissue holding the muscular fibres together, and the extractives and salines of the muscular juices, but none of the substance of the muscular fibres themselves—musculine not being soluble in boiling water, and consequently are feebly nutritive. Liebig's well-known tea, as most supporting from its composition, and as also tonic from the acid with which it is prepared, is the kind he most recommends; but even this, it should be kept in mind, is feeble, a thousand grains of it yielding on evaporation, on an average, only about fifty grains of dry residue. Raw beef, made into little pellets of five or ten grains, after having been finely grated, has had a trial in the nourishment of fever cases, but not yet to such an extent as to allow of any decisive conclusion as to its efficacy. There is an observation which he makes on the subject of sick-diet, which we give on account of its justness—viz., that hitherto the diet most appropriate during the febrile stage has been less considered than that during the stage of convalescence. We will add another, conceived by him in the same spirit, that the medical officer "is too often more intent on regulating the administration of medicines than in attending to the nice adaptation of the dietetic measures to individual cases."

The manner in which he suggests the use of ice when the object is to cool the heated head is deserving of being better known; it is by including small lumps of ice in a bladder, with at least an equal bulk of lard, which renders the melting of the ice slower, and insures a more moderate reduction of temperature than by the means usually employed in this cooling application.

Of the medicines of which he takes particular notice, there are two which we do not think it right to pass over; these are arsenic, the solid arsenious acid, and chlorate of potassa. Of the latter, whilst rejecting the idea that it supplies oxygen to the blood, he speaks as being valuable, given in doses of from ten to twenty grains in solution every two hours, as a diuretic and diaphoretic, especially in cases " of deficient secretion and heat of skin, of the second and third week," and where " the cerebral phenomena are marked." Relative to the use of arsenic, he informs us that Surgeon Cox has contributed a paper on it, which will be given in full in the medical history of the war, and in which he claims for this mineral a special effect in arresting intestinal follicular disorder. He prescribes it in the form of pill, three grains of the arsenious acid combined with three of opium, divided into thirty-two pills, one of which is to be taken three times a day, always after eating.

Of intermittent fevers, including congestive or pernicious intermittent and chronic malaria-poisoning, he treats but briefly. The proportional prevalency of these diseases as regards regions has been much the same as that of typho-malarial fever. Of the 72,810 cases returned to the Surgeon-General's office during the year ending June 30th, 1862, 40,375 cases and 32 deaths were given in as quotidian, 26,750 cases and 33 deaths as tertian, 3451 cases and 4 deaths as quartan, and 2234 cases and 351 deaths as congestive. It is worthy of remark, as illustrative of the manner in which diseases are intensified in war, that, comparing the mortality from intermittent fever during the first year of this war and the preceding eighteen years of peace, the deaths have increased proportionally in a triplicate ratio.

Dr. Woodward comments on the use and abuse of quinine dissolved in whisky, employed as a prophylactic, and we think judiciously. Whilst he admits that there are occasions when it may be given with advantage, such as under fatigue in a malarious country, he deprecates the giving it as a daily ration in the instance " of healthy troops, well fed and cared for, and not unreasonably exposed," and where coffee is liberally supplied, as in the United States army. He urges " that it predisposes to habits of intemperance, and that it diminishes rather than increases the daily consumption of food, upon which, of course, the soldier must chiefly rely for the maintenance of his energies."

The most severe and dangerous form of ague, that of the congestive kind, is stated to be almost unknown in the northern and middle States. In very many of its symptoms it closely resembles Asiatic cholera, so much so that in its first paroxysm it might be mistaken for that disease. It differs, however, essentially in its intermittent character, in the nature of the ejecta, and in the circumstance that its first attack rarely proves fatal. Of its varieties, the most remarkable was that witnessed by the author at the siege of Yorktown, in which, during the first or second paroxysm, petechial blotches made their appearance over the body. If not rapidly fatal, the disease ran its course as severe typho-malarial fever with variable results. This variety Dr. Woodward is inclined to consider identical with pernicious remittent fever. The administration of large doses of quinine was found, of all the means employed, least unsuccessful. For the general treatment of the disease, as the indications vary, he remarks, so must the remedial means. Stimulants during the stage of collapse were required—sulphate of quinine during the intermission, for instance, as stated by the author, two grains every second hour, combined with half a grain of opium and a quarter of a grain of capsicum. For the minutiæ of the treatment we must refer to the volume.

As a novelty in practice, at least to us, we may mention one of the modes which the author describes of raising a blister, when indicated, at the epigastrium, for the relief of irritability of stomach. The method is by laying on the part a wet cloth, and on it for a few seconds a piece of metal, such as a pound weight, heated by immersion in boiling water. After the removal of the cuticle thus raised, half a grain of morphia rubbed up with a little sugar may be sprinkled on the raw surface.

In the fatal cases, we are informed that there were no constant post-mortem appearances of a characteristic kind. Vascular congestion of the viscera was a frequent but not invariable occurrence; and a softening and enlargement of the spleen was generally observed, as in other instances of fatal malarial disease.

The account which Dr. Woodward gives of "chronic malarial poisoning" reminds us of Beri-beri, that obscure, anomalous, and often fatal disease, which at different times has occurred amongst our troops in Ceylon and the West Indies. The advanced stage of both is marked by anæmia, accompanied with dropsical effusion; and of both the exciting causes appear to be much the same, malaria, impoverished diet, and other noxious influences of camp life. Removal out of the way of these influences is described as being most conducive to recovery; and when that is impracticable, the adoption of hygienic measures, such as are available in the field, and appropriate medication, chiefly by quinine and steel; or, the former failing, substituting arsenic in the shape of Fowler's solution. When the disease was fatal, the spleen, liver, and kidneys were generally found to be enlarged. Their peculiar state is minutely and well described by the author.

The subjects of the later chapters of the volume—viz., jaundice, diarrhœa, dysentery, chronic diarrhœa, camp measles, catarrh, pneumonia, and lastly, pseudo-rheumatic affections, we must pass over unnoticed, or notice very briefly, for, besides what our limits prohibit, special notice of them is the less necessary, inasmuch as these diseases as they occurred in the United States armies for most part exhibited but few peculiarities.

Jaundice, on which there is a distinct chapter, as an epidemic, was of rare occurrence. It is described as most frequent during the first year of the war, when 10,929 cases were reported, of which 40 proved fatal. Dr. Woodward very properly notices it as a symptom rather than as a specific disease, and as most prevalent in the regions where the malarial influences were most intense; and yet, contrary to what might be expected, it was more common in winter and spring than in summer. Our author attributes it not to obstructed, but to suppressed secretion—a pathological condition of the liver, needing further research.

Of the other diseases just named, diarrhœa and dysentery, catarrh and pneumonia, showed nothing abnormal; as generally, whether in India or Europe, the two first were associated with high atmospheric temperature, and were most prevalent in summer and autumn. The two latter, on the contrary, were associated with a comparatively low temperature, and were mostly winter diseases.

The cases of both diarrhœa and dysentery were numerous, but their mortality comparatively small. Of acute and chronic diarrhœa, and of acute dysentery, 215,214 cases were returned during the year terminating June 30, 1862, of which number 1194 were reported fatal. As in the instance of fevers, these diseases were of greater frequency in the central region, and of least on the Pacific border.

Of the causes of these complaints some are well ascertained; whilst others, which have been assigned, and in this work, are open to question, such as hard water, from the presence of lime or magnesia, and malaria. The former is considered by Dr. Woodward as one of the causes of diarrhœa, and the latter of dysentery. For our part, we are disposed to view them rather as coincidences than as positive agencies in the production of these diseases. When we reflect on the very small quantity of lime or magnesia held in solution in hard water, we cannot agree with our author that they act in the same manner as saline cathartics. And as to malaria, it is a well-established fact, that there is no constant connexion between it, or between fever of the intermittent type and dysentery.

On the treatment of these ailments Dr. Woodward makes many good remarks; and the plan which he advocates seems, on the whole, judicious; but we are of opinion that he attaches too little importance to opium; indeed, except for procuring sleep, he rather deprecates its use—to quote his own words, "as exercising little or no curative influence, and as liable, by interfering with the already impaired digestion, to do positive harm." This he says when treating of chronic diarrhœa, which, as described by him, seems to us more deserving of the name of chronic dysentery; indeed, he himself is in doubt whether the designation is correct. His low estimation of the virtues of opium in dysentery we can only attribute to his never having had to contend with the worst form of the disease, such as occurs within the tropics, and is there so fatal;—a form in which opium in large and repeated doses often acts heroically, and most so when it produces no hypnotic effects. One remedy which he adverts to is deserving, we think, of more attention than it has commonly received—viz., nitrate of silver, administered in injections, from five to ten grains to the ounce of water, thrice daily. Assistant-surgeon Hartsuff, his authority for the practice, speaks of the greatest success from its use. A like remark is applicable to the subnitrate of bismuth, given in doses of from five grains to a scruple, four times daily, as used by another medical officer of the United States army, Assistant-surgeon Frask, who reports as having found it very efficacious, especially when, with the bowel complaint, there has been nausea and other derangements of the stomach. That portion of the chapter which treats of the histology and the pathological anatomy of these diseases, is most original, and is highly creditable to the author for the microscopical researches which it displays. We must refer to the work for details; a brief abstract would not do him justice.

"Camp measles," to which a chapter is assigned, is a new feature in the medical history of a campaign. As many as 21,676 cases were returned during the first year of the war, of which 551 were reported fatal; and these cases were not all that occurred, the deficiencies, the non-reported, are mentioned as innumerable. That the disease became epidemic from infection there appears to have been satisfactory proof; though, in some instances in which it could not be traced to contagion, it seemed to take its origin *de novo*. There was, at one time, a suspicion entertained that a fungus, which is developed in wet straw, might be one of its causes. The notion, advocated in the first instance by Dr. Salisbury, was, on further inquiry, and from trials made with the fungus, considered groundless — affording another example of erroneous conclusion founded on a coincidence.

In the chapter on catarrh, Dr. Woodward describes a peculiar pathological state of lung which hitherto has very much escaped attention—that of collapse, in which the air-cells are deprived of air, and are yet capable of receiving it, and of being redistended. He assigns as its cause, "a more or less complete obstruction of one of the bronchial tubes by the thickened secretions," allowing of a partial escape of air during expiration, none entering during inspiration; the opposite, we may remark, of pneumothorax.

Under the head of the same disease, when treating of the distinction between pus and mucus, he very property points out that "it is to be sought in the physical and chemical characters of the fluid in which the corpuscles float, and not in the specific characteristics of the corpuscles themselves;" as, indeed, is now admitted by the best authorities in modern pathological research. The clear manner in which he discriminates this is worth quoting:

"When the fluid is tenacious, colourless, transparent, or slightly opaque and whitish, with more or less abundant epithèlial elements, and few or none of the so-called mucus corpuscles, it is to be designated mucus.

"When yellowish or a greenish yellow, with little or no epithelium, and abundant corpuscles, but tenacious, tough, stringy, and but little miscible with water, it should be called muco-pus.

"When the abundant corpuscles float in an albuminous serum, and are readily miscible with water, the name should be pus."

So prevalent were catarrhal affections amongst the troops, that during the first year of the war over 125,000 cases were reported. They were most prevalent during the cold and cool months, and about equally so in the three several regions. Pneumonia, too, during the same period, was of frequent occurrence; 11,061 were the reported cases, of which a comparatively large proportion proved fatal—viz., 2134. The disease was mostly, from its first invasion, of an adynamic character, thus accounting for its great mortality. Its pathological anatomy is minutely and carefully described. The treatment he advocates is that now most approved on this side of the Atlantic, from which the use of the lancet is almost entirely excluded. When treating of therapeutic measures, including alcoholic stimulants and their proper mode of use, he makes some remarks, which we are induced to quote both for their goodness and as an example of his writing and reasoning:

"Alcoholic stimulants are amongst the most important of the therapeutic measures to be employed in those cases of pneumonia which have gone on to consolidation of the lung; and as considerable looseness of opinion exists as to the purposes for which they are given, and the proper mode of use, even, indeed, as to whether they should be used at all, a few remarks on the subject appear appropriate.

"The lung having once passed into the state of consolidation, may either gradually undergo resolution, and be restored to its normal condition, or it may become softer and more disorganized, passing finally into a state of diffuse suppuration, or even occasionally of gangrene with a fatal result.

"Resolution is effected by the gradual casting off with the sputa of the elementary forms which stuff the minute bronchial tubes and air vesicles, and the reproduction of normal epithelium. This process requires for its accomplishment a certain degree of vital energy and constitutional force; and where these are exhausted by the previous progress of the disorder, stimulants and nutrients are imperatively demanded to sustain the sinking energies of the economy in the condition in which these favourable changes are possible. They are as necessary here as in those surgical cases where a slough is to come away and cicatrization is to be accomplished; and to omit them is as irrational and injurious in the one case as in the other.

"The fatal process to be dreaded, the transformation of the consolidated lung into grey hepatization or diffuse suppurative alteration, is not a process of increased vitality, whatever may be thought of the processes of the earlier stages. On the contrary, it is essentially a degenerative change, accompanied by the fatty transformation, and must be accelerated rather than diminished by depressing remedies of every kind, just as in a great granulating surface the healing process is accelerated by tonics, nutrients, and, if the extent of mischief is great, by stimulants, while mercurials and other depressing agents increase the suppuration, and impede the healing process.

"Such is very briefly the theory of the use of alcoholic stimulants in the stage of consolidation. It will be at once perceived that no direct curative influence is claimed for them, no good can result from their reckless and indiscriminate use; they are merely supporting remedies. If they moderate, and even shorten the disease, it is simply by aiding in sustaining the vital energies at the points at which the natural restorative processes are possible. They are resorted to in pneumonia with very much the same objects that lead to their moderate employment in typhoïd fever or in gun-shot wounds."

The last chapter, the tenth, is on "Pseudo-Rheumatic Affections," a subject of much difficulty, and of considerable importance in military practice. The most characteristic group of these cases, he says, is popularly designated "*sore backs* and *weak backs*." Putting aside instances of malingering, Dr. Woodward is of opinion that, in a majority of them, there is a scorbutic diathesis owing to a deficiency of fresh vegetables in the dietary of the soldier, and to other conditions of military life conducive of a cachectic state. This chapter is specially deserving of the attention of the army surgeon. For his perusal and study we quote the following paragraphs, comprising some excellent rules for the detection of malingerers: they are from the "Regulations for the government of the Bureau of the Provost Marshal General, issued in 1863," and provide that—

"Chronic rheumatism, unless manifested by positive change of structure, wasting of the affected limb, or puffiness or distortion of the joints, does not exempt. Impaired motion of joints, and contraction of the limbs, alleged to arise from rheumatism, and in which the nutrition of the limb is not manifestly impaired, are to be proved by examination while in a state of anæsthesia, induced by ether only.

"Pain, whether simulating headache, neuralgia in any of its forms, rheumatism, lumbago, or affections of the muscles and joints, is a symptom of disease so easily pretended that it is not to be admitted as a cause for exemption unless accompanied with manifest derangement of the general health, wasting of limb, or other positive sign of disqualifying local disease."

In concluding, we would remark that there are few evils which have not their alleviations, and that war, with all its horrors, is not an exception, inasmuch as if it brings out some of the worst passions of bad men, it calls forth some of the best feelings of the good. In no wars have we had a stronger example of the latter than in this American civil war, especially as exhibited in the noble exertions made by the Sanitary Commission, and we are happy to think with so much success, in affording efficient aid in the relief of the sick and wounded. Another reflection has occurred to us whilst reading Dr. Woodward's work, that if war be dreadfully destructive of human life—and this American war has been specially so—some consolation may be found in the belief that the art, that science most deserving of the name of divine, the healing art, medical science, owe much of their advancement to war, from the vast and varied experience which it affords to their cultivators, the army physician and the army surgeon; and in adding that the volume before us may be adduced in proof, we pay no small compliment to its author.

Review VI.

English County Asylum Reports for the Year 1861.

If the study of insanity make such slow progress, it certainly will not be for lack of material. The yearly reports of the public asylums deal with ever increasing numbers, and the great difficulty in every county seems to be how to provide sufficient accommodation for its insane. Such a state of matters implies a large field for observation, and, moreover, a field hitherto but little explored, for the rapid increase of these establishments has taken place within the last twenty years or so. We cannot say, however, that the labourers in this new region of science have been many, nor that their work has been very productive. With few exceptions, the only visible results of their labours are concealed in their annual reports, and the information we would fain pick up is only to be had by a weary search through these tedious documents. The character of these reports varies with the notions of the superintendents, some dealing with medical questions pretty fully, and affording instructive material, while others—and the majority—dwell chiefly on domestic matters. Dr. Thurnam, of the Wilts Asylum, one of the ablest, most industrious and accomplished men holding a lunacy appointment, discusses the question of the proper scope of the annual report in that of his own asylum for the past year, in the following terms: "Considering that the great majority of readers to whom the reports are addressed are non-professional persons, the superintendent has been sparing of medical details. The notion of advancing our knowledge of insanity as a disease from details printed rather than published in reports of asylums, appears to him a mistaken one." He admits, however, as one of the necessaries of the reports, "full statistics of the cases treated, with the results obtained, as illustrated by an extensive series of tables," and considers that the adoption of a uniform series of tables by the various asylums is a great desideratum, and this is the chief point for which we have on former occasions contended. This is a matter on which the heads of the public asylums ought to be agreed, and while there exists the present diversity of custom in respect to it, their statistics, which might alone be of such value, are almost useless for any combined purpose. No one can reflect on the present position of our public asylums without feeling how unsatisfactory it is as regards the profession. These large establishments present opportunities for clinical study and teaching of the utmost value to every practitioner, but partly from their mode of management, are of as little use for this purpose as though they existed not.

At the present time, the London University requires a course of clinical study in mental diseases of certain of its graduates, and there is no satisfactory means of obtaining it; but what one examining board insists upon as needful is still more necessary for the great body of practitioners, who are debarred from any means of acquiring experience in insanity till they are called upon to deal with such cases in practice. It is then that they feel aware of their deficiencies, and display their want of knowledge in the ludicrous certificates, of which one occasionally sees some published examples.[1] In looking through the reports for

[1] In the Journal of Mental Science (April, 1864) Dr. Hitchman gave a few samples of certificates. Under the head of "Facts indicating Insanity," one surgeon wrote "pneumonia;" another, "being restless, uneasy, and sleepless;" a third, "violent language to his wife;" and a fourth recorded the solemn fact, "*he calls me a fool!*"

1864, we have been struck with the accounts of the prevalence of variola, six asylums having been visited with this disease, and one of them (Cheshire) for three years in succession. In all instances, however, the cases have recovered, nor have they been numerous, except at Northampton.

Among the casualties recorded are several suicides, and two homicides; and it is curious to notice by what slight means the insane contrive to take their lives. Thus one woman, whose health was very weak, and who had made repeated attempts on her life, managed to suspend herself to the iron frame of a water-closet by means of a piece of tape, which she had torn from the petticoat she was wearing. At Lincoln, we read, a man "succeeded in destroying himself by strangulation with almost inconceivable rapidity, in broad daylight, and in the midst, as it were, of his fellow-patients and attendants."

At the Cheshire Asylum a case occurred in which the patient—

"Whilst in a sitting posture, placed her head in a loose loop formed of two neckerchiefs suspended from a wooden projection in the wall, and by forcing her head forward produced suffocation. So loose was the loop that her head was lifted through it without trouble; and neither at the time, nor up to the time of the funeral, did her neck exhibit any symptom of discoloration, nor was the face turgid, and there was such an utter absence of the ordinary appearances of suffocation, that her relatives would scarcely believe that her death arose from this cause."

In another instance—

"A woman secreted herself in a dormitory, and strangled herself by fastening a piece of list to a nail in the wall, on which a picture was hung. So determined was she, that, finding the nail would not support the whole weight of her body when hanging, she knelt upon her bed, and placing the noose round her neck, pressed downward upon the list, and so succeeded in strangling herself."

These cases illustrate the singular tenacity of purpose that some suicidal patients possess, and show with what cunning they will avail themselves of any slight opportunity to take their lives. Not less odd, and very similar in character to the above, is the following case, recorded by Dr. Parsey, at the Warwick Asylum:

"It is," he says, "one of those extraordinary instances of self-mutilation which, even among the insane, when not under control, are of extremely rare occurrence; especially when, as in the case now under record, adopted not as a means of mere mutilation, under the influence of a special delusion, but for the purposes of self-destruction. A man, while in gaol in another county, deliberately, and as he states, after several incisions, cut away the whole of his genital organs. He was, doubtless, at the time, in a state of insanity, and had been previously released from an asylum . . . . . . Spite of the painful inconvenience resulting from the injury he inflicted on himself, he still retains a dangerous taste for experimental operative surgery, requiring careful guarding against whilst his mental unsoundness continues."

But more horrible still are the homicidal attacks that are sometimes perpetrated by the insane; and one of these tragedies is related in the Hanwell report as follows:

"The death by homicide was of an aged, weak man, many years resident, who occupied a room with four others, one of whom, recently admitted, rose at early morning, and, under a delusion that this man was exerting a baneful influence upon and would seriously injure him, went to his bed, dragged him out of it, put him into his own, sat on his face and chest, bumped upon him, and so destroyed life, as asserted by another inmate of the apartment and admitted by the aggressor."

Dr. Begley gives as his opinion that "the act was probably one of sudden impulse on awaking from a frightful dream." The other homicide occurred at the sister institution, Colney Hatch. The only novelty we notice, indeed as regards treatment, almost the only point alluded to, is the use of bromide of potassium, of which extensive trial was made at the Northampton Asylum, and with very encouraging results. In speaking of epilepsy as a complication of insanity, Dr. Wing says:

"The number of epileptics in this establishment has averaged about 40, being nearly equally divided between each sex, and as proof that I have not exaggerated the beneficial results of the use of the medicine, I may add that during the last five months of 1863, 1012 fits were recorded as having happened on the male side; whereas during the first five months of 1864, when they were under treatment, the number of fits was 706, thus giving a decrease of 306 in five months. On the female side the beneficial results were not so marked, but still the decrease was 157—viz., from 1127 to 970. The mere diminution in the number of fits, however, is not the only happy effect of the drug, as it undoubtedly exercises a most powerful influence on the nervous system, and often soothes the irritability of epilepsy when no other medicine will take any effect."

It is well known that among the insane many forms of disease, especially phthisis, may develop themselves very insidiously, and that even the usual signs, expectoration and cough, for instance, may be wanting; and to meet this difficulty, at the Wakefield Asylum Mr. Cleaton has set on foot the practice of weighing the patients periodically, so that any failing in health may be tested by this means. The patients are weighed every month, so that any variation is easily traced, and this plan is one which has been proved to work very usefully.

At the Cumberland Asylum there occurred an outbreak of dysentery, and the causation of it opens out an interesting inquiry, for in the end it was, Dr. Clouston tells us, found to be distinctly due to the method of applying sewage to the land. In February last, a male patient was attacked with severe and intractable diarrhœa, which passed into dysentery, but as he had chest disease as well, the death did not attract any particular notice. In April, 3 other men were attacked and died, and in the next four months 23 other cases arose. The water was filtered, the food and milk, &c. analysed, and every measure that care could devise was employed, but in vain. At last, however, the cause was found to originate in the sewage tank, which is situated about 200 yards from the wards. From a small opening in the tank which was at all other points closed, an open cut conveyed the liquid part of the sewage over a field where it ran in open cuts, and the exhalations from these cuts appeared to be the sufficient cause of the infection. The proof of this consists in the success of the remedy adopted—viz., carrying off the sewage by a tile drain into a deep ditch at some distance, where it was largely diluted with water, for after this had been done no further cases of dysentery appeared. It was further proved that the successive outbreaks were explicable by the direction of the wind, which, when blowing from the north, carried the exhalations towards the house; and on looking at a meteorological record it was found that the wind had blown from the north shortly before each attack. The apparent mysteries of the disease then became perfectly clear. "The patients in the lower wards were chiefly attacked because the exhalations were blown along the ground in through the lower windows. The patients were attacked by twos and threes, and not one at a time, because the cause was operating upon a great number of them, and those most strongly predisposed to its influence at the time, succumbed to it."

This epidemic and its investigation may thus afford a useful lesson, and one not likely to be overlooked at this time when the question of the disposal of sewage is so anxiously discussed.

Dr. Clouston alludes in rather an amusing manner to one of the difficulties arising in the course of his management in the following words:

"It is often a very difficult matter to say when a patient is fit for discharge. Insanity has very frequently a blunting and weakening effect on the mind after all the active symptoms have passed off, which makes it necessary to take into account the previous character and disposition of the patient before he can be pronounced sane. Among an agricultural population, many of whom are without education and of primitive manners, the tests of sanity require to be of a different kind from what would be needed in a more intelligent and educated class. If a Cumberland ploughman looks heavy and stupid, never likes to wash himself or comb his hair, except on Sundays, wears his cap pulled over his eyes and his vest unbuttoned, tells you that Carlisle is the chief town in England, talks incoherently about things in general, and knocks a very insane patient down because he thinks he is making faces at him and insulting him, it does not follow that there is anything in his appearance, conversation, or conduct which indicates insanity, while in a first-class city mechanic those symptoms would undoubtedly indicate maniacal incoherence and excitement."

It is a matter of regret that the two great Middlesex asylums should still preserve the unenviable notoriety they have so long had. Hanwell, the birthplace of the greatest reform in lunatic treatment, and famous in all countries from its connexion with the name of Dr. Conolly, and Colney Hatch, almost equally well known, though it be chiefly for its size and expensiveness, are two of the worst specimens of asylums in the country, and to judge from the Commissioners' Report show but little signs of improvement. These are the institutions that foreigners principally visit, partly from their proximity to London, and partly from their fame, and one cannot but regret that strangers should carry away the notion that these are models of English asylums.

The Lunacy Commissioners in their last Report remarked of Colney Hatch:

"It would be difficult to instance more perfect examples of what the wards of an asylum for the insane should *not* be, than are presented here by what are called the refractory wards, especially those in the basement of each division, constructed originally so as to exclude the light from those portions of the corridors where it is most required. The gloom is unrelieved by comforts of furniture of the commonest description; the seats, notwithstanding the many paralysed and feeble persons, are fixed wooden settees in the windows, or long unbacked benches on either side the dining tables. A chair and a small table is hardly anywhere to be seen, and there are only a very few scanty prints upon the walls. Remarking to the attendants in the male division on the fact of some of these wards containing no chair whatever, we were told that the patients would fling them at each other if they had such opportunity; and to the inquiry why none of the male inmates had knives and forks to eat their dinners with, reply was made, that the men were all of them too dangerous to be so trusted."

At Hanwell, where there does not exist so acrimonious a warfare between the Lunacy Board and the Committee as is the case at Colney Hatch, the official report is not so damaging, though far from satisfactory. Perhaps the most curious feature in the Hanwell Report is that of the chaplain, who seems to combine medicine and theology in his functions. While dealing with singing and funerals, evening classes and services, he bursts out into a disquisition on General Paralysis, which he considers to have different effects on men and on women, and which he also thinks has its origin in syphilis. These are his words:

"If this malady be the effect of a dreadful contagious disease, as has been surmised, in the case of several of our married patients, it must have been contracted from their husbands. *This was undoubtedly the case of a woman* who cohabited with a man to whom she was not married, and who gave birth to an infant fearfully diseased, but which it pleased God to remove by death."

This is certainly an emphatic theory for the origin of the disease; but we are at a loss to perceive the propriety of a chaplain obtruding his speculations on the subject.

The report of Dr. Saunders, of the Devon Asylum, contains much medical information, several interesting cases being recorded, as well as some thoughtful remarks.

One case is that of a female, the subject of chronic mania, who had heart disease and emphysema, and who died from embolism of the carotids. On Feb. 28th, it appears, she had bronchitis, and great dyspnœa and congestion of the face and head; the next day the chest symptoms were better, but she became paralysed on the right side, with coma and stertorous breathing, and died in four days. On post-mortem examination the aorta was found to be much dilated, with patches of atheroma, and spots studded with earthy plates to which recent fibrinous clots were adhering. On tracing up the vessels from the aorta, the left carotid, at its point of division, was found to be plugged with a firm coagulum which extended into both the internal and external branches of the common carotid. On opening the artery, the clot was firm and spindle-shaped, fibrinous and pale, and completely plugged the orifice of both vessels.

Dr. Saunders remarks:

"Such a well-marked case of embolism does not often occur in the practice of the asylum; but it is probable that embolism is a not uncommon cause of paralysis and death among our patients. May not embolism of the pulmonary artery, and other vessels, be an occasional cause of the sudden death, which is usually attributed to syncope, of patients in acute mania? And may not thrombosis of the cerebral sinuses or embolia be found in epileptics who die suddenly?"

Another case of interest was that of an epileptic, who died in a fit, and on the surface of whose brain were found echinococcus cysts, ten or more in number, and varying in size from a hazel nut to a pigeon's egg. The head of the animal was found with the cephalic hooklets and discs.

At the Devon Asylum, too, the thermometer has been employed in the daily medical practice of the wards, and its use has fully justified the belief, we are told, that it will prove an important aid in the diagnosis and treatment of mental as well as bodily diseases. Dr. Saunders is sanguine that this instrument will take its place with others of our art—the test tube, stethoscope, and microscope; and he hopes that some of our asylums will make observations "of the typical curves of temperature in mania and other diseases, similar to those published by Wunderlich in Germany, and by Parkes and Ringer, on fevers, pneumonia, and phthisis, in our own country."

He thinks—

"Thermometry will prove to be of great service in the prognosis and treatment of acute mania, especially in those cases which partake of the typhoïd type of the disease, with sleeplessness, excitement, gradual wasting, and tendency to death from exhaustion; in them the temperature of the body is often from three to five degrees above the natural standard, and in proportion to the departure from this average (98° F.) is danger to be feared. Both the wasting of the body and the high temperature in such cases are due to the same cause—viz., the rapidity with which retrograde metamorphosis and the disintegrating processes are going on within the body, and are at all times certain indications for the employment of stimulants."

In general paralytics, as might be expected, the temperature is often below the average, but it rises on the access of excitement or of their congestive attacks. In one case, the "temperature of the body for some time remained at 96; but an hour after one of these congestive attacks, with twitchings of the extremities, the temperature rose to 105, and on the following day to 106. In this case the temperature indicated great danger, and the patient died thirty-six hours from the commencement of the attack." These hints are valuable in themselves, and seem likely to prove of great service in cases of phthisis and chest diseases.

The report of the Hants Asylum is remarkable for the singular nature of the obituary list. The deaths recorded in the year are 88, a large number, which Dr. Manley explains on the ground of the unfavourable nature of the cases admitted and the great age of the patients. Of the 88, however, we find that 67 deaths were accounted for as follows: general paralysis, 21 (7 women); pulmonary consumption, 14; apoplexy, 6; gastritis, 8; and general decay, 18; leaving 21 who died from heart disease, or other common maladies, except 1, which is attributed to the vague cause of "insufficient nervous power." The number of deaths from "general decay" or from "general paralysis" in an asylum of ordinary size is somewhat singular; but the pathology of those from "gastritis" is explained by Dr. Manley after this fashion. He says, these cases are—

"Generally connected with insanity in one of its acute forms, chiefly with melancholy, and consist of great disorder throughout the mucous membranes, accompanied with considerable congestion of the brain. All such patients are the subjects more or less of delusions, and all have a greater or less repugnance to take nourishment. It is very often fatal—the patients *dying of gastritis and typhoïd fever.*"

The death list is one of the most interesting pages in asylum statistics, and, perhaps, one of the most instructive. It is well known that the mortality is greater among the men than the women in asylums, and this is one cause of the difference in the number of the sexes resident. Dr. Palmer, of Lincoln, thinks that

"The true cause of this disparity lies in the fact that insanity in men is much more frequently associated with irremediable and destructive organic disease than in women, arising, probably, from their greater wear and tear of life and exhausting excesses, which shatter and destroy the great nervous centres rather than simply derange their functions; while, at the same time, insanity arising from nervous shock, or purely emotional causes, which is so common amongst women, rarely occurs amongst men, except in cases of strongly-marked hereditary predisposition."

With this view we quite agree; but it seems likely that as far as the deaths are concerned, the difference may be explained by one special cause—viz., the occurrence of general paralysis, which finds so many more victims among men than women. In order to elucidate this point, we have compared the deaths in several of the county asylums, and it will be seen that if those from general paralysis be excluded on both sides, more *women die than men.* Is it not a fair inference, then, that this particular form of disease constitutes the essential difference between the mortality of the two sexes?

The following table shows the deaths from all causes and from general paralysis in nine average county asylums:

TABLE I.—*Deaths.*

Asylum.	From all causes.			From general paralysis.				
	M.	F.		M.	F.		M.	F.
Sussex	36	21	*minus*	12	0	=	24	21
Dorset	16	11	"	5	1	=	11	10
Hants	46	42	"	14	7	=	32	35
Rainhill	30	29	"	9	5	=	21	24
Devon	42	40	"	14	5	=	28	35
Lincoln	28	15	"	6	2	=	22	13
Oxford	35	35	"	10	5	=	25	30
Essex	27	38	"	7	4	=	20	34
Somerset	39	20	"	9	1	=	30	19
Totals	299	251		86	30		213	221

From this we see that the subtraction of the cases of general paralysis actually leaves the female deaths in excess of the male, and if the inquiry be extended over greater numbers the result will be found the same. Thus, taking the statistics of twenty-four asylums, including Colney Hatch and Hanwell (which of themselves furnish 144 cases of general paralysis), it appears that the total number of deaths in these twenty-four was 1008 males and 843 females, of which 350 males and 85 females were general paralytics, leaving 658 males and 758 females as the ratio of the sexes, *minus* the paralytics. In other words: of the total number of deaths, 34·7 per cent. among the men, and 10 per cent. among the women, are attributed to this one cause. This fact is an important one in the consideration of asylum mortality, and one that has not been sufficiently dwelt upon.

With a view of analysing the history of this disease a little further, we have taken the nine asylums just alluded to and tabulated the deaths from general paralysis in decenniads:

TABLE II.

	Deaths. M.		Deaths. F.
From 20 to 30	5	...	2
„ 30 „ 40	28	...	14
„ 40 „ 50	36	...	8
„ 50 „ 60	12	...	3
„ 60 „ 70	5	...	3
Total	86		30

This disparity between the deaths in the two sexes becomes more marked by comparing the relative mortality from all causes with that from general paralysis alone, as seen in the following table:

TABLE III.—*Deaths from all Causes in Twelve Asylums for the same Periods.*

	M.		F.
From 20 to 30	56	...	33
„ 30 „ 40	97	...	70
„ 40 „ 50	121	...	76
„ 50 „ 60	131	...	65
„ 60 „ 70	71	...	68
Total	476		312

It is somewhat surprising that so little has been done towards the pathology of general paralysis—or *paresis*, as its newest designation is—considering how common a disease it is in asylums and how uncommon outside them. Since Austin's book there has been no English treatise on the subject, and the asylum reports seldom approach it nearer than to give the catalogue of yearly deaths. It is probable the term is used somewhat vaguely, and made to include chronic changes in the brain of various character; but properly it should be limited to certain physical deteriorations, preceded by mental symptoms of a definite nature. Speaking of its causation, Mr. Austin says:

"The comparative immunity of women from general paralysis, and their greater moral endurance, the frequency of the disease among men, and their ready prostration under calamity, go far to prove that an acute susceptibility to, and an intolerance of, moral pain, are among the most frequent causes of the malady."

We doubt whether such an explanation is a sufficient one; but the speculation is ingenious. In the work just quoted, the author gives his conclusions from an analysis of 147 cases, and they accord very closely with the table we have just given. He says:

"It appears that the period of the greatest frequency in both sexes is the decade between 30 and 40; the succeeding decade, however, nearly equalling it, the respective decimal percentages being 37·5 and 31·25 for males; and 33 and 29·4 for females. For all practical purposes, then, it may be stated that the vicenniad between 30 and 50 is, in both sexes, the especial epoch of general paralysis: 66·6, or exactly two-thirds of all the cases, having occurred during those twenty years of middle life." (p. 63.)

One curious fact that remains to be noticed, is the variation in the number of cases in certain districts. For instance, though it is well known to be more frequent in towns or places of much enterprise and activity, we were not prepared to find such a difference in several agricultural counties. In the Wilts Asylum, with 385 patients, there was but *one* death from general paralysis last year; in Shropshire 8, with almost the same average of patients; at the North and East Riding only 4; while at Stafford there were 17; and at one of the Lancashire asylums 55.

In taking leave of these Reports, we are glad to think the asylums are in so satisfactory a state, and can only hope for richer medical harvests every year. Dr. Thurnam, we see, promises an analysis of the weights of the brain in 470 cases, and hopes "that these weights will be found useful by those who may be engaged in inquiries connected with the physiology of the brain, and the circumstances influencing the size and development of this organ." We wish he had more imitators.

REVIEW VII.

1. *The Croonian Lectures for* 1864, *delivered before the President and Fellows of the Royal College of Physicians of England: the Significancy of Dropsy as a Symptom in Renal, Cardiac, and Pulmonary Diseases.* By W. R. BASHAM, M.D.—*London*, 1864. pp. 86.
2. *On the Relation between Anasarca and the Renal Affection at the commencement of the so-called Acute Renal Dropsy, &c., &c.* A Thesis for a Medical Act in the University of Oxford. By JAMES ANDREW, M.D. 1864. (Pamphlet.)

WE are indebted to Dr. Basham for giving to the profession the results of his close investigation of one special symptom, common to several and diverse pathological states, and for his practical deductions on the subject of dropsy.

Lecture I.—passing by that form consequent upon loss of blood —is devoted to the consideration of dropsy associated with changes in the kidney. The advances which have been made in the more accurate appreciation of the pathology of dropsical affections were set on foot thirty years ago by the late Dr. Bright. The scalpel could, however, add little more to the then existing knowledge than an accurate description of the evident and naked-eye appearances of affected tissues; and it was reserved for subsequent labourers with the microscope to penetrate more deeply into the causes of these appearances, and so to place general pathology on more decided ground.

To the nucleated element of the cell both secretion and excretion have been traced. Mr. Goodsir came to the well-known conclusions: "That all true secretions are formed or selected by a vital action of the nucleated cell; that the secretions are first contained in the cavity of that cell" and he added, "that both growth and secretion are identical, the same vital process under different circumstances." (p. 7.) To use as most appropriate the author's own words, we may say before proceeding to the more close consideration of the subject: "If then from the physiological and anatomical point of view we are taught to recognise the nucleated cell as the fundamental source of the vital processes of secretion and development, so must we from the pathological stand-point equally regard the nucleated cell as involved in the process of disease." (p. 8.)

Attention is then directed to the characters of the healthy epithelium-cell of the renal tubule; and while this manifests faintly opalescent contents with well-marked cell-wall and nucleus, we find that in the early stages of renal mischief with albuminous urine and dropsy, the cell is apparently increased in size, that the nucleus is seen but with difficulty, and the cell-contents become cloudy and granular; very shortly afterwards epithelial cells, all of them imperfect or degenerate in structure, are thrown off, either isolated, or in close aggregation, constituting the tubular epithelial casts. A dropsical state of the whole body, more or less decided, is a constant accompaniment, the blood, too, is altered in composition and physical character, the water and colourless cells are increased, while the red corpuscles are diminished.

Acute cases of this type, too frequently fatal despite any kind of treatment, still afford abundant proof that, although the disease may seem most thoroughly to have affected the kidneys, the cell development in other organs is equally deteriorated. More decidedly can this wide-spread failure be traced when the case is more chronic, more insidious in progress; the epithelium-cells of all the mucous membranes, whether they are devoted to purposes of secretion, or are merely protective in character, exhibit a uniform departure from the healthy type; they swell, become cloudy, and disintegrate readily; imperfectly developed epithelial cells, once declared by Gluge to be products of inflammation, are present with their highly-refracting nuclei; and smaller cells also, with nuclei altered in form, and merging into the trefoiled pattern of the true pus-cell nucleus, may be found in rapid succession.

Interior structures of the body give evidence also of a general degeneration: thus the muscular walls of the heart are spoiled, the muscular striæ are lost in places, and granules soluble in ether, a mere fatty débris, occur within the enclosing membrane, and the pericardium exhibits opaque white patches, seats of degenerated tissue; and the liver-cells also are charged with large resplendent fat granules (p. 22).

Hence is implied in this form of dropsy no mere local kidney disease, but a far more general depreciation and decay of the cellular elements of the tissues.

Lecture II.—The treatment of the disease above-named is here entered into. Before the question of treatment, two points of great interest receive notice (p. 27): 1. The nature and origin of the urinary tube-casts in morbus Brightii. 2. The source and channel whence the albumen is derived in the same form of disease.

Dr. Basham traces an analogy existing between the so-called waxy or hyaline casts of the renal tubes, and the matters thrown off from the bronchial mucous membrane when this latter structure is irritated or inflamed.

The cell-products differ, it is true, in the absence of a tubular arrangement or grouping, but are in other respects declared to be identical with the cell-formations found in any long-standing case of morbus Brightii. The sputa of capillary and plastic bronchitis present appearances more strictly parallel to those of the renal casts, specially to the so-called "granular epithelial cast." With the casts of the inflammatory or acute stage of "morbus Brightii," the sputa in pneumonia have much in common.

The reader will be able to compare these different appearances side by side, since some well-executed drawings of the morbid excreta appear in the book.

This, then, is the deduction:

"That varieties of abortive deteriorated and depraved cell-growth, present in the excreta from the various forms of pulmonary disorder, are identical in appearance with those which are seen in the renal casts—the only distinction being in the apparently tubular form of the one, and the more diffluent aspect of the other. . . . . In pulmonary diseases, these excretory matters are derived from the metamorphosis of the epithelial elements of the bronchial mucous membrane. Can we hesitate to adopt the proposition, that strictly analogous appearances occurring in the renal tubes are also derived from the metamorphosis of the abortive epithelial elements?" (pp. 30, 31.)

To the general belief, that the albumen found in cases of albuminuria more or less abundantly is due to a percolation through the Malpighian capillaries, the lecturer opposes the objections, that the explanation is too mechanical, and that, were this theory true, the watery character of the blood would be diminished, while the absolute converse of this holds good. He would rather refer its existence to the abortive tube-casts, or to those disintegrated and imperfectly-formed epithelial cells, which take the place of the vigorous and true gland-cell of the kidney.

Very much weight is laid, when the appropriate treatment of this disease is considered, on the paramount necessity of bearing constantly in mind, that this form of disease represents and embodies an all-pervading degeneration and decay.

Therefore blood-letting, as directly opposed to this fundamental principle, is reprehended; while rest, warmth, nutritive stimuli, and blood-forming remedies are to be employed to restore to its normal condition of health a circulating fluid, which observation has shown to be in this class of maladies constantly deficient both in fibrin and in blood-corpuscles. Special remedies will serve a good purpose in selected cases, and may not unfrequently prove of essential service.

Various stimulants (whatever be the truth of chemical dicta on the question of their employment) have a tangible clinical value; for they enable the system to take up and to appropriate more thoroughly nutritive material. In renal dropsy, the author would without hesitation employ these agents when once the early feverish stage has gone by; preferring wine to any other fermented liquor, ordering it in moderate quantities, and always in combination with food.

The preparation of iron noted as of most value would seem to be the tincture of the sesquichloride, given, however, in combination with liq. ammon. acetat., first acidulated by acetic acid: so that an ammonio-chloride is really employed constantly held in solution by the excess of acid.

Lecture III.—To illustrate the subject of dropsy, as dependent on cardiac or pulmonary disease, two cases are sketched: the one, an instance of mitral and aortic deposit, occurring in a young person, and consequent upon an attack of rheumatic fever; while the other, in a patient of middle age, exhibits the symptoms of heart disease secondary to long-standing pulmonary affection. The one case has reference to the valvular structures of the *left* side of the heart; while the other stands in relation with the state of the ultimate muscular fibre, and the capacity of the cavities in the *right* side.

Valvular deposit is especially a result of rheumatic fever: the degree and extent of the fibrinous deposit will in some measure determine the pulmonary disturbance which may be expected to follow.

Virchow has of late shown reason to doubt the impression that these deposits are derived directly as an exudation from the blood. He maintains that the cellular elements of the endocardial membrane take up a greater quantity of material; the spot then becomes swollen, and eventually rugged excrescences arise which destroy the integrity of the affected valve.

Subsequent changes occur.—1. The thickened patch may become the seat of the deposition of carbonate and phosphate of lime, "calcifying process" (p. 64). No further change in structure can then occur, and although permanent murmur will be present, the general health will not of necessity suffer. A frequent consequence, however, is that of hypertrophy of the left ventricle; and the extension of the same calcareous deposit to the aorta and cerebral arteries may determine the occurrence of apoplexy. On this calcified condition of valves, dropsy does not follow.

2. Or again: The deposits in the valves may undergo fatty degeneration, and then the microscope will detect granules of fat, with cholestearine plates in the material.

It is maintained, then, that these changes are brought about by the agency of the cellular elements of the exudation itself, "prone to take up earthy matter in one case, or to undergo fatty metamorphosis in the other." (p. 69.)

When these local deteriorations of the valve-exudations occur, analogous changes in cell-growth are coincident in other parts; and on this new structural change the symptom of dropsy results. The mechanical impediment to the circulation is there from the early stages, but dropsy does not show itself *ab initiô*.

In the other form of heart disease, when the cardiac change is secondary to pulmonary complication, the same decay of tissue may be traced.

The pulmonary cells are dilated, and the vascular walls of the pulmonary capillaries are atrophied, as the consequence of a granular and fatty degeneration of their textures. This condition is co-existent in the heart-structure, and probably simultaneous in origin. "I have never examined a heart in a case of cardiac dropsy with emphysema, in which this fatty degeneration of the muscular fibre of the right heart did not exist to an exaggerated extent." (p. 79.)

As the natural inference from these conditions, the author points out the propriety of directing quietude, steel, and animal diet in those cases of valvular mischief in young people which follow on rheumatic attacks.

In the other set of cases where cardiac mischief results on pulmonary disturbance, a parallel plan of supporting treatment must be followed. The constant expectoration of chronic bronchitis implies an abundant waste of cell-formation; and to meet this symptom alone the general powers must be sustained.

In every case of emphysema and chronic bronchitis, long before cardiac complications show themselves, generous living, fresh air, moderate exercise, and strict avoidance of depressing remedies, form the principal rules of treatment.

The thesis by Dr. Andrew commences by exhibiting in tabular form a classification of renal diseases, according to their pathology—a laudable endeavour to place these affections on a secure basis, yet one which is not without some very palpable objections. Can it, for example, be maintained that an observer can *always* determine the *point de depart* of a local kidney disease?

The essay is occupied by the consideration of acute renal dropsy—that kidney affection with which anasarca is coincident, or upon which it is immediately consequent. The local changes are first noted: "Whatever the nature of the exciting cause of these changes, the first in the series would seem to be some alteration in the nutrition of the epithelial cells lining the Malpighian capsules, and the tortuous portion of the *tubuli uriniferi.*" (p. 4.) The cells become more numerous and larger, not varying materially from the healthy standard; congestion of the whole organ, and an increased supply of blood, occur therefrom as a secondary not as a primary condition; hence the kidney enlarges, and a large, smooth, and watery kidney is produced.

The contents of the tubules are also altered; a transparent fibrinous material is found in them, and this moulded to the tube, and then washed away in the urine, is recognised as the urinary cast.

The physical changes in the kidney are dwelt upon as occurring in the following order:

1. Hypertrophy of the epithelium.
2. Impairment and perhaps perversion of function.
3. Congestion.

The succession above named is chosen in obedience to the prevailing opinions on inflammation, the change of texture determining the congestion of a part as its secondary consequence.

The term "hypertrophy" is not selected as thoroughly adequate to the expression of the affection of the epithelium, but because it brings prominently forward the best-marked phenomenon of that condition. In his expressed opinion on the cells, Dr. Andrew is at issue with the Croonian lecturer; thus:

"For though it cannot be supposed that the properties of the hypertrophied epithelium are identical in all respects with those of the same structure in health, still the increase in the number of the cells is almost the only change the reality of which can be conclusively established. At any rate, I have never been able to arrive at any definite characters by which isolated cells, obtained during the early stage, can be distinguished from normal renal epithelium." (p. 10.)

The "large red" kidney is said to merge into the "small granular" kidney; and the atrophy of the separate portions would seem to be regulated by the amount of difficulty with which blood reaches them; the morbid changes being best marked when the circulation is most interfered with.

The spots of congestion in the large red kidney are stated to be due to local extravasations of blood in the tubules. As the disease advances, the pyramids alter in appearance: *pale* while active congestion is present, and in comparison with the *red* cortical part, they become in the later stages *red*, while the cortical portion appears *white*.

The renal disease in question, and the dropsical symptoms often coincident with its onset, are stated by Dr. Andrew to have no necessary connexion with each other. "The albuminuria and the anasarca must be looked upon as being co-ordinate effects of one common cause;" some previously existing blood disorder being, in all probability, the essential and determining element.

Both these works are well worthy a careful perusal: the one from its suggestive and thoughtful character, the other eminently practical, as furnishing a definite ground for a philosophical treatment of the whole class of dropsies, and as supplying a link in the yet only too imperfect chain of physiological inquiry and therapeutical endeavour.

Review VIII.

1. *Manual of the Turkish Bath. Heat a Mode of Cure and a Source of Strength for Men and Animals. From the Writings of Mr. Urquhart.* Edited by Sir John Fife, M.D., &c.—*London*, 1865. pp. 419.
2. *Bathing: how to do it, when to do it, and where to do it.* By E. Sheppard, M.D., &c.—*London*, 1865. pp. 31.
3. *The Anglo-Turkish Bath; or, the Modern Application of the Ancient Roman Therma as a Hygienic, Prophylactic, and Therapeutic Agent; with Practical Suggestions how and when it should be employed.* By Y. J. Moore, M.R.C.S.—*London*, 1865. pp. 58.

The first of the above publications is in many respects a remarkable work, both for the information it gives and the expectations it raises. Should half of these expectations be realized, Mr. Urquhart will certainly deserve to be considered a benefactor of his country and of mankind, as the discoverer in the air-bath of high temperature, of a very powerful medical means for a large number of the diseases to which man and his domestic animals are liable.

It has been long known,—i.e., since the interesting experiments of M. Tillet, related in 'The Memoirs of the Academy of Sciences for 1764,' and those of our countrymen, Dr. Fordyce, Sir Charles Blagden, and others, of which an account is to be found in 'The Philosophical Transactions for 1775'—that a very high temperature as high as the boiling-point of water, and even higher, may be endured for many minutes in dry air with impunity, and without the production even of any very disagreeable sensations. Nor is the application of heated air as a curative means in certain ailments a novelty. What appears to us to be the chief merit of Mr. Urquhart is, his having brought into use the hot-air bath with details as to its construction and operation, in the accomplishing of which he has shown a zeal and perseverance deserving of all praise, and the more especially as his sole motive appears to have been the desire to do good, impelled by the enthusiastic belief that the means were adequate to the end.

The title of the work, and a considerable portion of its contents being in the form of colloquy, aided by the spirited manner in which for the most part it is written, are altogether well adapted to make it popular, to arrest attention, and to enforce at least the belief that the bath as described is deserving of a fair and extended trial.

Regarding the title itself some objection may be made, as the stress laid on the Turkish bath is liable to mislead, that bath being a vapour-bath of comparatively low temperature, its sudatorium according to our experience, when we have tried it at Constantinople, not having exceeded 98° Fahr., whilst the bath, the special one of Mr. Urquhart, on the contrary, is a dry hot-air bath, of a temperature which according to him should not be less than 150°, and is, he says, of greater efficacy of a still higher temperature, as when ranging from 150° to 200°.

It would appear from the particulars which he relates, that he first derived the idea of the curative power of heat from the late Lord Dundonald, that sailor of marvellous enterprise and invention, better known as Lord Cochrane. Mr. Urquhart says:

"I began with myself. I have been requested to relate to you [he is addressing an audience at Newcastle] how I came to the discovery. I said that Hippocrates had made it—it had to be made a second time. But I was not the discoverer, and I will tell you how the idea first arose; the same might have happened a thousand times without becoming a point of departure. I was suffering from ague. I was on board a steam yacht belonging to Lord Dundonald, whom England has recently lost. He said to me, 'Heat must beat cold,' and rolling me in a blanket, being a strong and tall man, he took me under his arm and carried me down to the stoke-hole, and stopped the fit."

Other personal incidents Mr. Urquhart relates, which brought conviction to his mind of the great efficacy of pure heat as the antagonist of disease, followed by another conviction, founded also on personal experience, that to derive the fullest benefit from heat it must be of the radiant kind, and this conviction by a third, that diseases may be known by the odour of the patient, and that the test of cure is the removal of the distinctive morbid smell by elimination, by perspiration of the impurities of the blood, and the restoration of the natural sweet smell of the human body, which he likens to that of "fresh-sawn fir boards." These three, which we have designated convictions, he calls discoveries, and on them a large portion of the work is expended. Relative to the construction of his bath, there are many details and some illustrative plans. The main requisites are a heated metallic surface for warming the atmosphere of the compartment, and a tank containing water either cool or warm for "revelling in." We give the description of the one which he speaks of as the most perfect he had yet built, and the least expensive, having cost only 37*l.* with all its fittings for the supply of water hot and cold:

"There is a furnace; two couches, one raised, and receiving the close and full radiation of the red-hot metal, the other movable in the floor; also a tank, to which you descend two steps, there being a slab to prevent splashing. You may have it hot, tepid, or cold water, as well as streams of hot or cold water, from pipes arranged so that the stream gushes out as from a rock. The water is heated by being in tanks close to the ceiling. The water in the tank may be three feet deep; but one foot to twenty inches is enough, for sitting in or boiling [pouring ?] over you, an agreeable operation, and furnishing a necessary diversion, when by cause of malady the bath has to be endured for many hours of the day. The floor is cement. There are no flues. From a common laundry stove, an iron pipe conveys the smoke across it. There is no plate glass, but only a double sheet of crown glass let into the wall for a window. I use coke instead of coal, so as to keep the pipes clean, that the heat may be given off; no coal is used, even for lighting the fire. At a heat varying from 180° to 230°, night and morning, the fuel consumed will not amount to more than one-half of what is required for an ordinary fire. The ventilation is by suction from below. The air is drawn into the furnace; there is a constant and rapid circulation up one side and down the other. Entering the furnace heated, the combustion is more perfect; the bad air is consumed, and the saving of heat enormous. The fresh air being admitted from above, it comes in its passage downwards to the bather to breathe. There are three plugholes adapted for convenience of the bathers, so that when a high temperature is required for a long time, they can refresh themselves, or even envelope the whole head in a hood, and breathe as in the open air. There is no difficulty whatever in thus obtaining perfect command of the circulation. The suction being everywhere *inwards* and *downwards*, crevices are easily stopped. The door must, however, open outwards and with a screw hinge, or a flap pivot out of the perpendicular; the edges being bevelled, the door falls-to close at every point. The command of the admission air is such that I am independent of the use of the damper; and when the door and plugs are closed, the draught through the furnace and flue is stopped. An invalid can be carried in on a bed or chair. The platform on the outside is on the level of a window, that can be thrown open to a current of air; or the ante-room can be closed, and by leaving the bath door open 120° can be obtained. There is but *one step* from the hottest point to the open air or into the tank. The cocks have particularly to be attended to. The turning part must be equal to the running part, otherwise the water will come out wabbling. A space sufficient for washing must be allowed between the cock and the basin; neither of these points an English tradesman understands. The cocks must be large, so as to admit of a full stream. The escape from the tank must also be free, so as to change the water rapidly. It is $2\frac{1}{2}$ inches diameter. In stating the cost to be no more than half an ordinary fire, it has to be observed that refuse coke or braise answers better in a stove, and does not cost more than half. Therefore such a fire can be kept up for 2d. a day. But even were the expense equal to several fires it would be an economy; by means of it all the passages in the house can be heated, and several of the rooms besides, having always at hand a drying closet. I have myself not only dispensed with seven fireplaces, but taken them out. With open fire-flues and straight chimneys, fully twenty parts of the twenty-four of heat are lost; managed in this way, twenty parts of the twenty-four are retained. Such an apartment would be an economy and an advantage in any house, wholly irrespective of the uses of which I am at present treating. On the other hand, the incalculable benefits to the sick from the bath can never be otherwise realized; for the patient to secure them, he must have it under the same roof, so as to be able to go in at any hour of the day or night, and to have it within reach in case of acute disorder.

"This bath (he says) is the only one I have constructed specially for invalids. It was made in view to the confinement of a lady, nevertheless it thus loses none of the utility or the charm that may be sought by the healthy. When I come back to it after the absence and the use of other baths, even the best, it is like getting on the back of a thoroughbred after having to ride a cart horse. It is of service at every moment and at all temperatures. You come in from a journey, say before dinner; you go in not heated, when it may stand at 120°; you dress in that charming temperature, with streams of hot or cold water, or the tank to revel in. So also, you may dress in the morning. My regular practice, when not requiring it for health's sake, is to go in on getting up and on going to bed, dressing and undressing there. Five to ten minutes suffice to bring on the flow of perspiration. After that, a plunge in the cold water, and you come out fresh, glowing with a sense of cleanliness, health, and strength, which no other operation can convey to the body. You are then indifferent to the heat of summer and the cold of winter. If time is an object, the waste of time is thus no objection."

He continues:

"I will give an instance of its use as a substitute for exercise. One wintry day, feeling the want of exercise, I was deterred by the aspect of the skies; there was sleet, and a bleak easterly wind blowing, so I bethought me of going into the bath instead for half an hour. I found it particularly agreeable. Instead of half an hour, an hour and a half had flown by; so, as I looked at the thermometer, and observed that the bath stood at 176° and the tank at 76°, I had been passing from the one to the other—say ten minutes out of the tank and five minutes in it, alternately. I noted these points as furnishing a datum line, giving, without strain, the contrasted enjoyments of the temperate and torrid zones. I left the bath invigorated, as I might have been by a gallop of fifteen miles under a bright sun on the Sussex Downs."

For an account of the bath on a larger scale and on a more elaborate plan, we must refer our readers to the work itself. The bath in Jermyn-street, one of the first magnitude, the property of a limited company, was erected at a cost of 20,000*l*. Of course, any amount of money may be expended on luxuries, furnishing, and ornamentation. It is satisfactory to find that what is essential as regards efficiency may be obtained, as in the cottage bath described, at a very moderate outlay.

That Mr. Urquhart has made many converts to his views there cannot be a question; the number of baths already constructed affords ample proof. In England, not to mention private baths, the number of public baths is seventeen, which have been erected within the last six or seven years, and have been used by 502,870 persons. Of these, one is in the Newcastle Infirmary, and during five years from its construction has been tried in 23,440 cases. The editor of the book, Sir John Fife, to whom the erection of it was owing, reports very favourably of its efficacy. He says: "I state as the result of my experience, that in diseases of the skin, joints, liver, and kidney, the action of the Turkish bath [the hot-air bath] is immediate and direct." He adds:

"When it is remembered that in most diseases the important viscera above mentioned are deranged in their action, we see at once in how vast a number of diseases the Turkish bath, by correcting the morbid action of these viscera, must inevitably exercise an influence beneficial and powerful, though *indirect*."

In the Report of this Infirmary, the following diseases are specified as having been submitted to its trial with satisfactory results—viz., acute and chronic rheumatism, lumbago, neuralgia, sciatica, gout, dropsy, catarrh, influenza, many affections of the throat and air-passages, diarrhœa and dysentery, cases of "congestion or stagnation of the blood," hernia, stomach and liver affections, *scrofula* affecting the *integuments, glands,* and *joints, incipient phthisis,* and other conditions attended by wasting; further, confidence is expressed of its beneficial influence in the treatment of disorders peculiar to females and in cases of hypochondria in men. Other reports are given also highly in its favour. Of these, one is by Dr. Leared, which was published in the 'Lancet' of November and December, 1863, of its effects in pulmonary diseases, including phthisis. Such are this gentleman's expectations of its efficacy, especially in the early stage of the last-mentioned disease, that he declares, in conclusion: "If it unfortunately fell to my lot to be affected by phthisis, I should give the hot-air bath the fullest trial." There are also reports and statements of cases favourable to its use in cancer, leprosy, hydrophobia, epilepsy, tendency of blood to the head, Bright's disease, scarlatina, ague, chorea, mental disease. Relative to the last-named, the authority is Dr. Robertson, medical superintendent of the Sussex Lunatic Asylum at Hayward's Heath, where a bath has been established.[1] Writing in 1863, he states:

"My experience of the use of the bath has hitherto been chiefly

[1] See Dr. Robertson's Cases illustrating the Use of the Roman Bath in the Treatment of Mental Disease, in the 'Journal of Mental Disease,' July, 1862; also the description of a Case of Mania with Albuminuria cured by the Roman Bath, in the 'British Medical Journal,' Feb. 1864.

limited to cases of chronic mental disease."—"My great success has been with cases of melancholia with refusal of food and loss of strength and flesh."—"In irregularity of the uterine functions I have found, in several instances, a cure follow the restoration through the agency of the bath of healthy uterine action."

After mentioning that the bath removes "the noxious secretion of the skin so frequent in the insane," whence their peculiar offensive odour, he adds, in recommendation, "Lastly, the bath is a remedial agent grateful to the feelings of the insane, and which they do not, like other means of washing, associate with the idea of punishment."

The reports given of the efficacy of the bath in diseases of horses and cattle, though fewer in number, are not of a less favourable kind. One is by J. E. Scriven, Esq., on an Irish farm; the other from the experience of Lord Kinnaird. It is stated: "In Lord Kinnaird's bath for cattle, there having been no trouble either with patients or medical men, the necessity for high temperatures for the cure of grave disorders has been ascertained." The words of a report in the 'Scottish Farmer,' 1862, are: "The heat required in cases of pneumonia needs to be very great—up to 200°, to 212°—boiling point, in fact. At a less temperature the curative effect was not visible."

Here, certainly, is an amount of evidence in favour of the bath of a most respectable kind, such as even the medical sceptic must respect. We repeat, it should be kept in mind that the bath of such great promise is not the Turkish bath, the vapour-bath of low temperature, but the air-bath of high temperature. How it acts, if it can be proved by experience to be so very efficacious, is quite a secondary consideration. Mr. Urquhart, in attempting to account for its effects, associates it with electricity, with the dictum "Electricity is life," and that radiated heat from red-hot iron is similar to that of the sun. These are hypothetical ideas, especially the first, and if true of the latter only in part, a large portion of the heat derived from a stove not being radiant. Such air as that of the hot-air bath is eminently dry, and, as is well known, the drier the air, whether with a low temperature or high temperature, the worse conductor of heat it becomes; and, in proportion, the more endurable. The experience of the Arctic explorers fully confirms this as regards low temperature, and that of M. Tillet and Sir Charles Blagden and others, already referred to as regards high temperature. What the immediate effects of highly heated dry air are on the living body have not, we think, been yet sufficiently investigated. Independent of the sensations excited, we learn little more from Mr. Urquhart's book than that sweating is copiously produced; no record appears to have been taken either of the pulse or of the temperature of the body. The only physiologist that we are aware of who has attended to these points is Dr. John Davy. He found the pulse of men employed in a steamer within the tropics in charge of the fires to increase greatly in frequency, and the temperature of the body as measured by a thermometer placed under the tongue to rise proportionally. We quote an instance, that of the chief engineer, a robust, healthy man, about thirty years of age, who had been below two hours seeing to the engines, but not in the hottest parts: "After an exposure of twenty-five or twenty-six minutes to a temperature of 111°, the temperature under the tongue was 102·3°; of the closed hand 100°; his pulse 142°, and not feeble. He was profusely sweating. He said he had no uneasy sensation.[1] It is stated by the same author, that the firemen of the West Indian steamers are less subject to fever than the crews generally. And here we may remark that Mr. Urquhart has a hypothesis on the subject of fever founded on what he considers two facts, but which we cannot admit as such: one, that fermentation cannot be excited under 90°; the other, that the fever heat is 112°; and he reasons, that as fermentation, zymosis, is arrested at 140°, if—to use his words—"I can get the blood subjected to a heat of 140°, I will take the inflammation, that is to say the fever, out of it." This we need not comment on. Nor shall we comment on his speculation, that in the majority of diseases, when the hot-air bath is efficacious, it is, as he supposes, by the washing of the blood by means of sweating, the fluid exuding in sweat, according to him, removing the *materies morbi* contaminating the blood.

We must confess, that whilst we hold in much respect Mr. Urquhart's acuteness of observation and practical turn of mind, we have not quite the same feeling relative to his speculative views, or his estimates of different races of men and their habits. He thinks it a mistake to wear clothing, forgetting that man is the only one of the mammalia not provided with natural clothing, and that the warmth or bad-conducting power of the covering of other animals of this class increases in proportion to the degree of cold to which they are subjected; he thinks that exposure of the naked body to the sun's rays is beneficial, though in his own case such exposure was followed by a painful cutaneous eruption; he holds the opinion that man should eat only once a day, and that the full repletion of the stomach is most conducive to digestion; he considers the Turk superior to the European in cleanliness; water, he maintains, is rendered impure by our mere contact, and that to wash, so as to secure perfect purity, water should be poured over us; the skin he views as the main purifier and emunctory of the body, regardless very much of other excreting organs, the kidneys, lungs, and liver, altogether of far

[1] Physiological Researches, p. 39.

greater potency. Such extreme views, with some paradoxes, he supports by a ready command of words, and as, we think, a notable disregard of facts—i.e., large experience, not isolated instances. Our limits do not allow us to discuss these several matters. Those who read the work will make their own reflection; and those who have been in the East and have had experience of Mussulmans' habits will not be disposed to consider the Turk—we speak of the common people, who sleep in their clothes, paying little attention to their cleanliness—a cleaner people, or a better, than the majority of Englishmen, though they may be, as Mr. Urquhart describes them, more polished and formal in their ways.

What we wish to fix attention on most is the hot-air bath, and the question of its fitness for general use, and whether we have sufficient evidence to enable us to conclude with any confidence that it is safe and efficient in the treatment of diseases, and is superior to *every other* mode of purification. That we have evidence of the latter we hesitate in pronouncing: we hope it will have every trial as a therapeutic agent. It would be very desirable to have ascertained, besides its effects on the pulse and the temperature, whether it has any effect on the corpuscles of the blood, and what are the substances which are discharged from the system by the skin when under the influence of the hot-air bath. For ordinary purposes of cleanliness and personal comfort, we think daily ablution, with good rubbing from head to foot,[1] adequate, whether hot water is used or cold, or Castile soap or any other quality of soap, especially when occasionally aided by the warm bath or a vapour bath not exceeding 98° or 100°. The hot-air bath we cannot but think hazardous when employed carelessly, and the air heated to and above 150°. The author whom we have quoted relative to the effects of hot air in a steamer on the pulse and temperature, mentions how a stoker who, after four hours' attendance on the engines, was struck down by dangerous apoplexy; and in the report of the experience of Sir Charles Blagden and others, it is specially stated that "they experienced some inconvenience; their hands shook very much, and they felt a considerable degree of languor and debility. Dr. B. had also a noise and giddiness in his head."

Man, it is well to remember, is the hardiest of animals, and it is astonishing how much he can bear. How else could he be so widely spread over the earth's surface? how else can we account

[1] The glove used by the Turks, made of the hair of the Angora goat, is, we think, greatly preferable to any other, and it is to be regretted that the cheap material of which it is made is not imported into this country. Of soaps, perhaps the glycerine soap is best for the skin; the Castile soap, recommended by Mr. Urquhart, contains alkali in excess.

for the many curative methods, as wide as the poles asunder as to their nature, which have from time to time been in vogue? The cold bath has had advocates for its medicinal use almost as enthusiastic as is Mr. Urquhart for the hot-air bath; and at the present time we have little doubt that the partizans of hydropathy and homœopathy will maintain faith in their creeds, whatever success may attend the calid method of cure. We could wish that the hot-air bath should be made the subject of minute scientific inquiry by competent men, as by a committee of the Royal College of Physicians or the Royal Medico-Chirurgical Society, and be reported on after due research, after the manner adopted in France by the Institute and the Academy of Sciences. In its physiological aspect it might with propriety be brought before the British Association for the Advancement of Science for discussion in its zoological section.

Before concluding, a few words may suffice respecting Dr. Sheppard's and Mr. Moore's pamphlets. That of the first-named gentleman, the skilful Resident-Physician at Colney Hatch Asylum, is well adapted to make the reader acquainted with Mr. Urquhart's plan, of which he is a warm admirer. The cautions he gives as to the impropriety of getting cool before using a cold bath or taking a draught of cold water, have long had the approval of his professional brethren. He might have given with propriety another caution—viz., to beware of taking a cold bath or a drink of cold water when hot *after exhausting fatigue*. That of the latter, chiefly addressed to the non-professional reader, without pretension to any originality, may be perused with advantage, as showing how much exact inquiry is needed to determine the special effects of the hot-air bath and of the vapour-bath, and their proper uses.

The literature of baths, we need hardly remark, is very copious. We may refer those of our readers who wish to refresh their memories on the subject to some of our former volumes. In the fourth volume for July and October, 1837, containing a notice of Dr. Oppenheim's essay on the state of medicine, &c., in Turkey, a description will be found of the Turkish bath, with a good deal of information respecting the Turks, nowise in accordance with Mr. Urquhart's high opinion of that people. In the seventh volume (new series) for January and February, 1851, various kinds of bath are described, including the Roman, and their uses and abuses. And in volumes twenty-three and twenty-nine, some of the latest works on balneology are noticed and commented on.

Review IX.

Transactions of the Epidemiological Society of London. Vol. II. Part I. Sessions 1862-63, 1863-64.—*London,* 1865. pp. 246.

This volume contains eighteen papers. These we propose to notice seriatim and briefly; indeed, owing to our limits, more briefly than in some instances we could wish.

I. *On an Anomalous Exanthem: Rosalia Idiopathica.* By B. W. Richardson, M.D.—The author in this paper calls attention to cases of eruptive disease of ambiguous character, in some respects resembling scarlatina, in some measles and nettle-rash, of which one under his care proved fatal. The conclusion he draws, after some ingenious reasoning, is, that the disease is the same as that to which Dr. Mason Good gave the name of "Rosalia Paristhmitica,'" and for which, holding it to be one *sui generis,* he prefers that of "Rosalia Idiopathica." He considers it non-contagious, and probably produced by some article of diet poisoning the blood.

Whilst we think that the author's conclusions are based on too limited an induction of facts to justify the name given by him, we fully appreciate the importance of the inquiry concerning these obscure eruptions, often, as it were, of a hybrid character, perplexing the most experienced, and as difficult to treat as to name. The only way, we apprehend, that the obscurity in which they are at present involved can be removed, is by their careful study, and ampler details and collation of facts.

II. *On certain Endemic Cutaneous Affections observed in Schools, Factories, and Workhouses.* By T. Hunt, Esq.—This paper we would willingly pass over, merely remarking that the doctrines advocated in it, so far as they are peculiar to the author, seem to us—will he pardon the expression?—not a little whimsical, and nowise in accordance with sound science. He holds, for instance, that scabies is an epidemic disease, and that dirt rather than an insect is its most common cause, and that it may be cured by good air, cleanliness, exercise in the open air, and good and varied food. The reverse of all which, according to him, is productive of most cutaneous ailments.

III. *On Recent Typhus in Lancashire.* By G. Buchanan, M.D., &c.—In this short paper the author describes the partial outbreak of typhus which occurred during the cotton distress, attributable mainly to the ordinary causes of the disorder—

overcrowding, dirt, and poverty. It was arrested by the judicious measures by which it was encountered—segregation, liberal relief, and attention to sanitary precautions.

IV. *On the Yellow Fever Epidemics of Bermuda.* By W. M. G. Smart, M.D., &c.—This paper is partly historical, partly critical. The author passes in review the several epidemics which have occurred in Bermuda of which there are authentic records. The great and disputed question of the origin of the disease is carefully considered. The conclusion he favours as most in accordance with facts, is that it was in no instance imported, but was generated by the action of several co-operating causes, chiefly comprised in a peculiar atmospheric condition, and an extreme neglect of hygiene in all its details. That there was commonly an "epidemic constitution" conducive to it seems proved by the fact, that the disease rarely occurred in Bermuda without a visitation of it elsewhere, either in the West Indies, or on the continent of America. The unsanitary condition of the localities where it first appeared was notorious—crowding in the hulks and barracks, bad ventilation, bad drainage, and indeed, a combination of noxious influences, such as in a cool climate is considered adequate to the production of typhus. It is worthy of remark and satisfactory to learn, that as these evil influences were diminished, as the sanitary state improved, yellow fever became, if not of less frequent occurrence, less severe and less fatal.

The author has some peculiar views relative to the disease: he regards it as the culmination of morbid effects from local and epidemic causes in action, and as commonly ushered in by other diseases which he considers its forerunners and premonitory of its advent, such as diarrhœa, dysentery, cholera. This view he endeavours to support by statistics; but, as it appears to us, his facts are too limited to support his conclusions, especially as there are well-recorded instances of the invasion of the disease at times when the general health has been exemplary, and its outbreak, in consequence, most unexpected. We bear in mind instances of the kind in the West Indies, especially in Barbadoes, the healthiest of these islands, and yet the one which has suffered most from this dire visitation. He has also peculiar ideas respecting the pathology of yellow fever: he believes that the blood corpuscles are diseased, and are mainly concerned in its phenomena, and that the black vomit is an excretion of these corpuscles in an altered state. We could have wished that he had supported this his opinion by facts. According to our experience—and we have tried the blood in some advanced cases of the disease—the blood corpuscles are not perceptibly altered—i.e., when taken, not from the stomach, but by a puncture direct from a vein. The alteration they undergo in the stomach is, as is generally understood, owing to the action on them of its fluid acid contents; and further, that their eruption into the stomach is not from "exosmosis," as he presumes, but from ruptured or wasted vessels, as proved, we think, by the able researches of the late Dr. Blair, now too much forgotten.

On the treatment of yellow fever the author does not enter; but he is very full on its prophylaxis, on which he makes judicious remarks, happily in accordance with what has been most approved and found most efficacious. Thus, for the exclusion of the disease, strict attention to sanitary measures, those which are best adapted to secure vigorous health; and in case of an outbreak, vacating the locality and removal to some healthy spot.

This paper, we have no doubt, will be held to be a valuable addition to the literature of the formidable disease of which it treats, and will often be referred to. We thank him for it, giving him credit for the words with which he concludes—that in the study of the disease in the documentary evidence before him he has been governed by an earnest desire to seek the truth.

V. *On the Epidemic Diseases of Tasmania.* By E. S. HALL. —From Mr. Hall's well-written paper, Tasmania appears to have a climate peculiarly favourable to the health of man and domestic animals. He assures us that epizoötic maladies as yet are almost unknown there, that ague is quite unknown, and that other fevers, except when imported, as in the instance of typhus, are of rare occurrence. The complaints to which the inhabitants are chiefly subject would appear to differ but little from those most prevalent in Great Britain. Of these, however, tubercular phthisis does not hold the first rank, but the second, as to predominance, which he attributes to the purity of the air and the ample diet of animal food. It is remarkable, however, that whilst diseases of the respiratory organs and rheumatic affections are of less frequency than in England, those of the nervous and circulating system are of greater—a difference he attributes, not, we think, very satisfactorily, to the sudden and great changes of temperature to which the climate is subject. Though variola has as yet been excluded, measles, scarlatina, and whooping-cough have appeared there, and have been productive nearly of their ordinary mortality. He briefly notices what is there called "native pock," which he designates as a spurious kind of chicken-pox, and as contagious. Dysentery, diarrhœa, and common cholera, he states, are of occasional occurrence, chiefly during the summer and periods of drought, when bad stagnant water is used.

The death-rate, especially of the country, is low, proving the salubrity of the Tasmanian climate: it is 12 per 1000; but in

Hobart Town, where sanitary measures are little attended to, it is just double.

VI. *Observations on the Influence of Pandemic Causes in the Production of Fever.* By ROBERT LAWSON, M.D. &c.—As we are informed by the author in an appended note that this paper contains the results of an investigation which has been submitted in detail to the Director-General of the Army Medical Department for publication in the forthcoming volume of the Army Statistical Sanitary and Medical Reports, we think it right to wait for the data, *in extensó*, before passing any opinion on the results, and the more so considering the obscurity of the subject, one almost of a transcendental kind, and so abounding in sources of error.

VII. *On an Outbreak of Typhoïd Fever at Wing, Bucks.* By E. R. HARVEY, M.B., &c. — We have in this paper a marked example of the manner in which an English country village may be decimated by fever originating from local causes, such as bad drainage, impure water, dirty rooms, and overcrowded dwellings. The details are impressive, strikingly showing the fatal effects of the neglect of sanitary measures and the connexion of vice and disease. Of the births in this village 13 per cent. were illegitimate. From such evidence as could be obtained, it would appear that the disease began in one of the worst and most crowded of the cottages, and from thence spread by infection. Of the inhabitants, 1000 in number, 200 were attacked and 18 died.

VIII. *Reports of the Trial of Sarracenia Purpurea, or the Pitcher Plant, in Small Pox.* By J. F. MARSON, F.R.C.S., &c.— This trial was made in consequence of a favourable account of the effects of the plant in variola by Mr. C. Miles, of the Royal Artillery. Mr. Marson's results were entirely negative. He states that he could not perceive it had any influence in modifying in the least the eruption, or in influencing any of the secretions.

IX. *On an Epidemic of Jaundice at Rotherham, in Yorkshire.* By Dr. WILLIAM ORD.—This epidemic occurred during January and February of 1863. Of a population of 18,922 about 200 were attacked, all of whom, with the exception of a pregnant woman, recovered. The attack is described as commonly beginning with flatulence, loss of appetite, nausea or vomiting, accompanied by pain in the epigastrium, and a feeling of fulness and distension. Catarrhal symptoms, with a slight degree of pyrexia, ushered in the severer cases. The greatest sufferers from it were pregnant women, many of whom, whilst labouring under it, aborted. Nothing at all satisfactory was ascertained either respecting its etiology or pathology. Its duration rarely exceeded two or three weeks, when the jaundice gradually disappeared, whether remedies were or were not administered.

X. *Notices of the Epidemics of* 1719-20 *and* 1759 *in Peru; also of the Mexican Hæmorrhagic Disease of* 1736 *and* 1855. By A. SMITH, M.D.—In each of the epidemics described in this paper, there were, besides some symptoms in common, others which were peculiar. In all there was an hæmorrhagic tendency, chiefly from the nose and mouth. In the earliest, that of 1719-20, a burning heat was commonly experienced. Its mortality was great, and it was held to be contagious. The llamas and donkeys employed in carrying the dead to the grave showed symptoms of it in a bleeding from the mouth. The epidemic of 1759 was remarkable for the rapidity of its progress, excluding the idea of contagion. In five or six days it had spread through the entire population. The attack terminated commonly in two or three days, leaving the patients in a very weak state. It was fatal in comparatively few instances. Dogs were reported to have suffered from it. The epidemic of 1736 more than decimated the inhabitants; it was peculiar in the rapidity of its action, often proving fatal in a few hours. That of 1855 is described as a recurrence of the preceding, with somewhat modified symptoms. Neither was considered contagious. Of all of them the etiology was obscure. By many they were attributed, according to usage wherever ignorance and superstition prevail, to some coincident and unusual circumstance: such as an eclipse of the sun, an earthquake, or a luminous appearance in the heavens.

XI. *On Epidemic Pleuropneumonia in the Mediterranean Fleet.* Communicated by Dr. BRYSNON.—This paper is very instructive as showing to demonstration the close connexion of neglect of sanitary measures and the production of disease. One instance may suffice, that of the crew of the *St. Jean Acre*, as the most remarkable. From the outbreak of the epidemic in April, 1860, to the end of the year, the number of attacks reached 285, out of an average complement of 815 men. The cases were grouped as follows by the author: bronchitis, 7; pleuritis, 19; pneumonia, 13; hæmoptysis, 7; phthisis, 102; pleurodynia, 20; and cachexia pulmonis, 117.

The author attributed this extraordinary sickness to malarious emanations from the decay of woody fibre and sawdust left in the channels by the shipbuilders, in conjunction with an excessively crowded state of the deck, the lower deck, the sleeping place of the crew. There, he states:

"The hammock-hooks were placed ordinarily only fourteen inches apart, less than the average breadth of the men's shoulders; consequently, when in harbour, when no watch was required at night, and all hands turned in, they formed a compact mass close beneath the beams, the only air available for respiration being above them, that beneath the hammocks being almost entirely shut out from the space above; [the upper was found from 8° to 12° higher than the lower.] All the ports, as well as the small round scuttles, were kept closed at night."

We need not comment on this. It may well remind one of the Black Hole of Calcutta and its horrors; and the more so as to the shameful crowding of the men, when we are informed that there was ample unoccupied space in the upper deck, which in some ships was utilized with the best effects. The number of men invalided from this ship alone in the course of the year was 146.

It is remarkable, that though the crew was well fed with vegetables and fresh meat, yet a scorbutic diathesis was not prevented. It is remarkable, too, that on the transfer of the worst cases to the naval hospital at Malta, a number of the inmates ill of other ailments became affected with similar symptoms suggestive of infection.

XII. *Notes on the Epidemic and other Diseases of the Natives of India.* By Gavin Milroy, M.D.—The notes constituting this interesting paper, designated by the author as brief memoranda, have, he informs us, been drawn up from the replies made by the medical officers to the queries of the Royal Commissioners, on the sanitary state of the Indian army. The summary and practical information we obtain from them is, that the sanitary condition of the native population is generally bad, affording scope for and needing great improvement; that the diseases most prevalent and fatal are fevers, dysentery, diarrhœa, and cholera; that cutaneous diseases and venereal diseases are very common; that scarlatina is unknown there, at least in an un mistakeable form; whilst variola, propagated by inoculation, and not checked by vaccination, except very partially, is both prevalent and very fatal.

The author notices a remittent congestive fever, which has proved very destructive in Lower Bengal, resembling the plague of the Levant; and he notices, also, the occurrence of true typhoïd, or enteric disease, among the native population. Goitre, elephas, and elephantiasis, and also dracunculus, he states, are of limited occurrence: the first among the Hill people bordering on the Himalayas, where it is exceedingly prevalent; the second and third, mostly on the coasts of Malabar and Coromandel; and the last wherever stagnant water is most used.

XIII. *On an anomalous Form of Eruptive Disorder, which it is proposed to designate Rubeola Notha.* By Dr. BABINGTON.—In the spring of 1864 so many cases of the cutaneous affection named above occurred in London as to deserve almost the appellation of an epidemic. They were of a mild kind, hardly needing any treatment. Appended are notices of somewhat similar affections, which were observed in Malta in 1861, and described by Dr. Mackay and Mr. R. Pickthorn, R.N. The latter gentleman considers them as febriculæ, showing irregular and badly-defined eruptions on the skin, and not as specific exanthems.

XIV. *Report on the Epidemics in Great Britain in* 1863. By J. N. RADCLIFFE.—From this Report it would appear that the health of the people during this period was below the average. The explanation of the fact by the Registrar-General is ingenious, and may be true. It is as follows:

"During 1863 there has not been nearly the usual amount of change, variation, or daily and monthly range of temperature. In fact, there has not been a sufficiency of variation of temperature to strengthen the animal constitution against the tendency to debility caused by the unusual mildness and dampness of the atmosphere. Sudden changes of temperature, as all are aware, are hurtful; but experience of this and former seasons fully demonstrates that a certain amount of variation of temperature is essential to health. To the circumstance of two years having followed each other when such atmospheric vicissitudes have been smaller than usual, while, at the same time, there has been a greater fall of rain and more atmospheric humidity, may in great part be attributed the high mortality which has prevailed during every month of the year 1863."

Our limits do not allow us to make the merest abstract of the Report, which on the whole is not favourable either to the measures taken to improve the sanitary condition of the country or to check the spread of infectious diseases. Salisbury is the only town of which mention is made in which the death-rate has been diminished by better drainage, &c. Small-pox was more prevalent, even, and fatal, than usual. Taking London as an example, the deaths there from the chief epidemic diseases were as follow: Continued fevers, 785; small-pox, 2012, the largest amount since 1838; scarlatina, 5075; measles, 1698; diphtheria, 724; whooping-cough, 2229; diarrhœa, 2448; croup, 927.

XV. *Remarks on the Enthetic Diseases affecting the Health of the Troops serving in the United Kingdom for the Years* 1837-46, 1859, 1860, and 1861. By Dr. FRANCIS BOWEN.

XVI. *On the Prevention of Syphilis in the Navy.* By Dr. DICKSON, R.N.—These two papers are admirably adapted to show the evil effects of the diseases of which they treat in the army and navy, and the need there is of legislative interference to check the evil which has well been called the crying evil of the day. From both of them, supported by documentary evidence, it would appear that venereal complaints are the cause of greater inefficiency in the army and navy than any other class of ailments in ordinary times of peace, and at stations where there is no state-provision of a preventive kind.

We need hardly point out to our readers that a temporary disability for duty, and the loss of service for a time, are not the worst effect of these diseases, it being now a well-established fact that they tend to impart a taint conducive to cachexia and tuberculosis.

Appeals time after time have been made to the Government to interfere, and to attempt, especially at home, to restrain the evil by means of a system of medical police, after the example of France and Belgium and other continental countries. All medical men of any experience must be aware of the propriety—indeed, as regards the good of each service, the necessity—of such interference; and we think it must be conceded that those who have the opportunity of bringing the subject to the notice of the authorities, could hardly aid the cause better than by recommending to them the perusal of these papers, the documentary evidence in which is so clear, and the examples in illustration so strong.

XVII. *On the Cause, Malignancy, and Persistence of Yellow Fever aboard Ship.* By A. N. Bell.—We hardly consider this paper as needing a notice, there is so little in it that is new. It is an old opinion, and it is the author's, that the disease owes its origin to the decomposition of organic matters, and is capable of being propagated by what has been called a fomites, *that*, whatever it is, acting with more or less intensity, according to the degree of temperature and the condition of the atmosphere, whether favourable or otherwise. Another part of his doctrine is, that the disease cannot be propagated from person to person—i.e., is not contagious, or transmitted from the person to any substance.

XVIII. *Results of Re-vaccination in the British and in some of the Continental Armies, with Remarks on all Cases of Small-pox reported as having occurred among the Troops from* 1859 *to* 1861 *at the Home and Foreign Stations of the Army.* By Francis Bowen, M.D.—In this paper very satisfactory proof is given from official returns of the benefit of re-vaccination as a security from small-pox. Those who have any doubts can hardly fail of having them dispelled by a perusal of the documentary evidence which the author has brought together.

Besides these papers, the volume contains an animated address by Dr. Richardson 'On the present Position and Prospects of Epidemiological Science,' delivered in 1863, and an excellent one by Dr. Babington in 1864 on his vacating the chair, after an uninterrupted presidency from the founding of the Society in 1850, with some appended matter—viz., 'A Memoir of the late Dr. M'William,' by Dr. Babington; a 'Report of the Small-pox and Vaccination Committee;' and a 'Report on the Question submitted by Dr. Farr to the Council concerning the Classification of Epidemic Diseases,' followed by replies.

Review X.

Reports of the Inspectors of Factories to Her Majesty's Principal Secretary of State for the Home Department, for the Half Year, ending 31*st October*, 1864.—*London*, 1865. pp. 144.

The reports of the Inspectors of Factories for the latter six months of 1864, present many points of interest to the medical profession, particularly with regard to the sanitary condition of the operatives engaged in the various branches of manufacture now for the first time brought under inspection and control by the provisions of the Factory Extension Act of 1864.

There are two Inspectors of Factories for England and Ireland, aided by numerous sub-inspectors, dispersed throughout the country in those districts where manufacturing operations are principally carried on. The actual work of inspection necessarily devolves mainly upon the sub-inspectors, who report their proceedings to the inspectors and receive instructions from them; but when opportunity serves, or special causes arise, the inspectors themselves undertake a personal investigation. The reports now before us show how largely this has been done with regard to the manufactures recently brought within the operations of the Factory Extension Act.

The primary objects of this Act are the protection of the working classes, particularly of women and children engaged in manufacture, from over-work, and from the imposition of work at too early an age; the securing to them of sanitary conditions in their factories, and of safeguards essential to their security where machinery is employed, and, in general, the promotion of their moral and educational position; consequently, the ends contemplated by the Act very largely fall within the class of duties usually allotted to medical men. The more important objects, indeed, contemplated by the Act are essentially medical matters, such, namely, as the protection of children and females from overwork, or from work imposed at too early an age, or upon human frames physically incapacitated for labour; the examination of the conditions of a manufacture as affecting the health and the means of obviating its injurious influences; the determination of the sanitary conditions of work-rooms and kindred matters. The reader of Mr. Inspector Baker's Report will fully apprehend the truth of this statement, and be convinced of the advantages of possessing in him a medical inspector. Without medical knowledge, indeed, the instructive and valuable report presented by that gentleman would never have been written; and, in general, without medical knowledge a correct and satisfactory recognition and appreciation of the influence of the various processes of manufacture upon the health of the operatives cannot be arrived at, nor are profitable suggestions in behalf of the physical well-being of those employed likely to be advanced.

The Government has, indeed, recognised to a certain extent the value of medical knowledge in carrying out some of the objects of the Act. It calls in the aid of medical men as certifying surgeons, to give certificates of age and of physical competency for the labour to be undertaken, of all children and young persons under sixteen years of age; but the certificate once given, the surgeon's duties cease. The child, or young person he has certified for work, may become disqualified for the imposed tasks, and yet continue of its own accord, or be kept by the greed of others at its work, to the detriment of its future health and happiness. The certifying surgeon has no powers of inspection, no access to the workshops, and the only chance of rescue for the individual thus improperly employed is to be found in his attracting the notice of the sub-inspector (who is almost always a non-medical man), during an official visit made with other objects in view. Now we are far from wishing to see the certifying medical man placed at all in the unenviable position of an inspector of the manufactories in his town and neighbourhood; yet opportunities might be given of re-examinations by him from time to time as to the physical competency of the operatives under certificate for the labour required of them; or the evils above indicated might be, in some measure, obviated if the sub-inspectors were medical men capable of forming a correct opinion regarding the bodily health and strength of the work-people falling under their observation. But, at the present time, a medical sub-inspector is a rarity in the corps, which is recruited pretty extensively from gentlemen ambitious of Government employ as supplementary to private resources, who have influential political patrons to recommend them to the Home Secretary, but of whose fitness by character or education no guarantee need be presented. A Lord Dundreary might receive such an appointment, could he arrive at a conclusion that it would comport with his dignity, and not interfere with his constitutional objections to being bored with work and low people.

There is every prospect that the Factory Act will, with necessary modifications, shortly be extended to a vast number of mechanical and chemical processes requiring child labour and ofttimes prejudicial to health, and that, in consequence, the Factory Inspectors' office will assume a most important position in the internal administration of the country. The principle of Government supervision of manufacturing processes in the interests of those engaged in them having already been so widely accepted as correct and beneficial, its extension to those that remain may sooner or later be anticipated. The commercial and manufacturing activity and rivalry of the present day have created such a demand for labour, and have consequently so enhanced the wages of operatives, that whilst masters, on the one side, find it to their interests to push the manufacturing powers of their works to the utmost, their men, on the other, are induced by the increased gain offered, to overtax their own energies and to enlist those of their wives and children in undue exertion. The inducements to overwork being found in all trades, the State intervention to prevent it should be legitimately extended to all those whose sex and age demand protection.

With a field of administration so wide and yet so little proved before it, the Government has need to frame some general principles of action, and to adopt an organization calculated to fulfil the purposes in view with regard to the varied occupations to be brought within its scope. No such general principles and no such organization have as yet been propounded. With reference to the textile, the pottery and some minor manufactures, the provisions of the "Factory Acts" are put into force; but when the sanitary state and physical well-being of bakers is the subject of legislation, another class of regulations is enforced under a different organization. So again, when the wrongs of dressmakers are to be remedied, other plans and other agents are thought of, and the impending extension of the State control over a multitude of mechanical trades is written and spoken of as indicating the need of other and novel arrangements.

The necessity for a general principle of action and of a harmonious plan will make itself felt when this State supervision is extended, and the aid of the medical profession is further called into requisition. For it is evident on reflection that the present system of certifying surgeons and non-medical inspectors must break down. Medical qualifications will be found more largely required than heretofore in carrying out the contemplated legislation, and at the same time the existing plan of certifying surgeons will be found defective. It will cease to be practicable for most medical men in general practice to undertake as heretofore the duties of certifying surgeons to the various works within their districts, except in agri cultural localities; and withal, the scale of payment will be unremunerative when such works are widely scattered and employ but little juvenile labour, as in the case of brick-fields. A subdivision of districts might render the prevailing system possible, but at the same time it would make the appointments of small value, and militate against the efficiency of legislation by creating difficulties in the way of central supervision and management.

But to return from this digression to the contents of the Report before us. Mr. Baker enumerates the following works as brought under the Factory Extension Act:—

"1. Earthenware works, including china, earthenware, stoneware, crucibles, retorts, casting-pots, terracotta, chimney pots, tobacco pipes, and ornamental, ridge, plain, and encaustic tiles, wares for medicinal purposes, toy wares, and closet wares. Drain-pipes and sewage pipes were also included, but have been withdrawn by your (Home Secretary) directions. 2. Lucifer match-making, including vestas. 3. Percussion caps, including railway signals. 4. Cartridge making. 5. Paper-staining. 6. Fustian cutting. The following works have also been brought under the Bleaching and Dyeing Works Extension Acts, namely: Places (warehouses) in which the processes of hanking, lapping, making up and packing, are carried on, excepting where all the persons employed are males over fourteen years of age."

The condition of the Potteries and the peculiar features of the potter's art viewed in reference to its hygienic relations constitute the principal subjects of Mr. Baker's Report. This is explicable enough, since the pottery manufacture is the most important in the list of new trades brought under the regulations of the Factory Act, and the great seat of it in North Staffordshire—the Potteries, *par excellence*—falls within his inspection-district. On the other hand, Mr. Redgrave, the other inspector, dismisses the subject in four pages, inasmuch as he remarks,

"There is no one place in my district in which the earthenware manufacture is the special industry of the locality. . . . . In Lambeth there are establishments for the manufacture of stoneware; at Leeds, Castleford, Swinton, Rotherham, Newcastle, Sunderland, for the ware ordinarily in use; in Glasgow, for fine and coarse earthenware; near Edinburgh for coarse earthenware; but as a general rule the manufacture follows the custom of the 'Staffordshire Potteries.'"

The Worcester china works, we presume, are likewise in Mr. Baker's district, but we do not find them alluded to.

It will, we apprehend, astonish many of our readers to learn that there is a population in the Staffordshire Potteries of considerably over 100,000; for, according to our observation, this earthenware and china-producing district attracts few visitors, and most Englishmen live in profound ignorance of the nature of the potter's art, and of the character and condition of its operatives. The perusal, however, of Mr. Baker's admirable Report will serve greatly to dispel this ignorance; whilst a due appreciation of the beauties and importance of the art may be obtained from the copious biographies of Wedgwood, recently published by Miss Meteyard and Mr. Jowett.

Mr. Baker laboured hard to make himself personally acquainted with the state of the manufacture newly placed under his inspection, and with that of the operatives engaged in it.

> "From the 16th of August to the 15th of December (he writes), with the exception of two or three days now and then, I was resident in the Potteries, visiting most of the 'Banks' (i.e., the potworks), endeavouring to understand the points on which the Act was likely to be brought to bear, making myself familiar with every standard of excellence in the construction of premises, stoves, yards, offices, ventilation, and cleanliness, the social or improvident habits of the people, the amount of school accommodation to be obtained, and the readiness with which it might be adopted if needful."

The number of individuals actually engaged in the pottery manufacture he calculated at 27,432, including "not fewer than 593 little children of five years old, of whom 159 were females; nor less than 4605 other children of between five years old and ten, making altogether under ten years old an aggregate number of 5918 persons, of whom 2917 were females." Mr. Baker proceeds very justly to remark:

> "In looking at the form and helplessness of a child five years old, one's own child, for example, it is scarcely to be believed that such a state of things could ever have existed, and especially when the average wages of potters (as afterwards reckoned on all ages 15*s.* 3*d.* per week each) is taken into account. But these figures are not of my collecting . . . . but they fully bear out the statements made by the master potters in their memorial to Her Majesty's Government:—'That children are employed in the Potteries at a very early age, and in a way to interfere injuriously with their education, and that this state of things is the cause of various moral and physical evils to the youthful population of this district.'"

But if employment in the pottery manufacture at so early an age, considered without reference to its character, "is scarcely to be believed," it becomes a harder trial to believe the little ones should, in a Christian community, have been put to work of the sort we read of in this Report. It seems that the usual time for apprenticing children to the several branches of the trade is the fourteenth year, and that prior to this the principal employment for boys is in "mould running" and "jigger turning." The jigger is the horizontal wheel upon which the workman forms his ware; this he does by pressing a thin layer of clay upon a solid plaster of Paris mould, which is made to rapidly rotate on the jigger by the boy turning a handle moving horizontally and acting on the jigger by a strap. The force required to turn the jigger is to be measured by the weight of the solid wooden wheel, which is inconsiderable, and by that of the plaster mould and clay, but most of all by the pressure of the workman upon the mould and the velocity at the same time required. But this does not represent the whole of the jiggerer's work; he has, besides, to act as "mould runner," that is, to take the mould with the article in clay moulded on it from the "jigger," and to run with it into the neighbouring hot closet or stove, sometimes heated to 140° Fahr. This short but exhausting run accomplished, he resumes his place at the jigger-handle. Relatively to the age of those employed, this labour may correctly be designated excessive, particularly as it was customarily enforced, not only for the usual twelve working hours, but also frequently to late hours in the evening, and occasionally far into the night, when the hard task-master—i.e., the workman, sought to make up by overtime the loss of earnings entailed by his indolence and intemperance over two or more days of the week.

It is clear enough that a remedy was demanded for these poor little white slaves; and the Factory Act has accordingly intervened to limit the work of children under twelve years old to the half-day, the other half being given to school instruction. The hours of labour at the factories for women and young persons are now also curtailed and fixed by law.

Young girls were employed in these same operations, and in some others equally laborious, but in very much smaller numbers than boys.

The sort of work imposed upon children referred to need be witnessed in order rightly to appreciate its unfitness for them, except for short hours. Indeed, it is a description of work altogether unsuited to children under ten years of age. The Factory Act allows them, however, to be employed in it on the completion of the eighth year, because that age was previously fixed upon as the minimum for those engaged in textile manufactures. This appears certainly rather dictated by red-tapism than regulated by those conditions alone—viz., those dependent on the nature of the work, which should have suggested it. For there is a vast difference in the amount of physical labour required in tending a loom and in that involved by the heavy labour of the boy-workers in clay; and we are persuaded that to the immature frame under ten the latter sort of labour is far too great, even when continued for only half a day.

Some excuse may, indeed, be found for allowing such early imposed toil in the necessity probably felt by the Government of not interfering too seriously with the resources in labour available in the Potteries. The sudden enactment of regulations to prevent children under twelve working more than half time, diminished proportionately the supply of labour, a result that would have been more serious still had none been allowed to work under ten years old, particularly in a district like that of the North Staffordshire potteries, where a growing rival manufacture, that of iron, creates a large demand for juvenile operatives. However, as the rule is well nigh universal, that supply meets demand, it is to be expected that the required supply of labour will find its way into the large market opened for it in the Potteries. And when this is accomplished it will become the duty of the State to advance the minimum of age at which children shall be employed in the laborious operations of the "pot-banks."

The reports to be found in the account rendered by the Children's Employment Commissioners, in 1864, of the physical condition of the inhabitants of the Potteries, read, in all their sad details, like the natural consequences of the circumstances of labour prevalent in that population. Premature imposition of labour and overwork of children; prolonged toil of women; excessive work on the part of men, making up for lost time, and too often weakened by dissipation, and the work carried on mostly in hot, ill-ventilated, dirty shops, amid an atmosphere of dust, are conditions that afford sufficient explanation of stunted growth, wide prevailing scrofula, anæmic frames, potter's asthma or consumption, and premature death.

Besides hot, draughty, ill-ventilated, and too often ill-lighted shops and an atmosphere laden with the mineral dust of the dried clay, proceeding from begrimed clothes, dirty floors, and dirty workbenches, the potter is exposed to a yet more tangible cause of sickness in the composition of the glaze used for the ware, for lead enters into it in considerable proportions and exerts its poisonous effects upon those concerned in the process called "dipping," and in the packing, technically "placing," the newly dipped ware in the boxes of coarse clay, the saggars, preliminary to its being fired.

The Factory Act contains one clause intended to obviate, in some degree, the consequences of the singular disregard of the operatives working with lead to those precautions necessary to preserve their health. By this clause, children and women are interdicted taking their meals in the "dipping houses," and also remaining there "during any time allowed for meals." But as Mr. Baker points out, this interdiction falls far short of what is required; for in this matter the restrictions of the Act must be extended also to the men employed, and precautionary measures and disciplinary regulations enforced on all as to dress, ablutions, &c. In the process of dipping, the hands are constantly wetted with the poisonous glaze, and the face and clothes bespattered by it; yet the last are worn mostly day by day unwashed, and the ablutions of the body usually sadly neglected. The Report indicates with what a pretence at washing the work-people in general are satisfied; and yet these workers in lead are, of all people in the world, just those who need thorough washing with soap and water, and, in addition, a warm-bath at frequent intervals. Let us hope the day may arrive ere long, when operatives shall put a higher value upon their lives and health, and take heed to those sanitary measures calculated to preserve them; and when masters shall find it to their interest to aid them in so doing.

It is well to remember that it is only during the last three or four years that "the Potteries" have emerged from their midland obscurity, and been brought face to face with the advanced civilization and progress of most other regions of England, where commerce and manufactures have their seat; and it is therefore not surprising that the prospect gained of them is rude, coarse, and neglected. Indeed, it will take some years more to instil in them the enterprise, and to give them the social and civic status, to which their extent as a manufacturing centre would entitle them.

Although disagreeable revelations have been made concerning the social and educational condition of their population, the physical deterioration and brief lives of the working potters, and the frightful state of mortality of their infantile population, yet it may be esteemed a happy day for the potteries when a State investigation respecting the condition of their workpeople was instituted. It was well for their inhabitants to learn how they appeared to their fellow-countrymen; to have their weakness and their defects pointed out, and to receive suggestions of remedies. It was high time for the law to interpose in behalf of the overwrought children, and to insist on better ventilated and better kept workshops. The enactments enforced must somewhat interfere, and disturb the system of operations of past years; they will, for a time at least, disarrange the previous relations between the supply and the demand for labour, and bear hard upon the small manufacturers; but in the long run they can be productive of nothing but good. Even already, as the Report intimates, their operation for good is shown in the increased impulse to the introduction of machinery for many of the simple processes of the potter's art, which, as heretofore carried on, astonished every visitor acquainted with the applicability of machinery to far finer and more complex processes, by the primitive and clumsy simplicity of its appliances, and the immense loss of time and power involved in them.

We had intended to have noticed other operations in the manufacture of pottery in relation to their effects on the health of the operatives, and also to have discussed the contents of Mr. Baker's excellent observations upon the mortality of potters, and its causes; but the space already occupied by us forbids. We must therefore content ourselves with calling attention to the very full analytical table of the deaths, and of their causes, among potters in the year 1864; and with the remark, that the results of Mr. Baker's statistical inquiries are entirely confirmatory of those previously arrived at by Dr. Arlidge, and which are given in summary in the original communication contributed by him to this Review (No. LXVII., July 1864, p. 214), and more fully in his published pamphlet "On the Mortality of Stoke-upon-Trent, with particular reference to that of Children and Potters."

PART SECOND.

Bibliographical Record.

ART. I.—*Lectures on the Elements of Comparative Anatomy.* By THOMAS HENRY HUXLEY, F.R.S., Professor of Natural History Royal School of Mines, &c.—*London*, 1864. pp. 303.

THIS volume should be very acceptable to the students of comparative anatomy—an increasing class at present, and likely to be so even more and more in future, as the marvellous knowledge which it discloses is more duly appreciated, and the conviction is felt that an acquaintance with the science is indispensable, as it surely is, for attainments of sound views of life, and the infinitely varied structure to which living matter is subject.

Professor Huxley opens his lectures very appropriately with some critical remarks on classification, stating the grounds for his preference of that which he has adopted—viz., a morphological arrangement, founded on purely structural considerations, in which animals have to be viewed, "not as physiological apparatuses merely; not as related to other forms of life and to climatal conditions; not as successive tenants of the earth, but as fabrics, each of which is built on a certain plan," and this not of isolation and independence, but one of gradation and interconnexion; every animal, as he says, having "something in common with all its fellows; much with many of them; more with a few, and usually so much with several, that it differs but little from them."

The objects and uses of this classification he describes as manifold, the most important of which is, "that every group is such in virtue of certain structural characters which are not only common to the members of that group, but distinguish it from all others; and the statements of these constitute the definition of the group."

Following this scheme, which seems to be as much as possible free from all hypothetical views, he begins his classification in natural order from the lowest, the simplest forms of living matter, life without perceptible organization, as exemplified in the Gregarinida, &c., and ascends, but not without breaks, in the orderly scale to the highest, those distinguished for complexity of organization, of which man forms the summit.

According to the present state of our knowledge, he proposes an eight-fold division of the whole animal kingdom, grouped as follows:

Vertebrata.

Mollusca.	Annulosa.
Molluscoïda.	Annuloïda.
Cœlenterata.	Infusoria.

Protozoa.

Speaking of the departure of this arrangement from that of Cuvier's system of classification, he observes:

"The departure is very nearly in the ratio of the progress of knowledge since Cuvier's time. The limits of the highest group, and of the more highly-organized classes of the lower divisions, with which he was so well acquainted, remain as he left them; while the lower groups, of which he knew least, and which he throws into one great heterogeneous assemblage—the *Radiata*—have been altogether remodelled and re-arranged. Milne-Edwards demonstrated the necessity of removing the Polyzoa from the radiate mob, and associating them with the lower Mollusks. Frey and Leuckart demonstrated the subregnal distinctness of the Cœlenterata. Von Siebold and his school separated the *Protozoa*, and others have completed the work of disintegration by erecting the *Scolecida* into a primary division of *Vermes*, and making the *Echinodermata* into another."

The volume comprises in substance the first course of lectures which its author officially delivered at the Royal College of Surgeons in 1863, in his capacity of Professor of Comparative Anatomy and Physiology. Of the fourteen lectures, the number which constituted the course, the first six treat chiefly of classification, and on the divisions and subdivisions of the entire animal kingdom of the principles laid down, with a description of the characteristic features of each great group, so far as they can be well defined. The remaining eight are devoted to the vertebrate skull of the several orders of the great class of vertebrata, concluding with its theory.

This last portion, occupying twenty-six pages, is an excellent example of searching and just criticism, and of inductive reasoning. Professor Huxley vindicates the claims of Goëthe as the originator of the hypothesis of the vertebral structure of the cranium, and acknowledges the merit due to Oken for having independently taken the same view of the growth of the skull, at the same time rejecting the theory of the latter in its fanciful extension—as not being supported by facts—the facts which embryology has disclosed, and for which we are indebted to the researches of Rathke, Vogt, Agassiz, Von Bäur, and others. His general conclusions on this interesting and much-disputed subject are stated in fifteen distinct propositions. We quote the first five, regretting that our limited space does not allow of the transferring of the remainder to our pages, all of which have the recommendation of being expressions of facts:

"1. All crania result from the modification of the anterior parts of that 'primitive groove' of the embryo, the posterior parts of which give rise to the vertebral column; and, at the very first, there is no discernible difference between that part of the groove which will give rise to the vertebral column, and that from which the skull will be produced.

"2. The first changes which take place, in both the cranial and the spinal regions of the primitive groove, are also precisely similar, the dorsal laminæ growing up and uniting together in the middle line, so as to inclose a cavity which is, on the one hand, the primordial brain-case, and, on the other, the primordial spinal canal. So far, a unity of organization may be predicated of both the brain-case and the spinal canal; but the brain-case is not yet a skull, nor the spinal canal a vertebral column.

"3. Beyond this point, the cause of development of the cranial region differs absolutely from that of the spinal region. In the latter, the histological differentiation takes place which results in the formation of the proto-vertebræ, while in the skull no such process occurs. Again, the notochord extends throughout the whole length of the spinal column; while, as soon as the skull is distinguishable as such, the notochord ceases to extend beyond the middle of its floor, stopping immediately behind that part which lodges the pituitary fossa.

"4. Furthermore, when chondrification takes place in the spinal column, separate masses of cartilage are developed in each proto-vertebra; but when chondrification commences in the base of the skull, it gives rise to a continuous body of cartilage, which never exhibits any trace of the transverse division or segmentation, but is always divided under the pituitary body into two longitudinally-arranged crura, the '*trabeculæ cranii*.'

"5. Hence it follows that, though the primordial brain-case and the primordial spinal canal are identical in general plan of construction, the two begin to diverge as soon as the one puts on the special characters of a skull, and the other those of a vertebral column; the latter taking one road while the skull takes another. The skull is no more a modified vertebral column than the vertebral column is a modified skull; but the two are essentially separate and distinct modifications of one and the same structure, the primitive groove."

Finally, while he holds that all the facts which he adduces negative the hypothesis that the skull is in any sense a modification of the vertebræ, he admits that, leaving out certain hypothetical considerations, its *ossified* segments are homologous with vertebræ, that Oken's expression of the broad facts of the structure of the completely ossified brain-case is the best that has been given. This discriminating acknowledgment, we may remark, is singularly contrasted with the contradictory opinions on the same subject expressed by Mr. Owen.

We are glad to find that this volume, which we have so briefly and imperfectly noticed, is intended to be the first of a series; those which are to follow, like this, to be complete each in itself, each comprising a single course, the author holding out the prospect of eventually completing "a comprehensive, though condensed systematic work on comparative anatomy."

Judging from this first specimen, such a work cannot fail to be of great value. It is seldom that we find so many qualities fitting a man for such an undertaking as are united in the Hunterian Professor of the present time—a love of truth and its common accompaniments, great accuracy; a logical mind, and a happy power of expression in simple and forcible language; a minute, a great, and a comprehensive knowledge of the facts of his science, acquired by an untiring industry; and, we will add, another quality, alas! not common, that love of justice which insures correctness in historical details.

ART. II.—*On the Ultimate Nerve-Fibres distributed to Muscle and some other Tissues; with Observations upon the Structure and probable Mode of Action of a Nervous Mechanism.* By LIONEL S. BEALE, M.B., F.R.S. *London,* 1865. (Pamphlet.)

THIS *brochure* is the reprint of a communication made to the Royal Society in May, 1865, which, as we are informed, is shortly to meet the public eye in another form, accompanied by all the drawings originally attached to the paper. We shall not take this opportunity of guiding the reader over all the ground traversed by Dr. Beale before arriving at the results which reward his labours; such a proceeding will better suit a more general consideration of his investigations when viewed in conjunction with those of other observers in the same direction. It may, however, be premised that Dr. Beale, after devoting attention to the consideration of movements in cells and the tissues of living beings, and of contractility in general, proceeds to discuss the distribution of nerves, first to involuntary, and subsequently to voluntary muscle, noticing, in passing, the researches of many others, chiefly German physiologists. He then dwells upon the essential structure of a nervous mechanism, and enters upon the arguments in favour of uninterrupted nerve-circuits derived from various sources, concluding with the following digest of the results of his inquiry. He observes:

"The first important point is, that in no tissue have I been able to demonstrate an '*end*' to a nerve. In all cases the nerve cell or nucleus exhibits fibres proceeding from at least *two opposite directions*. The apparent cessation or thinning off of the nerve-fibre in many tissues results from its becoming so thin as to be invisible, unless special methods of investigation are resorted to. It has also been shown that near nervous centres, and near their peripheral distribution, the bundles of nerve-fibres and the individual nerve-fibres divide into very numerous branches. The bundles of coarse or fine fibres given off from a large or small trunk consist of fibres which pursue opposite directions in that trunk, one set passing as it were *from*, the other *towards*, the nervous centre. The nerves distributed to striped muscle of all kinds and to the various forms of unstriped muscle in vertebrata and in invertebrata, are arranged so as to form networks and plexuses, but no indication of termination or ends is to be seen.

"These facts seem to render it probable that the fundamental arrangement of a nervous apparatus is a complete and uninterrupted circuit. This view is supported by the existence of at least two nerve-fibres in all peripheral organs and by facts observed in the branching and division of individual nerve-fibres and of compound nerve-trunks. I have also shown that in nerve-centres it is doubtful if apolar or unipolar cells ever exist. All nerve-cells have at least two fibres proceeding from them in opposite directions, and the multipolar cells in the brain and cord exhibit lines across them which are probable indications of the paths taken by continuous currents which traverse them in many different directions.

"The general inference from this anatomical inquiry is, that a current probably of electricity is constantly passing through all nerve-fibres, and that the adjacent tissues are influenced by the varying intensity of this nerve-current rather than by its complete interruption and re-establishment; so far as I know, no fact has ever been discovered which would justify the conclusion that there exists any arrangement for making and breaking contact in any part of the nervous system. In all cases it is probable that every nervous circuit is complete, and that there is no interruption of the structural continuity of a nerve-fibre at any part of its course."

Art. III.—*Medical Diagnosis, with Special Reference to Practical Medicine.* By J. M. Da Costa, M.D. Illustrated with Engravings on Wood.—*Philadelphia,* 1864. pp. 690.

The author of this treatise is Lecturer on Clinical Medicine and Physician to the Philadelphia Hospital, and may consequently be held to possess the necessary credentials for a writer upon diagnosis. For in no department of medical knowledge, not excepting therapeutics, is long-continued and widely-extended observation of so much value or so necessary to its advance as an art. Diligent and prolonged observation is essential to the advancement of therapeutics, but its results are sadly depreciated in value and sacrificed by contingent conditions far greater in number and in importance than are any such encountered in the study of diagnosis. Hence it is that diagnosis is a much more advanced and advancing department of medicine than therapeutics, which is unfortunately apt to become less clear and definite as years and experience are allotted to the medical practitioner.

Dr. Da Costa's work is especially addressed "to advanced students and young graduates of medicine (as) a book on diagnosis of an essentially practical character, one neither so meagre in detail as to be next to useless when they encounter the manifold and varying features of disease, nor so full as to be unwieldy and lacking in precise and readily applicable knowledge." We do not perceive the force and accuracy of the last clause of this paragraph, for surely a "full" book need not, *quoad* its fulness, be lacking in precise and readily applicable knowledge, any more than a shorter treatise need possess those good qualities; on the other hand, a book, whether of full or of meagre dimensions, if diffuse, will be lacking in them. The treatise under notice, containing, as it does, 690 pages, and occupied as it is with one subject, may fairly be ranked as a "full" book, but we are glad to find it not a diffuse one, but well filled with "precise and readily applicable knowledge."

The morbid states described are arranged not on a pathological basis, but according to their marked symptoms, or a "purely clinical classification." This plan, the author remarks in the preface, has involved more labour, but its advantages recommended it. As an example we quote the arrangement of the subjects of the second chapter on "Diseases of the Brain, Spinal Cord, and their Nerves.—General Considerations. *Deranged Intellection*:—Delirium, Stupor, Coma, Insomnia. *Deranged Sensation*:—Hyperæsthesia, Anæsthesia, Headache, Vertigo, Derangement of Special Senses. *Deranged Motion*:—Paralysis, Tremor, Spasms, Convulsions. *Acute Affections of which Delirium is a prominent Symptom*:—Acute Meningitis, Tubercular Meningitis, Cerebro-Spinal Meningitis, Delirium Tremens, Acute Mania. *Diseases marked by Sudden Loss of Consciousness and of Voluntary Motion*:—Apoplexy, Sun-stroke, Catalepsy. *Diseases marked by Convulsions or Spasms*:—Epilepsy, Chorea, Hysteria, Tetanus. *Diseases characterized by Gradual Impairment of the Mental Faculties with Paralysis*:—

Chronic Softening, Tumour, General Paralysis. *Diseases characterized by Enlargement of the Head :*—Chronic Hydrocephalus, Hypertrophy of the Brain. *Diseases characterized by Paroxysmal Pain :*—Neuralgia in general, Facial Neuralgia, Hemicrania, Sciatica."

A review of this arrangement of brain and nerve disorders leads at once to a recognition of its imperfections. It involves repetition, as in the instance of paralysis considered first simply as deranged motion and subsequently in reference to its causation. Again, the so-termed characteristic symptom or symptoms under which definite disorders are arranged, are neither constant nor positively peculiar to many of those disorders. Thus, delirium is far from always being a prominent symptom in tubercular meningitis, whilst, on the other hand, it may assume that character in acute softening, which is treated of among diseases marked by sudden loss of consciousness and of voluntary motion. Again, the existence of tubercular meningitis is not so unfrequently first declared by the occurrence of convulsions; and, in the case of tumours of the brain, gradual impairment of the mental faculties with paralysis cannot certainly be assigned as a characteristic symptom, though it may make its appearance in the progress of the disease and under certain conditions of situation and the like.

This method of arrangement for diseases adopted by Dr. Da Costa is analogous to, and partakes of the same imperfections as, the Linnæan or artificial system of botany; the characters employed have no necessary and constant relations to the objects characterized, whereby their true nature and affinities may be discovered. In fact, Dr. Da Costa admits the imperfection of his classification, but claims for it a balance in advantages over a pathological one.

A treatise on diagnosis neither demands nor affords matter for analysis and extraction, although the accuracy and completeness of its contents are fully open to criticism. Our perusal of the volume before us has satisfied us of its possessing those qualities in a high degree, and consequently of its value to the medical student and practitioner. Here and there some omission may be signalized, or the information be less precise and full than could be desired; but such blemishes are few, and detract very slightly from the value of a work of such extent and general accuracy.

A few notes of some topics imperfectly discussed are desirable. And first, we should have preferred meeting with a more complete history of the symptoms of reflex paraplegia in this treatise on diagnosis, rather than to be referred to the essay of Dr. Brown-Séquard for a satisfactory notice of them. Again, the account of cerebro-spinal meningitis is chiefly restricted to the epidemic variety of the lesion, and the notice of spinal meningitis and of spinal myelitis is meagre. Hæmorrhage in the spinal cord is only incidentally touched upon, and the remarkable group of symptoms identified by Dr. Brown-Séquard as distinctive of lesions affecting one-half of the cord, or of the medulla oblongata, or involving the pons Varolii, are unnoticed. The "general paralysis" of the insane is evidently a disease with which Dr. Da Costa is not familiar, and consequently his sketch of its characteristics is incomplete. In the matter of laryngeal affections also, as displayed and elucidated by the use of the laryngoscope, the author likewise seems to be at fault. In his notice of cough as a symptom, and also elsewhere in the observations on throat affections, chronic folliculitis of the throat is omitted; and owing to the manner in which renal affections are discussed, the diagnosis of stone in the kidneys is nowhere separately and concisely stated. Moreover, the account of so-called Addison's disease, and the differential history of rheumatism, of gout, and of rheumatic gout, are wanting in precision and completeness. It will suffice to quote one more example of deficient information (in our ungracious task of fault-finding, in a work of so great merit)—viz., the too slight notice of the altered states of the urine in fever, which indeed, in the opinion of some physicians, are of much importance in diagnosis. On the other hand, commendation is demanded for the history in general of the several forms of fevers, and particularly for that of fevers with which most English medical men are likely to be unacquainted from home observation, but with which they may come in contact if located in foreign lands. As an American, Dr. Da Costa has the advantage of discoursing respecting some of these from personal experience in the treatment of them. Such are the remittent, the yellow and malarial congestive fevers met with on the North American continent, with several varieties of which the physicians of the United States have unfortunately had prolonged experience during the late terrible war which devastated their country.

Among the several observations collected during the war, one is mentioned by the author of a peculiar functional disorder of the heart, that is worth quoting:

"It is the curious cardiac malady of which we now see so many examples in soldiers. Its main symptoms are habitual frequency of the action of the heart, constantly-recurring attacks of palpitation, and pain referred to the lower portion of the præcordial region. The palpitations occur chiefly during exercise, but may also take place when the patient is quiet, and in many cases are most frequent, or indeed happen entirely at night, thus interfering with sleep. It is not unusual to hear soldiers that are subject to the disorder complain much of headache and of dizziness, and especially of being thus affected when suffering from palpitation. The pain is generally dull and constant, but is often also described as shooting, and as taking place only in paroxysms. Its chief seat is near the apex, and it is combined very commonly with excessive cutaneous sensibility. Often there is pain nowhere else in the body; but in some instances the cardiac distress is associated with pain in the back, which itself is not unusually connected with the excretion of oxalate of lime by the kidneys.

"The action of the heart is very rapid, and in many instances its rhythm is irregular. The impulse is slightly extended, but not forcible, like that of hypertrophy; it is rather abrupt and jerky. As a rule, to which thus far I have met with but few exceptions, the sounds of the heart are modified as follows:—The first sound is short, sometimes sharp, like that of the second, at other times extremely deficient and hardly recognisable; the distinctness of the second sound is very much heightened. We hear no murmurs, either in the heart or in the neck. The area of percussion dulness does not appear to be augmented. The pulse is almost always easily compressible; it may or may not share the character of the impulse. It is generally very much influenced by position, falling rapidly twenty beats or more when the erect is changed for the recumbent posture. The increased frequency of beat is not connected with increased frequency of respiration, for often with a pulse of one hundred, the respirations scarcely exceed twenty in the minute. The disorder is a very obstinate one to manage, and improvement comes but slowly. Keeping the heart quiet by occasional doses of digitaline, or by atropia, and improving its tone as much as possible by tonics, among which iron is serviceable, has been the treatment which I have thus far found to be the most successful.

"What the cause of the morbid cardiac impressibility is it is very difficult to ascertain. It seems, in many instances, to have followed fatiguing marches; in some it occurred after fevers or diarrhœa. As far as I have been able to observe it was not connected with scurvy, or with the abuse of tobacco. That it is not due to anæmia is at once proved by the general aspect of the men, which is often that of ruddy health."

This extract, besides its intrinsic value as a record of a peculiar group of symptoms, affords some notion of the author's style, which may be stated to be throughout simple, without any straining after effect, and always clear. A larger introduction of tables of diagnosis between allied disorders might be introduced advantageously in a subsequent edition, and it would also be well if Dr. Da Costa would collect more together, and in some way emphasize, the pathognomonic and most important symptoms of the several diseases, so that the eye of the reader, on making a casual reference to the book, might at once light upon them. The wood-cuts inserted here and there in illustration of the printed statements well subserve their purpose as diagrams. They are introduced chiefly in elucidation of the diagnosis of chest and of urinary affections, but they might advantageously be brought into larger use in the chapters on abdominal diseases. The index is very copious and well arranged, so that the reader who might be at a loss to know under which head in the "clinical classification" adopted to look for the symptoms of any particular malady, may readily discover the page where its diagnosis is discussed. We have already expressed our high opinion of Dr. Da Costa's work; it now only remains to add that the printer's part in its production has been well performed.

ART. IV.—*The Ophthalmic Review: A Quarterly Journal of Ophthalmic Surgery and Science.* Edited by J. Z. LAWRENCE and THOMAS WINDSOR.—*London*, 1865.

THE numbers now before us of this journal, those for April, July, and October, well maintain its rising reputation. The introductory article in the first number, entitled "Ophthalmic Surgery at Home and Abroad," is deserving of praise for the ability with which it is written, and for the liberal and philosophical spirit in which the author compares the state of ophthalmic science in England and on the Continent, and more especially the claims for its advancement by means of the ophthalmoscope due to the labours of British and German inquirers. He happily and forcibly points out the influence which this instrument has exercised, and how in the short period, little more than decennial, which has intervened since its discovery, it has entirely changed the character of ophthalmology; in brief, has converted an art to a considerable extent conjectural into a science of wonderful precision—a precision, from the nature of the organ, the eye, likely to have a beneficial effect in advancing medical science generally.

An abridged report of the Heidelberg Ophthalmological Congress, held in September of last year, and a copious retrospect of British and foreign medical journals, form a large portion of each number, and add much to their value. Of the more important contributions we would name Dr. Mackenzie's "Hints on Cataract Glasses," and Professor Von Gräfe's Clinical Lectures, commenced in the October number, "On Amblyopia and Amaurosis."

We must restrict ourselves to one extract of peculiar interest —a case of restored vision, very like the celebrated one of Cheselden's, from Dr. J. H. Knapp's Report of the Eye Infirmary at Heidelberg:

"The case was that of a boy, thirteen years old, *born blind*, from fully-formed cataracts, the eyes being otherwise healthy, which is rare. Both eyes were operated on with complete success by linear extraction. In five days the case was cured. At first, the boy had not the least idea what use to make of his eyes. He could, however, distinguish the *principal colours*, and the *directions* of objects, but of *distance* he had no idea; he grasped at an object at the other end of the room just the same as he did at one quite close to him. He did not know the name of anything he saw; but when he touched it, he did. He soon, however, educated his eyes to name objects without touching them. Every day he learned the names of more and more things, using the sense of touch as the instructor of that of sight—a task in which he was further assisted by his fellow-patients. By the time he left the hospital, he could go alone. Two months afterwards, he had learned his letters, and could even spell tolerably."

ART. V.—*The Natural History of the Human Teeth, explaining their Structure, Use, Formation, Growth, and Diseases.* In Two Parts. By JOHN HUNTER, F.R.S. With Notes by FRANCIS C. WEBB, M.D., F.R.S., &c.; and ROBERT T. HULME, M.R.C.S., F.L.S., &c.—*London,* 1865. pp. 253.

THIS new edition of a treatise of deservedly high reputation is much enhanced in value by the able manner in which it has been annotated. The notes are copious and very informing; indeed, they are so copious, that they exceed in amount the text itself. Those appended to the First Part, relating chiefly to the comparative anatomy of the teeth, have been contributed by Dr. Webb: they display a minute knowledge of the subject, and of the writings of the best authors. Those to the Second Part, which are less ample but not less judicious, on the diseases of the teeth, we owe to the editor, Mr. Hulme.

To the medical student the work thus brought out, in a handsome and well-printed volume, should be welcome: we can strongly recommend it to his attention. It is well adapted, we may remark, to serve both as an introduction to the study of comparative anatomy, and to the study of the more elaborate and standard works on dental surgery. And in either way the references which have been made to the writings of those to whom we are most indebted for the vast advance that has been made in dental science since the time of Hunter, cannot fail to be of much value.

We are reminded in a note of an opinion of Mr. Owen, that man, as he thinks, judging from the structure of his teeth, was designed to be the inhabitant of a garden; yet it is remarkable, that his teeth differ essentially from those of the frugivorous apes, even of the most anthropoïd; and it is also remarkable and derserving the consideration of those anthropologists who hold that unity of species is not characteristic of the human races, that there is no essential and constant difference to be found in the teeth of the most and of the least civilized peoples, the variations of form, such as they are, being of degree, not of kind.

Far advanced as dental science is, a glance at the notes with which this volume is enriched may suffice to show how much further research is needed to clear up the many doubtful points which there are at present relating to the minute structure of the teeth—research which, to be successful, we need hardly remark, must be conducted with no ordinary care, and, considering who have been hitherto the eminent labourers in the field, by men of no ordinary ability.

ART. VI. — *Military, Medical, and Surgical Essays, prepared for the United States Sanitary Commission.* Edited by WILLIAM A. HAMMOND, M.D., Surgeon-General of the United States Army, &c.—*Philadelphia,* 1864.

IN a former number of our Review, that for October, 1863, we gave a favourable notice of these essays on their first appearance. Deserving as they are of all praise, we are glad to see them collected and presented to the public in a handsome volume, with notes, fewer than we could wish, by the editor. Viewed as a whole we consider them as highly creditable to their contributors, and to the advanced state of medical science in America. The work is well deserving the attention of the medical student, especially if in course of preparation for the army or naval medical department.

Of the editor, Dr. Hammond, the late Surgeon-General of the United States Army, of whom we have formed a very high opinion, we regret to learn that he has been dismissed from the service of his country by the sentence of a court-martial held on him at his own urgent request. We have read what he has put forth in his vindication;[1] and strong, indeed, is the impression left on our minds of the iniquity of the proceedings from the first to the last, and of the vileness of character displayed by their instigator, the Secretary of War, Mr. Stanton. The time will come, and we hope soon, when such "*wickedness*" as Dr. Hammond has so manfully, ably, and so convincingly exposed, "*will meet with its due reward.*" The words in italics are his; and the following, which we think he is fully entitled to use, conclude his statement: "Till that hour comes, I shall not bear myself less proudly, by reason of the temporary triumph of my enemies; but, conscious of right, will patiently wait for the full vindication which is sure to come."

[1] A Statement of the Causes which led to the Dismissal of Surgeon-General W. A. Hammond from the Army, with a Review of the Evidence adduced before the Court.

We must express our surprise that the finding of the Court on charges, all but disproved, of a frivolous kind, should have had the approval of the President, the late Mr. Lincoln. We must presume it could only have been given in haste and in ignorance. Republics have never been distinguished for justice or for gratitude for good services, and it is clear that the United States Government is not an exception.

What Dr. Hammond's services were, may in part be understood from that which was said of them by a very competent judge, the Rev. Dr. Bellows, President of the Sanitary Commission; and also what was the animus towards him of the Secretary of War. The passages we are about to quote are from a letter of Dr. Bellows to a member of the Senate, bearing the date of the 26th Feb. 1863:

"The Surgeon-General has brought order out of chaos in his department, and efficiency out of imbecility. The sick and wounded owe a *hundred times* over more to the Government and the medical department than to all the outside influences and benevolence of the country combined, including the sanitary commission! The Surgeon-General is the best friend the sick soldier has in this country, because he wields the benevolence of the United States Government. For God's sake, don't thwart his zeal and wisdom!"

The Secretary of War he writes of as—

"A man of strong personal prejudices, irritable, and often very unjust. His dislike of Dr. Hammond is a most unfortunate thing for the service. If he succeeds in injuring the Surgeon-General with the military committee or with the Senate, he will inflict a greater wound on the sick and wounded in the hospitals and in the army than he can heal over in two years to come."

Sure we are that this greatly injured and most deserving medical officer will have the sympathy of all right-minded men in this country, and especially of his professional brethren, the best able to appreciate his abilities and exertions, strained to the utmost in the office he filled during the height of a war of unparalleled greatness, and with difficulties of an extraordinary kind to encounter in his special department.

ART. VII.—*Contributions to Practical Surgery.* By W. H. VAN BUREN, M.D.—*Philadelphia*, 1865. pp. 208.

THIS volume consists of a series of surgical papers selected from various medical periodicals, and contains a valuable set of cases. These have been accumulated during several years of considerable experience, which the author (a member of the United States Sanitary Commission) had had the opportunity of gathering from his public practice as surgeon to the New York, to the Bellevue, and to other hospitals, as well as from a large private practice. In a notice such as this we can do little more than indicate the general character of the cases described.

The series commences with the details of three cases of *Amputation at the Hip-joint,* by a "modified process." This process appears to be a modification of Liston's operation with antero-posterior flaps, in which the posterior flap is made by cutting from without inward toward the bone, instead of in the opposite direction, the disarticulation of the thigh being left to the last. Dr. Van Buren has adopted it with a view of facilitating the disarticulation of the head of the bone, and the detachment of the great trochanter from its connexion. His method also obviates the necessity of transfixing the thigh so very near Poupart's ligament, the joint not being so extensively exposed in the first incision as in other modes of operation.

Dr. Van Buren then details three cases of *Tracheotomy,* which he performed, in one case successfully, when a foreign body existed in the air-passages; in another case, with relief, when death was pending owing to syphilitic ulceration of the larynx; and in the third (with a fatal result) in a case of croup. In the first of these cases chloroform was used with the greatest advantage; its details are full of very considerable interest.

Then we have the history related of two cases of *Inguinal Aneurysm,* in one of which the author operated successfully, tying the artery after attempts at manual compression of the external iliac artery had failed; whilst in the other (operated upon by Dr. Mott) gangrene produced death. Dr. Van Buren affixes to these cases a table, in which are arranged the results of the Hunterian operation in 24 cases of arterio-venous aneurysm; concluding from their consideration that the operation "not only promises no good result, but that its performance is absolutely unjustifiable." The author then proceeds to detail a case of *Malignant Polypus of the Nose,* attended by frequent dangerous hæmorrhage, in which the common carotid artery was ligatured, and which proved fatal, with the occurrence of cerebral symptoms in connexion (as was proved by post-mortem examination) with softening of the brain; and subsequently gives the particulars of a case of successful removal of a fibrous tumour (weighing 7 lbs.) of the left ovary, by the large abdominal section, from a patient twenty-seven years of age. In this case, the freedom from unpleasant symptoms in the after-treatment was attributed by the operator to the free use of opium, and the employment of chloroform at the time of operation. With regard to the statistics of the operation of ovariotomy in his country he remarks: "I have knowledge of 36 operations performed up to the present time, of which the issue of 14 has been fatal. Of these, 5 have been done in this city; 3 of them, which have never been recorded, terminated fatally from peritonitis; the remainder were perfectly successful—viz., that of Mr. D. L. Rodgers, and the case now recorded.' '

The next chapter which Dr. Van Buren gives us contains an interesting account of the use of *Forcible Dilatation of the Sphincter Ani*, in cases of fissure, or irritable ulcer of, or spasms of neuralgia of the rectum, also resorted to in certain cases after the operation for the cure of hæmorrhoidal growths by ligature or écraseur. This mode of treatment was originally proposed by Recamier, of Paris, but though it has been adopted at various times and in various degrees by others, is not described, as it appears, in our works on diseases of the rectum. Several instances showing its usefulness are recorded, the resulting good being attributed to the temporary paralysis thereby induced of the sphincter, owing to which pressure on the ulcer, or fissure, or other affection, is prevented, and their healing facilitated. The frequent laceration of the mucous membrane, or even that of the muscle itself incident to the operation (which is performed by means of the two thumbs introduced into the anus and forcibly drawn asunder) is thought not to be in any way injurious or objectionable. Out of upwards of twenty cases, in which this mode of procedure has been used, which have come under the author's notice, in none have indications of incontinence of urine shown themselves.

Following the above, we have related cases of *Vesical Calculus* treated by lithotomy and lithotrity, in one of which the nucleus was a piece of a catheter, whilst in another it proved to be a piece of slate-pencil, and in another a piece of straw; and following these cases, one of *Popliteal Aneurysm* successfully treated by compression, and then three cases of *Dislocation of the Femur*, showing the good effects of anæsthetics in the treatment of such cases. Dr. Van Buren then relates the cure of six cases of *Strangulated Hernia of the Tunica Vaginalis* (congenital hernia) occurring in the adult. In connexion with these, he insists on the necessity of recognising this form of hernia, inasmuch as owing to its peculiar mechanism the stricture by which it is strangulated is perhaps always situated at the neck of the sac, therefore any operation which fails to include the opening of the sac and the division of the neck will be improper. In addition to the suddenness of the occurrence of rupture descending into the scrotum, the author adds, as significant of this form of hernia, the liability to excessive dilatation of the scrotum and inguinal canal, with simultaneous strangulation at the neck of the sac, accompanied with symptoms of sudden shock to the system, severe local pain, and inability to assume the upright position. Dr. Van Buren suspects that the process of peritoneum, connecting the general cavity with the tunica vaginalis, remains *un*obliterated more frequently than is supposed, and that the effort at obliteration is more marked at the internal ring, and just above the testis, which seems to explain the frequency of stricture at the internal ring as well as the constriction observed below the middle of the sac, as also the hour-glass shape in many cases of hydrocele.

We now pass on to a case of *Ligature of the Subclavian* artery above the clavicle in a case of rapidly-increasing false aneurysm, resulting from a knife-wound, which occurred three weeks previously, and to this are appended statistical data resulting from the consideration of 101 cases of ligature of this vessel, which the author has collected ; the conclusions having reference especially to the comparative mortality of the operations on the different parts of the bloodvessel, the period of separation of the ligature, and the causes of death.

The book closes with an account of a *Complete Salivary Fistula of Steno's Duct* following a gunshot wound, and cured by an ingenious operation which Dr. Van Buren had himself learned. After the operation much inflammation of the parotid gland and other parts occurred, that of the gland resulting from an experiment made with a view of testing the assertion of Bernard, of Paris, that the injection of any fatty substance into the duct of the parotid will arrest its secretion and produce atrophy of the gland. In this case the fistula was injected with melted lard previous to the operation, "but the result was negative," as at the end of nine months the gland was "secreting freely as ever" by means of the new duct, whose orifice was seen discharging saliva into the mouth.

So much for a rough analysis of the cases embodied in Dr. Van Buren's contributions. We can only say in addition that the surgeon will find them interspersed with interesting and practical remarks, which cannot fail well to reward the attention which he may bestow upon them.

ART. VIII.—*Report on the Cheap Wines of France, Italy, Austria, Greece, and Hungary.* By R. DRUITT, M.D.—*London*, 1865. pp. 179.

THIS volume, containing as it does information of great interest, and written in a very pleasing manner, is yet, we think, on too limited a scale, considering the importance of the subject. The author has dealt, as it appears to us, perhaps too much on the price, and not enough on the dietetic value, of the various wines, as dependent on their constituent qualities.

We agree entirely with Dr. Druitt in his condemnation of the perilous stuff which, whether it be sold under the name of Sherry or Port, Burgundy, Gladstone's Claret, or Champagne, is equally injurious to health, nauseous to the taste, and, in reality, dear to the purchaser. Nevertheless, we cannot condemn sherry and port in the same general manner that our author does; and we believe, from personal experience of these things, that sound, wholesome sherry, and good healthy port can be bought at such a price, when its character and vinosity is considered, as to make it as cheap as any of the wines which he recommends. We have seen port made on the Douro as well as sherry at Xeres and Port St. Mary, and the vineyards of Sicily and the slopes of Capri have all yielded their products to our eyes and palate on the spot, but we have also tasted and repudiated the manufactured article in the busy city on the Elbe.

We say without hesitation that port wine can be purchased at Oporto into which not a drop of cheropiga has been put, and not more than eight gallons of spirit to a pipe, the duty paid on it, and deposited in the cellar at 30*s*. per dozen. This wine will require to be kept four years in bottle, and therefore you must add five per cent. for interest on the money set fast; this, at compound interest, will come to about 8*s*., or, to make even money, it will cost 40*s*. if you purchase it of the wine-merchant as having been four years in bottle. This will make the cost 3*s*. 4*d*. per bottle, while Dr. Druitt admits that 1*s*. 6*d*. per bottle is the very lowest that it is possible to purchase wine of any sort fit to drink. But now comes the test. Taking a fair bottle to hold twelve glasses, this bottle of port will certainly last four days, or at least three, and remain good and drinkable to the end, but we shall be unable to drink the 1*s*. 6*d*. wine on the third day—nay, it is hardly possible to use it the second—so that it is either finished or wasted; and, in reality, 1*s*. 6*d*. becomes well-nigh 4*s*. 6*d*., vice 3*s*. 4*d*., at the end of the third day.

We are prepared to state the same with sherry. It is true that it is a much more mixed and manufactured article than port—that is, a butt of sherry is the produce of many other butts, and it is added to, and blended in many degrees, to suit taste and demand; but it is the intermingling of many wines, and not the introduction of heterogeneous articles, such as takes place at Hamburgh.

We have tasted wines in the cellars of Mr. Domecque and others at Xeres, at all prices, from 40*l*. a butt to 400*l*., dependent on quality and age, which is in sherry so large and important an element in price. Still we assert that a pure unsophisticated sherry fit to drink may be bought at 3*s*. 4*d*. a bottle, and go twice or three times as far as a bottle of Gladstone.

Yet we are as ready as anyone to admit the value of the boon which this celebrated statesman has conferred on society, by checking the price of wine, and great is, or will be, the advantage that has ensued to our purse and health therefrom. All that we object to is the making out the French and other wines to be better than they are.

We cannot help thinking that Dr. Druitt has been carried away by his subject when he speaks of prescribing a bottle of Chablis for his anæmic, overworked needlewoman. We doubt if she would like it, to begin with; we more than doubt the beneficial results to her of this compared with other wines. Could he order her eight ounces of real full-bodied, unloaded Bordeaux daily, it would be another matter. Chablis is an admirable wine when genuine, and is to Burgundy what Sauterne is to Bordeaux; but a huge quantity of thin, poor white wine passes muster as Chablis. Grown on a light and hungry soil, it has little flavour, less body, and still less fortifying principle in it, so that we fear the poor sempstress would remain anæmic despite her nine pennyworth of Chablis daily.

Dr. Druitt will pardon us for taking exception to the following passage. When treating of sparkling wines, he observes:

> "There is one method of producing these wines by art which almost surpasses that of nature. Instead of trusting to evolution of carbonic acid from the last stages of fermentation, it is better to choose sound wine and charge it with carbonic-acid gas, as soda-water is made. We are much more sure of getting a wholesome wine; and as for the gas, it produces its fillip and flies off without leaving an unwholesome fermenting substance in the stomach. I have tasted a Vino d'Asti at 2*s.* per bottle, aërated in this way, which is far above bad champagne, as it does taste of wine."

Is not this to advise the practice of just what is done by the Elbe wine makers, who send out a desperately-unwholesome stuff thus fabricated? We lately saw some of this, wonderfully well got up in shapely bottles, handsomely labelled and silver-foiled, offered to the wholesale buyer at 10*s.* per dozen, delivered in London. So, also, with regard to the Vino d'Asti. Dr. Druitt forgets that there is Vino d'Asti Spumante and Vino d'Asti Sereno, just as there is still and sparkling champagne and Moselle, all made without any help from the gazogene.

That champagne and sparkling wines often disagree is due to the excess of sugar they are apt to contain, not from any fermenting process that goes on in the stomach after it is swallowed.

But it is time to say a word about these cheap wines quoted in Dr. Druitt's book. We by no means assert that there is any error in the quantity of alcohol quoted, but our own experience goes to the effect that the cheap and inferior wines are naturally far below this range, and we greatly suspect that these have been *fortified* as well as ports and sherries.

The French sophisticators are not one whit behind their brethren in the knowledge of this art of adulteration; and an acquaintance of ours, an enlightened Englishman, has long been employed by the French Government in certain districts in examining wine, and deciding on the absence of improper mixture. On his decision we have known the kennels of streets run red with the vile stuff, and the vendors sent before the correctional tribunal and fined or imprisoned; and it would stagger our readers if we were to name some of the ingredients used. No public-house port, no wine-vault sherry could be worse compounds than the vin ordinaire, the light clarets, &c., that are sold cheap to the French themselves.

This being the fact, we must not take it as safe to drink cheap wine without some little guarantee as to quality and production.

We quite agree with the author as to the grand mistake made in England in the period of dinner, and the succession in which wines are handed round; but connoisseurs in dining know that, as a rule, sherry after soup, and red wine (Burgundy or Bordeaux) after brown meats and game, champagne after sweets, or, as a dessert wine, is the arrangement which proves most congruous. It will, however, take a generation or two to set this right, as it has taken more than that to abbreviate the time spent in the dining-room after the ladies have left it.

We also quite agree with Dr. Druitt in his condemnation of the increasing use of the puff medical. It is degrading the profession in the eyes of every sensible man, and bringing it down to the level of a trade to an extent that is lamentable. It is true that you cannot prevent Messrs. Hills and Underwood from quoting the 'Lancet' as to the purity of their vinegar, or Messrs. Turmeric as to the freedom of their pickles from copper, but the deliberate purchase of an opinion to be appended to the trade circulars ought to be discountenanced by every reputable man of science. It reminds us of a Hebrew optician applying to an ophthalmic surgeon, now no more, who was at the head of his branch of his art, for a certificate as to the excellence of his spectacles, and offering at the same time an elegant pair of the same in gold frames and tortoiseshell case. The Hebrew very nearly got his head damaged for the insult.

We may here observe that we see no reason why Dr. Druitt should give the names of certain wine-merchants, or, while giving some, why he should withhold others. It would, to our mind, have been sufficient to quote the names of the wines and their country, recommending this, and condemning that, as he saw right.

While on this subject, we must take the opportunity of protesting against the impertinent system of wine touting now pursued by so many foreign wine-merchants. As regularly as spring comes round, M. A. of Vin Rouge, Herr B. of Swiggerheim, Signor C. of Villa Corpo di Bacco, intrude themselves with so good address and manner, as quite to beguile your servant, into your study, and try to persuade you that the only plan to have *good* wine and *reasonable* is to give them an order. They stick like burrs; they wont go; they will change the wine if you don't like it; you need not even pay for it if you don't like it; and you are generally worried into ordering a case of wine you don't want, and don't like when you have got it.

In conclusion, we give a caution, if after the perusal of this 'Report on Cheap Wines' any of our readers venture on a trial, don't lay in a stock—buy from hand to mouth literally; for if a quantity is bought and not used quickly, it will probably be impossible to present it to your friends or drink it yourself.

There is no purchase so dear as bad wine. It is mischievous in its effects, painful in consumption, and becomes a potatory, slow suicide; but that price and quality have no relation to each other in some hands is plain enough to those who, having ventured to order wine at these grand limited hotels in London and its vicinity, find sherry at 8*s*. per bottle is only our old friend at 40*s*. per dozen; and Gladstone at 4*s*. per bottle is only something else of an equally common kind. As we before said, Dr. Druitt's book is only too brief; but the short time which anyone will spend in reading it will be well and profitably occupied.

ART. IX.—*On the Arrangement of the Muscular Fibres in the Ventricles of the Vertebrate Heart: with Physiological Remarks.* By JAMES BELL PETTIGREW, M.D. (Reprint from the 'Philosophical Transactions,' part iii.)—*London*, 1864.

2. *On the Relations, Structure, and Function of the Valves of the Vascular System in Vertebrata.* By JAMES BELL PETTIGREW, M.D. (Reprint from the 'Transactions of the Royal Society of Edinburgh,' vol. xxiii. part iii.)—*Edinburgh*, 1864.

THE first of these communications, lengthy and elaborate and well illustrated as it is, both by the description of dissections and by diagrams taken from photographs, is divided into four sections, devoted respectively to the anatomy of the ventricles of the fish, of the reptile, and of the bird and mammal, that devoted to the ventricles of the mammal being much the most minute and detailed. The author prepares his hearts by continued boiling (from four to six hours in the case of small quadrupeds, and from eight to ten in that of the larger) having first stuffed the ventricles with some pliant material, as bran, bread-crumbs, so as to distend without overstretching the muscular fibres, and by means of his dissections he observes that he has arrived at results "which appear to throw additional light on this complex question, and which seem to point to a law in the arrangement, simple in itself, and apparently comprehensive as to detail."

We find it utterly impossible to put the reader in possession of the intricate details of this paper, the full understanding of which requires frequent allusions to the diagrams; we must be content to supply an epitome of the conclusions to which the author arrives at. They are summed up in the following twelve statements, which we give in his own words. He observes:

"I. By exercising due care, I have ascertained that the fibres constituting the ventricles are rolled upon each other in such a manner as readily admits of their being separated by dissection into layers or strata, the fibres of each layer being characterized by having a different direction.

"II. These layers, owing to the difference in the direction of their fibres, are well marked, and according to my finding, are seven in number—viz., three external, a fourth or central, and three internal.

"III. There is a gradational sequence in the direction of the fibres constituting the layers, whereby they are made gradually to change their course from a nearly vertical direction to a horizontal or transverse one, and from the transverse direction, back again to a nearly vertical one. Thus, in dissecting the ventricles from without inwards, the fibres of the first layer, which run in a spiral direction from left to right downwards, are more vertical than those of the second layer, the second than those of the third, the third than those of the fourth —the fibres of the fourth layer having a transverse direction, and running at nearly right angles to those of the first layer. Passing the fourth layer, which occupies a central position in the ventricular walls, and forms the boundary between the external and internal layers, the order of arrangement is reversed, and the fibres of the remaining layers—viz., five, six, and seven—gradually return in an opposite direction, and in an inverse order, to the same relation to the vertical as that maintained by the fibres of the first external layer. This remarkable change in the direction of the fibres constituting the several external and internal layers, which is observed to occur in all parts of the ventricular walls, whether they be viewed anteriorly, posteriorly, or septally, has in part been figured by Senac, and imperfectly described by Reid, but has not, so far as I am aware, been prominently brought forward by any one. [The gradation in direction referred to is here illustrated in the original paper by arrows.]

"IV. The fibres composing the external and the internal layers are found at different depths from the surface, and from the fact of their pursuing opposite courses cross each other,—the fibres of the first external and last internal layers crossing with a slight deviation from the vertical; the succeeding external and internal layers, until the fourth or central layer, which is transverse, is reached, crossing at successively wider angles. [The direction of these fibres is here illustrated in the original by diagrams.]

"V. The fibres composing corresponding external and internal layers, such as layers one and seven, two and six, &c., are continuous in the left ventricle at the left apex, and in the right ventricle in the track for the anterior coronary artery, the fibres of both ventricles being for the most part continuous likewise at the base.

"VI. From this distribution of the fibres, it follows that the first and seventh layers embrace in their convolutions those immediately beneath them, while these in turn embrace those next in succession, and so on until the central layer is reached—an arrangement which may in part explain, alike, the rolling movements and powerful action of the ventricles.

"VII. The fibres of the right and left ventricles anteriorly and septally are to a certain extent independent of each other; whereas posteriorly many of them are common to both ventricles—i.e., the fibres pass from the one ventricle to the other, an arrangement which induced Winslow[1] to regard the heart as composed of two muscles enveloped in a third. It will be evident from this distribution of the fibres, that while the ventricles are for obvious reasons intimately united, they nevertheless admit of being readily separated.

"VIII. If the hinge-like mass of fibres (common fibres) which unite the right ventricle to the left posteriorly be cut through, and the right ventricle with its portion of the septum detached, the left ventricle will be found to be nearly as complete as it was before the separation took place, and to consist of four sets of conical spiral fibres—two external and two internal sets.

"On the other hand the right ventricle, and its share of the septum, consists only of conical-shaped spiral fragments of fibres, or at most of flattened rings—a circumstance which, when taken in connexion with others to be mentioned presently, has induced me to regard the left ventricle as the typical or complete one, the right ventricle being a mere segment or portion nipped off at some period or other from the left.

"IX. If the right ventricular walls be cut through immediately to the right of the track for the anterior and posterior coronary arteries, so as to detach the right ventricle without disturbing the septum, and the septum be regarded as forming part of the left ventricular wall, it will be found that the fibres from the right side of the septum, at no great depth from the surface, together with the external fibres from the left ventricular wall generally, enter the left apex in two sets; and if their course in the interior be traced, they are observed to issue from the left auriculo-ventricular opening, also in two sets; in

[1] Mémoires de l'Académie Royale des Sciences, 1711, p. 197.

other words, the left ventricle is bilateral. I would particularly direct the attention of investigators to this bilateral distribution of the fibres, as it has been hitherto overlooked, and furnishes the clue to the arrangement of the fibres of the left ventricle.

"X. The double entrance of the fibres at the left apex, and their exit in two portions from the auriculo-ventricular opening at the base, are regulated with almost mathematical precision; so that while the one set of fibres invariably enters the apex posteriorly, and issues from the auriculo-ventricular opening anteriorly, the other set as invariably enters the apex anteriorly, and escapes from the auriculo-ventricular opening posteriorly. But for this disposition of the fibres, the apex and the base would have been like the barrel of a pen cut slantingly or lopsided, instead of bilaterally symmetrical as they are.

"XI. The two sets of fibres which constitute the superficial or first external layer of the left ventricle, and which enter the left apex in two separate portions or bundles, are for the most part continuous in the interior with the musculi papillares, to the free ends of which the chordæ tendinæ are attached. These columns occupy different portions of the left ventricular cavity, and give a very good idea of the symmetry which prevails throughout the left ventricular walls.

"Lastly. The apex is opened into and enlarged, and the auriculo-ventricular orifice widened, by the removal of consecutive external and internal layers, from the fact of the left ventricular cavity tapering in two directions and forming a double cone.

"There are other points worthy of mention, such as the construction of the septum, fleshy pons, and conus arteriosus, the varying thickness of the right and left ventricular walls, the shape of the right and left ventricular cavities, &c. To these, however, allusion will be more conveniently made subsequently.

"As the structure of the ventricles, with one or two exceptions, is the same in all mammals, *man included*, I have chosen to describe the arrangement of the fibres in the ventricles of the sheep and calf, from the readiness with which the hearts of these animals may be obtained. My descriptions, however, will by no means be confined to them."

Such is the summary of the facts established in this memoir, a consideration of which will show the extent to which Dr. Pettigrew has brought our knowledge of the subject beyond that which the labours of his predecessors in the same field of observation had secured to us.

The communication which stands second at the head of this notice, that referring to the valves of the vascular system, is, like the first one, very exhaustive and well illustrated by diagrams from casts and photographs. In his researches the author has occasion, when describing the peculiarities of the auriculo-ventricular valves, to allude to the arrangement of the muscular fibres in the ventricles (the subject of the paper which we first noticed) and does not confine his observations to any particular form of valve, but scrutinizes in succession the valvular arrangements of the fish, reptile, bird, and mammal. He freely controverts, where he thinks he is borne out by the results of his dissections, the statements of former investigators, such as John Hunter, Chevers, Dr. Halford, &c., and after treating in succession various topics, presents us with the following *résumé* of the points dwelt upon in his investigations:

"*First.* An attempt has been made to point out the intimate structural relation, existing between the veins, and venous valves; how the segments of the venous valves are composed principally of white fibrous and yellow elastic tissue, arranged in at least three well-marked directions; and how the segments are so disposed, that their free margins, unless when the blood is actually passing between them, are always more or less in apposition. An attempt has also been made, to demonstrate the nature of the apposition, by the employment of plaster of Paris, injected while in the fluid state into the distal and proximal extremities of the vessels. In the veins, as was pointed out, *the closure is to a great extent mechanical;* the segments, when two are present, being forced together in the mesial plane of the vessel, by the contraction of its walls, but principally by the weight of the refluent blood.

"*Secondly.* The structure and relations of the arterial or semilunar valves, particularly in man, have been examined afresh; a more precise description than that hitherto given of the segments in systematic treatises on anatomy having been essayed. The great vessels have further been shown to bifurcate at their origins, and to be greatly thickened between the segments, which they support and incline towards each other—an arrangement calculated to bring the free margins of the segments more or less closely together, unless when pushed aside by the advancing column of blood during the systole. The sinuses of Valsalva have, in addition, been shown to vary in size; the one curving towards the other in a spiral direction, and causing the blood to act in spiral waves upon the segments of the semilunar valves, which by these means are twisted and wedged into each other, when the reflux occurs. In the arteries, the action of the semilunar valves, as has been explained, *is for the most part mechanical,* the strong fibrous rings situated at the aortic and pulmonic orifices, tending to counteract the inconvenience, which might be supposed to result from an excess of vital contractility in the vessels and the ventricles. The arterial semilunar valves may be said to differ from the bi-semilunar venous ones, in having their segments wedged together by a spiral movement, which in the venous valves, is little more than indicated.

"*Thirdly.* The bulbus arteriosus of fishes, has been shown to be a contractile organ, and to contain in its interior a system of valves, the segments of which are, as a rule, more numerous than in either the veins or arteries. They have, in some instances, tendinous bands, resembling chordæ tendineæ, running between them; and are for the most part arranged in tiers; so that the blood which is not caught by the one set, falls into, and is supported by the next. The action of these valves, as will readily be inferred, *is partly mechanical, and partly vital;* for the contraction of the bulb must be regarded as contributing to the closure. They are, therefore, an advance upon the valves of the veins and arteries both as regards their number and the manner of their closure.

"*Fourthly.* In the reptiles, as has been demonstrated, *the valves are partly tendinous and partly muscular;* while in the right ventricle of the bird *they are altogether muscular.* Here, then, may be witnessed the first trace of a self-regulating power—*actively contracting muscular fibre* taking the place of *non-contractile fibrous tissue.*

"In the auriculo-ventricular valves there is immense variety; these including most of the forms referred to, and others exhibiting a still higher degree of differentiation. They are, for the most part, characterised by the presence of chordæ tendineæ, which connect them with the interior of the ventricles, or the structures arising therefrom; viz., the carneæ columnæ and musculi papillares. The auriculo-ventricular valves, therefore, differ from the semilunar valves proper. In some instances, only one semilunar flap is present; and this may be either *altogether fibrous,* or *partly fibrous and partly muscular, or altogether muscular.* In a second, there are two flaps or segments, so arranged *that their long diameters correspond to the direction of the muscular fibres lining the ventricular cavity; the segments being continuous with the muscular fibres referred to.* In a third, the two segments are attached to the interior of the ventricle *by rudimentary chordæ tendineæ.* In a fourth, two accessory or smaller segments are added to the two principal ones; *the whole being attached by well-developed chordæ tendineæ to rudimentary musculi papillares.* In a fifth, which is the most perfect form of valve, as it exists in man and in the higher mammalia, the segments are from four to six in number, *most exquisitely and symmetrically formed, and attached by minutely graduated chordæ tendineæ to highly developed carneæ columnæ and spiral musculi papillares.*

"The action of the auriculo-ventricular valves, owing to the want of uniformity in the number, structure, and relations of their segments, is varied. It is, however, on all occasion, carefully adapted to the wants of the circulation, and to the configuration of the ventricles and ventricular cavities; these cavities, as has been pointed out, adapting or moulding the blood, and causing it to act in given directions. Thus in the fish, where the circulation is slow, and where the ventricle is conical in shape, and composed of fibres interlacing in all directions, the segments, where two are present, are forced towards each other by the uniform expansion of the blood, and by the contraction of the ventricle, in a manner analogous to that by which the segments of the bi-semilunar venous valves are approximated by the retrogressive movements of the slowly advancing venous blood, assisted to a slight extent by the vital contractility of the vessels.

"In the reptile, where the circulation is also languid or slow, the shape of the ventricle, owing to the fibres pursuing a more or less spiral direction, is that of a cone slightly twisted upon itself. As the spiral arrangement extends also to the valves, their action may be aptly compared to that which obtains in the valves of the largest veins, and in the arteries. It is, however, in the auriculo-ventricular valves of the bird and mammal, that the spiral action of the segments becomes most conspicuous; the nature of the action being unavoidably determined, by the unmistakably spiral arrangement of the muscular fibres composing the ventricles, and by the spiral nature of the musculi papillares and ventricular cavities. As, however, the action of these valves has been already explained at great length, further allusion to them at this stage is unnecessary.

"The valves of the vascular system of the vertebrata, as will be perceived from this summing up, form a progressive and gradually ascending series; the valves in the veins exhibiting a lower type than those in the arteries; the valves in the arteries being less fully developed than the valves occurring in the bulbus arteriosus and in the auriculo-ventricular orifice of the fish; the valves in the fish being less highly differentiated than the valves in the reptile and bird; these again falling short both in complexity and adaptive power to those met with in the mammal. In the mammal the valvular arrangements may be said to culminate."

These monographs must henceforth be consulted by all seeking to make themselves acquainted with the most recent advances in our knowledge of the structures of the organs of circulation. They reflect the highest credit on the assiduity and philosophical acquirements of their author.

ART. X.—*A Contribution to the Anatomy of the Amphibian and Reptilian Retina.* By J. W. HULKE. pp. 26.

THE retina of the following animals has been the subject of Mr. Hulke's careful and minute researches: the frog, the black and yellow salamander, the common snake, the blind worm, Spanish gecko, land-tortoise, the water-tortoise, the edible turtle. The method adopted by him was to examine the retina immediately (when possible) after the decapitation of the animal, and both alone and with chemical agents, hardening the sections made of the membrane by alcohol or by an aqueous solution of chromic acid, and after imparting a stain to those sections by iodine or carmine, rendering them transparent by glycerine, either pure or diluted.

The results thus obtained showed, he states, a uniform type of structure of the retina of all the amphibian and reptilian species he examined, subject, however, to modifications justifying Müller's anticipation, that a special study would bring out characters sufficient to enable the anatomist, from the examination of a piece, to name not only the class of animals, but even the genus and species to which it belonged.

The following, common to all the species named, are the layers of the retina which are distinguishable, and have been described by Mr. Hulke, and of which, in four instances—the frog, turtle, common snake, and gecko, figures—(diagrammatic vertical sections) are appended.

1. Layer of rods and cones = Bacillary layer = Jacob's membrane.
2. Layer of outer granules.
3. Inter-granule layer.
4. Layer of inner granules = Bowman's nummular layer (layers 2, 3, 4, collectively form H. Müller's granule layers = Bowman's agglomerated granules).
5. Granular layer = Bowman's grey vesicular matter. The first of these terms, the author says, he employs because it involves no opinion respecting the nature of the tissue.
6. Ganglionic layer = Müller's nerve-cell layer = Bowman's caudate nucleated vesicles.
7. Optic nerve-layer.

These layers, he observes, are traversed radially by the connective fibres named after their discoverer, Müller, and are collectively pervaded by an interstitial net of great delicacy, first more minutely investigated by Schultze.

Mr. Hulke states that he has not been able to confirm Müller's belief that bloodvessels are traceable in the retina of the turtle as far as the inner granules.

The first notice of these researches, we are informed by their author, was published in the 'Proceedings of the Royal Society,' vol. xiii. for 1863–64. The later and fuller account, that which is before us, has been given in the Royal London Ophthalmic Hospital Reports, and thence reprinted. Now, considering their interesting nature, the difficulty of the investigation, and their value as a contribution to comparative anatomy, we cannot but express regret that the latter details, for the sake of greater publicity, had not appeared in the same series as the first, or in the 'Transactions of the Society,' in which, for their merit, they might, we think, be held deserving of a place.

ART XI.—*The Endoscope as an Aid in the Diagnosis and Treatment of Disease.* By FRANCIS R. CRUISE, M.D., &c.—*Dublin,* 1865. pp. 43.

MUCH credit is due to Dr. Cruise for the perseverance and skill he has displayed in his endeavour to bring the endoscope into use. That in his hands and in the hands of a Desormeaux it may be employed for the detection of lesions, in parts such as the urethra, there can hardly be a doubt. But we think it is very doubtful whether the instrument can be so simplified and rendered easy of application as ever to be employed except by an expert, who may have attained a mastery of it by a special training with a more than ordinary aptitude fitting him for its study.

We apprehend, too, that from the difficulties attending its use it may be abused, and, like the laryngoscope, serve the purposes of the charlatan. We should never forget that exact observers are more rare than the instruments of precision.

As we cannot give any adequate idea of the form and structure of the instrument, as described by Dr. Cruise, without explanatory woodcuts, we must refer those of our readers who wish to make themselves acquainted with it to the author's paper on it, which has been republished, with additions, from 'The Dublin Quarterly Journal of Medical Science' for May, 1865.

ART. XII.—*Om Dysenterie-Epidemien i Kragerö Lægedistrikt i* 1859. Ved C. HOMAN og C. HARTWIG. (Aftrykt fra 'Norsk Magazin for Lægevidenskaben.')

On the Epidemic of Dysentery in the Kragerö Medical District in 1859. By C. HOMAN and C. HARTWIG. (Reprint from the 'Norwegian Magazine of Medical Science.')—*Christiania,* 1860. 8vo, pp. 116, with Maps and Tables.

IN the years 1808, 1809, and 1810, a severe dysentery devastated many of the southern districts of Norway. From that time this disease was probably unknown as a widespread malignant epidemic until the summer of 1859, when it raged with fearful violence in the southern half of the jurisdiction of Bratsberg, and in some other parts of the southern portion of the kingdom. Nowhere, however, was it so severe as in the Kragerö medical district, where, in a population of 11,600, upwards of 2000 were attacked, of whom 400 died. The authors of the able and comprehensive Report before us having superintended the treatment, and having had the opportunity of observing the majority of the cases, have done well in communicating the results of their experience to the profession.

Having stated the general plan of their work, the authors enter into a full description of the symptoms of the epidemic, but as these do not differ materially from the usual phenomena of dysentery, it will suffice to observe that the writers assign to the disease a stage of incubation, varying in duration from two to eleven days. Death occurred, in the fatal cases, during an algid-cyanotic stage, which lasted from a couple of hours to as many days. Only in a few instances was the fatal result preceded by symptoms of peritonitis, where perforation of an ulcer might be inferred to have taken place. The average duration of the disease might be set down at from fourteen days to three weeks. Complications, if prolapsus ani be excepted, were very rare. Parotidean swellings were observed in a few cases ; abscesses or ulcerations were only rarely seen. Aphthæ were not uncommon. A petechial eruption was occasionally met with shortly before death, as was, in a couple of instances, an erythematous eruption. Of sequelæ the principal observed were ascites, which was rarely, however, of much importance, and acute rheumatism. The occurrence of the latter was the more remarkable, as it is a rare disease in the district. Thus in the year immediately preceding, when sthenic diseases were particularly prevalent (in 1858, 151 cases of pneumonia and pleuritis came under the author's treatment), Drs. Homan and Hartwig met with only six cases of acute rheumatism ; but in 1859, while no case of this disease occurred which was not preceded by dysentery, no fewer than 40 were met with where it appeared as a sequela of the epidemic. The rheumatic affection was not very severe. Usually, in some days after the cessation of the dysentery, pains set in in the different joints of the upper and lower extremities, with slight swelling and some tenderness over the same ; there was no tendency to well-marked effusion, nor was the fever high. Most of the cases yielded in the course of three or four weeks to a gently diaphoretic treatment. In a couple of instances the disease was combined with cardiac affection.

"Post-mortem examinations were made in six cases—viz., on the 4th, 6th, 8th, 9th, 14th, and 28th days. In the first (on the 4th day) the mucous membrane was found in the large intestines from the anus up to the valvula coli, almost everywhere lined with a greyish red investment, most marked inferiorly towards the rectum : this investment was easily scraped off, and beneath it the mucous membrane presented well-defined uniform redness. Subsequently less of this investment was found, while the mucous membrane exhibited a highly velvety redness, which was nearly uniformly diffused, though most strongly marked in the rectum, the flexures, and the cæcum. In one case it was less in both the superior flexures. In these injected places a number of small ulcers were now seen, which on the 10th and 14th days were more distinct, and began to be larger, to coalesce and to acquire an elevated fundus, presenting a watery appearance. The ulcerations were best marked on the projecting folds of the intestine. At a later stage the ulcerations were still more advanced, and the tissue under and around them was then infiltrated through all the coats of the intestine, to the thickness of several lines, and was at the same time softened. This was the case particularly in the rectum and sigmoid flexure, and the mesenteric glands were here swollen. The small intestines and the stomach were invariably healthy, as were the other abdominal organs, except that the kidneys were found somewhat congested." (pp. 9, 10.)

Among the conclusions arrived at by the authors with respect to the treatment of the epidemic are the following: that ipecacuanha employed in an emetic dose in the first stage of the disease was in several cases serviceable as an abortive, but that so used at a later period it was injurious; that small medicated enemata did not answer the purpose for which they were given, but that large injections of tepid water were extremely useful, and ought to be employed at least twice or three times a day.

The Krageró medical district is situated in the southern part of Bratsberg, about half way between Christiania and Christiansand. It contains a surface of 14·65 square miles, with about three miles (= twenty-one English miles[1]) of coast, and it extends from Söen in a north-western direction about eight (fifty-six) miles into the country. It contains 16,040 inhabitants, 4445 of whom live in the towns of Kragerö, Stathelle, and Langesund, while 11,595 reside in the country parts. The town of Kragerö is described as being irregularly built, with narrow, badly paved streets, and without large gardens or open spaces. The houses, both in the town and suburbs, are all of wood, old, small, and generally overcrowded. The channels and sewers are bad. The water is good and pure, but insufficient in quantity, at least in summer. Langesund and Stathelle are much smaller towns, the former containing only 753, and the latter 459 inhabitants. As to the mode of life, it may be stated that too little flesh meat is used; this is the case especially in the forest districts, while along the coasts the ease with which fish is obtainable makes this the principal part of the animal food. All food is, in general, used regularly dressed. Nothing is consumed in the smoked state. Fish is eaten fresh, or dried, or salted, and in the last case is always sufficiently salted. Potatoes, which constitute an essential part of the food, have of late years here, as elsewhere, become spoiled soon after being dug out. The use of brandy in the town and environs is considerable. The habits of the people as to cleanliness leave much to be desired. The climate must, on the whole, be looked upon as healthy; meteorological observations have, however, not been instituted. The average mortality during the last five years was in Kragerö 1·88 per cent, in Sandökedal 1·57, in Drangedal 2·00, and in Bamble 1·72 per cent. It should, however, be observed that in 1856 scarlatina prevailed throughout the whole district as an extensive and malignant

[1] The Norwegian is longer even than the Swedish mile, being equal to about seven English miles.

epidemic, so that the mortality was probably greater than usual.

Propagation of the Disease.—The first case of dysentery was imported into the district in the end of April, but it was not until the beginning of June that the extension of the malady as an epidemic came under the notice of the authors, about twenty cases being observed in the farms adjoining the place where the first patient was introduced. The disease now spread rapidly to different points in Sandökedal, Drangedal, and Bamble, and several times reached the town of Kragerö, though without obtaining any firm footing there. It continued to rage violently during the months of June, July, August, and September, might be considered to have ceased in Drangedal and Bamble in the first half of October, but still occurred sporadically in Sandökedal in November and December, and even at the end of the year could not be looked upon as having altogether ceased. Up to the beginning of October the number of those attacked and dead were: Population, 16,040; of these 2130 were attacked, being in the proportion of 13·2 per cent. The fatal cases were 413, being 19·3 per cent. of those attacked, and 2·57 per cent. of the population. Subtracting the towns, the proportion for the rural district is: Population, 11,595; attacked, 2077; percentage of population, 17·9. Died, 406; percentage of those attacked, 19·5; percentage of the population, 3·50.

The *mode of diffusion* of the epidemic is the point on which the observations made by the authors throw the most light. The writers believe that they can show that the disease arose exclusively from the introduction of contagion, while the nature of the district, containing a scattered population on broken ground, rendered it easy to follow its course. In the case of cholera, the conveyance of the contagion from the towns could be readily demonstrated, but the fact that the disease did not spread in the rural districts led to slight value being attached to its contagiousness, while it was shown that wherever it gained a footing a predisposing gastric morbid constitution had existed. "Our observations," add the writers, "upon the present epidemic of dysentery prove that it was not based upon a morbid constitution, and that contagion alone sufficed to produce the epidemic before any gastric morbid constitution had prevailed." (p. 45.)

As to the nature of the contagion, the authors believe it to be volatile, and they state that they have distinctly observed that it was capable of being conveyed by healthy persons. This occurred, however, only where the healthy individual was a constant resident in the infected house, or had remained for some time in an infected house in close proximity to the sick. The course of the epidemic more frequently followed the route taken by travellers from infected localities, and it was observed that the disease first appeared precisely in those places where passengers usually stopped to rest.

The authors next enter into the details of very numerous instances in which they have been able to attribute the outbreak of dysentery in the several localities to imported contagion; and they do this the more fully, as it is only, they observe, in a country like Norway, and especially in districts such as theirs, that the contagious origin of disease can be satisfactorily traced. As to the period of incubation, it would, according to their observations, as we have already stated, vary from two to eleven days.

Among *predisposing causes* the authors mention, as an important element, the gastric morbid constitution which prevailed in the months of June, July, August, and September, 1859, though it was met with in only one-sixth of the cases. That this constitution was so predominant in 1859 as compared with 1858, was probably due to the temperature, which in 1859 was so essentially different from that ordinarily existing. The summer was remarkable for an unusually small amount of rain, and for great heat, especially in the beginning. The temperature was then almost the same night and day, and the quantity of dew was not great. At the end of July the nights and evenings began to be cooler and moister, and there was a greater variation in temperature between the day and night. The same condition lasted throughout August, while rain, moreover, was still wanting. It was not until the beginning of September that rain came, which increased, and October was distinguished by unsteady weather and great storms. The authors remark that it is well known that hot and dry summers in many ways favour the gastric morbid constitution, and that the unusual temperature in 1859 had in other places also produced a similar effect.

Among other circumstances which might be looked upon as predisposing causes the authors enumerate the state of the water, food, and soil. The supply of water in the district is almost invariably derived from dug wells, and is to be considered as surface water. In a very dry season the quantity of such water available would necessarily be insufficient, and recourse would be had to stagnant marshes, &c., to make good the deficiency. The evil effects of such a course are obvious. Still, the example of the town of Kragerö, where the supply of water was extremely bad, proves that bad water was not sufficient to produce the dysentery; and other instances are quoted to show that the use of good and pure water failed to prevent the spread of the epidemic, where the contagion was introduced to the same extent and with the same malignancy as elsewhere.

As to food, the authors observe that there neither was nor had been any deficiency in it, nor were they aware that any article of diet in common use was of inferior quality. When the epidemic broke out, the potatoes had been eaten up; the same was the case with the corn raised in the district; what more of the latter was obtained was generally imported from Denmark and the Baltic, nor did it appear that what had been so imported in the year of the epidemic and in the preceding year was worse than usual. Some had attributed the outbreak of the disease to the use of milk supplied by animals who, in consequence of the drought, had used bad water, but the authors show satisfactorily that this could not have been the case. They rather attribute the comparative exemption of the towns—for which the statement about the milk, if correct, might have accounted—to the freer use of animal instead of vegetable food.

While the authors do not find reason to attach any weight on the great scale to the nature of the geological formation of their district, they are in a position to show that in the special localities the state of the soil is of the greatest importance.

A popular idea was, that those who had once had the disease were not liable to be again attacked by it. The authors cannot deny that they saw several old people who stated that in the epidemic of 1809 they had suffered from dysentery, and that in the recent epidemic they had with impunity tended the sick.

The authors conclude their valuable and interesting report with some remarks upon the general treatment of the epidemic, upon the method to be adopted by the medical attendants in the discharge of their duty, and upon the regulations which they thought it advisable to recommend in a hygienic point of view, upon the manner in which these were carried out, and what results were witnessed from them. They append some valuable tables of the details of the epidemic, and also two well-executed maps of the affected districts, and a diagram which exhibits at a glance the ratio of mortality in the several weeks during which the epidemic prevailed. On the whole, we consider the volume before us to be a most important contribution to medical literature in general, and to the history of epidemics in particular.

ART. XIII.—*Ophthalmic Hospital Reports,* vol. iv. parts 2 and 4. (*Papers by* Messrs. TEALE, CRITCHETT, and BOWMAN, *on Cataract Operations.*)

FOR many years now the best oculists in this country and abroad have been well agreed, as a rule, concerning the treatment of the most important class of surgical cases with which they have to do—viz., cataract. Since the time when the very unscientific operations for displacement of the cataract were abandoned, cataracts have, speaking generally, been removed by one of two methods, by "extraction" or by "solution" as they have been named. But an important modification of the latter operation had been introduced, during the period of which we speak, called "linear extraction," which, as a name, does not at all define the operation; and, moreover, it is more nearly allied to solution than to extraction proper. In short, linear extraction consisted, in the first place, in the operation for solution somewhat extended, the material of the lens being subsequently evacuated by a small opening through a grooved instrument, its removal by the tedions process of nature not being waited for.

The operations for cataract are now quite different. It has become a new study since the oculists have revised their tables of statistics, and have found how very unsatisfactory are the results of extraction when they are unsatisfactory, and how great a risk is run for the wonderfully perfect results of a "good case of extraction." The great changes lately introduced, which are rapidly being substituted for the old methods, are, theoretically, rather in the direction of linear extraction than of the older operations of extraction proper or solution—that is to say, the cataract is, in one way or another, extracted or removed altogether, and by a linear opening—a section, at any rate, much smaller than that of half the circumference of the cornea required for extraction proper. By this means the many and great risks of the last-mentioned operations seem to be avoided. Nevertheless, quite original notions have prevailed, and perhaps now the most usual substitute for extraction is the operation of "spooning out" the cataract after the performance of an iridectomy. Unfortunately, no two operators now are agreed in their practice; and when we inquired lately of a friend just returned from Germany, on a tour of the principal eye hospitals, what the German ophthalmologists were doing for cataract, he said, "They are all at sixes and sevens, as you are in England." Extraction and solution and linear extraction are, however, not quite abandoned, they are only generally superseded; and, in addition to the different plans for iridectomizing and spooning out cataracts, we have to consider, in relation to the modern surgical treatment of cataract, the "suction" instruments and the modes in which they are used. Our limits will not permit us to go into any details, but the use of the suction instruments, which are now adopted, constitutes a manifest improvement on the old operation of linear extraction, and deserves a more than passing notice. We do not mean that the principle is entirely new, but it has been neglected, and is now duly appreciated and improved. It is a manifest improvement, inasmuch as linear extraction was resorted to in consequence of the lens matter becoming fluid when the lens capsule has been long enough and sufficiently lacerated so as to expose the cataract to the aqueous humour, and the tension of the globe being considered sufficient, at the time of the final removal or evacuation of the lens matter, to expel it, together with the aqueous humour, along the groove of the curette. But, of course, the tension, and therefore the expulsive force, became less and less, and was soon exhausted, and then the remaining lens matter, or some small portion of it, could perhaps be coaxed out by manipulation with the curette. Not, however, without danger; in such manipulation the iris was bruised, and the posterior surface of the cornea was very probably scratched, and, after all, some lens matter was left behind by even the most adventurous of operators. And when they so left some lens matter behind, the cases did better, perhaps, than when they insisted on removing every minute portion of it with the curette. Then, again, it was not easy to determine when, after the preliminary operation with the needle, the linear extraction should be performed; if too soon, some considerable part of the lens matter, remaining firm, could not be evacuated, or some such fragment would obstruct the groove of the curette, only letting the aqueous humour escape by itself, without evacuating, in the current of the aqueous humour, the cataractous matter behind it, whilst, if the final operation were long deferred, all the ill results of pressure on the iris and ciliary processes might be superinduced.

In the 'Ophthalmic Hospital Reports,' a journal which, we must add, is far less known in the medical book-clubs, libraries, and societies than it deserves, Mr. Teale, of Leeds, has lately[1] given his method and the results he has obtained. His suction instrument may be briefly described as a curette, the groove being made tubular by a flat roof, and communicating with a hollow space in the handle, with which again communicates an elastic tube, at the further end of which is a mouth-piece for application of the suction-force by the operator. From post-mortem experiments in which healthy lenses were mostly or altogether removed by this suction instrument, Mr. Teale says, "I concluded that a soft cataract would pass through still more readily." He quotes only one case, his first, a traumatic cataract, for which class of cases the suction instrument, employed at some time sooner or later after the lens capsule has been lacerated, is, we know, very efficient and perfectly satisfactory. But we fail to understand, and it is an important question, if Mr. Teale has been generally able to remove the whole of a spontaneous cataract *in adults*, by suction, at one operation, performed when the anterior capsule has been preliminarily opened. In our experience it is best (indeed necessary) to freely open the anterior capsule first, and, *at a second*

[1] Loc. cit., May, 1864, p. 179; and see also a pamphlet on the subject subsequently published by him.

operation, to remove the lens matter by suction, and, even in young people, thus to defer, for a short time, the completion, when the preliminary operation has not been fulfilled by the very origin of a traumatic cataract, or the cataract be naturally diffluent. For, otherwise, fragments of lens frequently obstruct the tube, and though, as Mr. Teale suggests, the curette may be removed, and these fragments blown out, the tube is again and again filled by some firm lens matter which would require a dangerous amount of suction force to extract through the tube. Mr. Teale duly insists on the importance of freely lacerating the anterior capsule, of carefully avoiding the posterior capsule, and of removing all the cataractous matter that can be removed, with the end of the suction curette *in the pupil*. He describes other instruments (different ways of applying the same principle) contrived by himself and others, but none probably so good as that first made for him and here described. Removal of cataract by suction-instrument, used at the right time, when the cataract is fit to be so removed, must be considered a most valuable improvement in the treatment of one class of cataract cases.

Messrs. Critchett and Bowman, in a subsequent number, the last published, of the same journal,[1] treat of some of the new operations for spooning out cataracts, and now we are threatened with as many varieties of cataract spoons as, of late years, we have had of knives—that is to say, each operator has one peculiar to himself. This operation is called at the Moorfields Ophthalmic Hospital the traction method, as distinguished from that last mentioned, and the various "spoons," asat first, they really were, traction instruments. An iridectomy is first made, now generally at the time of the operation for the removal of the lens, as was proposed by Waldau (Schuft) who invented the "spoons," and the method of their use. But some have preferred to iridectomize as a preliminary operation performed some few weeks before that for the removal of the lens. We must well consider this new method of removing senile cataracts, for Mr. Critchett informs us he has lately adopted it exclusively, and Mr. Bowman says that in this operation the average of success "has uniformly been higher than that of the flap operation in my hands." It is of the greatest advantage that chloroform can be administered without any fear of the subsequent retching that may occur. As to the operation itself, the various precautions and details are so minute and vary so much in the hands of different operators, that they cannot be given and compared here. Among other considerations is that of the incision for the iridectomy to be first accomplished; for this purpose the broad spear-shaped knife is thrust across the anterior chamber, and, in this

[1] Ophthalmic Hospital Reports, 1865, vol. iv. part iv.

operation, as the anterior chamber is undiminished in size, and it is immaterial if the lens is wounded, we may quite safely adopt this now popular instrument. Mr. Critchett's incision extends backwards at either extremity, so that it forms a small section of a circle larger than that of the corneal circumference; whilst Mr. Bowman says of his incision, that it "follows the rim of the chamber, passing through the corneo-sclerotic junction" of about "a fourth of the circumference of the cornea." The iridectomy is then performed, and the anterior capsule *very freely* opened. The spoons, or rather, as they must now be called, the traction instruments, are of a long oval shape, and but slightly concave; at the extremity of Mr. Critchett's the edge is slightly recurved (vectis), whilst that of Mr. Bowman has a margin at an obtuse angle with the body of the instrument. This is introduced between the posterior capsule and the nucleus of the lens, and, this being first removed, any remaining fragments are also drawn out carefully, and not expelled, as in the flap operation. We may add here that, in our practice, we have found it advantageous, in the first introduction of the traction instrument, to pass it under the nucleus of the lens somewhat sideways, so as to make use in some degree of the *edge* of the instrument in separating it from the posterior capsule. Mr. Critchett insists much on the necessity of removing *all* the lens matter in the anterior chamber, and also, particularly, any there may be *left in the section.* The objections to an iridectomy *upwards* are rather mythical; the disfigurement is *nil,* and Mr. Bowman points out that whatever the objections may be, they are not *practically* found to exist.

"In addition," he says, "the inestimable advantage is gained of absolute immunity from all the complicated annoyances and dangers of prolapse of the iris; so that the after-treatment becomes comparatively simple, and the recovery generally rapid and satisfactory. . . . It has its own risks and liabilities, especially those attending the use of the tractor, but when its steps have been all well accomplished, which they ought to be under chloroform, the patient has a much better prospect of passing safely and speedily through the healing process, and of recovering his sight, than if he had left the operating table with a perfect iris, but with a large flap wound and its attendant risks. . . . The advantage of the traction wound is that it is smaller, that it can hardly undergo displacement, that it cannot be complicated by prolapse; it also admits of being more safely examined, and, if necessary, topically treated; I speak in each case of the wound made upwards. Both operations involve a danger of iritis," &c.

Mr. Bowman would not altogether abandon the old operation of flap extraction, he says:

"The surgeon will be strongly impelled to the traction operation in cases where a rapid recovery or escape from tedious treatment, especially of that for prolapse, is more than usually desirable—*e.g.*, generally among the poor, in old, very weak, or nervous persons, in those of an irritable habit, in inflammatory subjects, also in cases where the after-care of an experienced nurse cannot be procured; also in unmanageable patients, as lunatics, or the perverse, obstinate, or self-willed."

Again, he writes that "flap extraction may still be preferable under certain circumstances;" and that it *is* so we have no doubt, neither have we any but that it should be adopted only in those cases in which those circumstances which are well known as favourable to extraction exist. Mr. Critchett although, as we have seen, so very much in favour of scoop extraction, says:

"On the other hand, I feel that I ought in all fairness and candour to state as the result of my experience that iritis is more frequent after scoop extraction that when the old method is used; also that the occurrence of false membrane with adhesions is by no means uncommon, which vary exceedingly in the degree both of density and opacity, and also in other qualities. These secondary membranes often cause considerable disappointment," &c.

Mr. Bowman gives a long, particular, and very interesting account of the secondary operations for senile cataract. For our part, we intend at present to operate by the suction method on all cataracts that can be broken up by a previous needle operation, by flap extraction when all circumstances favour a good result, and as these cases are certain to be few, by the traction method, in the majority of cases of senile cataract.

Art. XIV.—*On Synostotic Crania among the Aboriginal Races of Man.* By J. Barnard Davis, M.D. With eleven quarto plates.—*Haarlem*, 1865. 4to, pp. 39.

This is a Dutch reprint of a very valuable paper contributed to a scientific society by Dr. Davis, the well-known author, in conjunction with Dr. Thurnam, of the 'Crania Britannica'—a standard work, sections of which, as they appeared, were reviewed in this periodical, now happily brought to a conclusion.

Synostosis, or the premature and abnormal obliteration of the cranial sutures as a cause of deformities and irregularities of the human skull, has been scarcely noticed by English writers; and by continental authors, has been studied almost exclusively in Cretin skulls. Yet, as Dr. Davis most justly insists—

"The subject of synostotic skulls, as well as of those deformed in other ways, deserves far more attention and estimation among craniologists than it has hitherto received. The human cranium is subject to developmental irregularities; to pathological conditions; to artificial deformations, both voluntary and involuntary, as Dr. Gosse has named them; and to posthumous modifications, all of which interfere with and alter its normal forms. When it is recollected how high a degree of importance has been justly attributed to the normal configuration of the skull in the study of anthropology—a science which has such various and momentous applications—the need of a close, full, and discriminating investigation of the kinds and degrees of the influences interfering with such configuration must be at once apparent." (p. 35.)

A very telling illustration of the truth of these remarks has recently been furnished by Dr. Davis himself, in his paper on the far-famed Neanderthal skull. This most ancient cranium has ever since its discovery given rise to multitudinous speculations and hypotheses, as well respecting the date of its former possessor's existence on the earth, as concerning the affinities that aboriginal man bore to the existing human race and to the beings next thereto in the animal scale. For it so happened that this old skull did not accord with the model examples of craniologists, but was so irregular and misshapen that it afforded a most excellent peg for those philosophic gentlemen who revel in the idea that their ancestors were apes, whereon to suspend the hypothesis that the prehistoric races of men, as exemplified by this skull, were only a slight remove in cranial development from monkeys. But alas! for this pleasing inference, its much-valued illustration collapsed under the examination of Dr. Davis, who showed it to be not a normal skull, and therefore not a fitting example of a race, but one abnormal and deformed by synostosis.

The premature growing together and consolidation of two cranial bones puts a stop to expansion in the one direction, and compels the skull to enlarge, as a rule, in another at right angles to it. For example, synostosis of the parietal bones from obliteration of the sagittal suture arrests lateral expansion and produces a transversely contracted, or long skull; the direction taken by future enlargement being at right angles to that of the synostosis.

The consideration of the abnormal ossification of sutures involves that likewise of the causes and effects of this process, "not merely upon the development and the form of the calvarium, but upon the brain also, and upon its functions." The subject of synostosis must therefore be replete with interest both to the anthropologist and to the physiologist; and, as medical men and students of natural science, we hail the discovery of fresh facts concerning man, and of additional means for comprehending and studying him, such as are supplied us in the interesting essay under notice.

Dr. Davis prefaces his description of skulls of aboriginal races by a notice of writers who have remarked the influence of synostosis on the conformation of the cranium, and made it the subject of investigation. Soemmering appears to have been the first to notice the fact, and among the principal writers upon synostosis are Lucae, now Professor of Anatomy at Frankfort-on-the-Maine, and the indefatigable worker, Professor R. Virchow, each of whom has noted and named the many varieties of abnormal and irregular crania. Of some instances of cranial deformity, the two writers last named were disposed to seek the cause in irregularity of the centres of ossification, an opinion controverted by Dr. Davis.

The memoir, giving the descriptions of aboriginal calvaria, is followed by a chapter of additions, written some time subsequently, including additional observations, and a few conclusions deduced from them, and of much importance. Among these conclusions are the following:

"1, That deformation of the calvarium is by no means a necessary and invariable consequence of synostosis; 2, that the deformations resulting from synostoses do not necessarily and invariably follow the premature ossification of those sutures to which they have been ascribed, and are sometimes occasioned by synostoses, to which they have not been attributed; 3, that scaphocephalism (boat-shaped crania) is far from being the usual result of the too early ossification of the sagittal suture; 4, there is a series of crania which, in their natural developments, present such a degree of length and of narrowness, as almost to deserve to be distinguished by the name of natural scaphocephali."

Further, some writers upon synostosis—

"Have unhesitatingly maintained that those races who interfere with the natural forms of the skull are pre-eminently distinguished for premature ossification of the sutures, and hence, that all artificial compression of the head predisposes to synostoses. My opportunities for further observation induce me to doubt whether synostosis of the cranial bones is more frequent in races who distort the skull, and whether artificial compression has any influence in causing premature ossification of the sutures.

"Certain races possess a super-activity in the process of ossification of the bones of the head. Many of the negro and Australian races are remarkable for the great thickness and weight of their skulls; and, in the same way, these races are distinguished for a proneness to closure of the sutures, which takes place at an earlier period of life than among European races. . . . . . . Professor Gratiolet has said, not only that the growth of the brain ceases sooner in those races in which the sutures close early, but also that there is a difference between the higher and the lower races as to the order in which the sutures are closed normally. In the latter, the anterior sutures close before the posterior, and in the higher races it is the reverse, the posterior sutures close earlier than the anterior. M. Gratiolet bases an argument for the greater perfectibility of those higher races upon this asserted fact."

Dr. Davis quotes from an inaugural essay by Dr. Joseph Schade, the history of an extreme case of scaphocephalism, recounting the mental characteristics and manners of the individual when living. On this history we give the following abstract:

"The boy perceived that everyone who first beheld him was frightened at him, and his schoolfellows shunned and vexed him. Yet in the school he learned easily . . . . and had as much desire for learning as any healthy youth. His repulsive appearance proved a hindrance to his being taught a trade in the usual manner, and he was obliged to take refuge in the workhouse, where he learned to weave. Wishing, however, to gain his own livelihood, he left the workhouse, and tried to get employment as a weaver; but being still persecuted on account of his deformity, he returned to the workhouse, where from vexation he took to habits of intemperance, and grew quarrelsome and very offensive, exhibiting, nevertheless, no suspicion of unsoundness of mind. He died at length of pleurisy. His parents and relations were well formed, and his birth was normal. The bones of the cranium were found to be thin and light, and scarcely a vestige of sutures was discoverable, the whole cranium appearing to be formed of one bone." (pp. 36, 37.)

With reference to this question of the connexion between synostotic deformity and the cerebral functions, Dr. Davis holds the opinion that synostosed bones are unusually prevalent in the crania of criminals: "That these synostoses (he writes, p. 25) in many cases influence the brain, by arresting its development, appears to me equally indisputable. The low moral character and base propensities generally displayed by microcephalic idiots is also admitted, and gives material support to the view I have taken"—viz., that compression of the hemispheres by a synostotic calvarium arrests their development and impairs cerebral activity —at least, so far as to entitle it to further investigation.

The dissertation is accompanied by a table of measurements of the aboriginal skulls described in the text, and by eleven beautifully lithographed plates of crania, drawn of the natural size.

ART. XV.—*An Inquiry into the Possibility of Restoring the Life of Warm-blooded Animals in certain Cases where the Respiration, the Circulation, and the Ordinary Manifestations of Organic Motion are exhausted or have ceased.* By BENJAMIN W. RICHARDSON, M.A., M.D. pp. 15.

THIS paper, which is from the 'Proceedings of the Royal Society for 1865,' differs but little except in mode of expression from an original communication contributed by its author to this Review, and which is to be found in our April number for 1863, with the title of 'Researches on the Treatment of Suspended Animation.'

Dr. Richardson starts with the idea,—certainly not a new one—that life and flame are analogous, both depending on the consumption of oxygen, and that death is "one"—i.e., "in every form of death, the elementary cause, however it may commence, lies in a simple variation of the natural act of the combination of oxygen with the blood."

The subject is of such a recondite nature that we think it best at present to decline offering any remarks on Dr. Richardson's views, merely observing that they are always ingenious and plausible, and his experiments, such as are described, seem to have been made with all the skill and intelligence that might be expected from such an accomplished physiologist. Even to those of our readers who are acquainted with the earlier paper, we can recommend the later for perusal, qualifying however the recommendation, in accordance with what we have already said, that its claim to attention lies more in novelty of treatment and arrangement of results than for any new results brought forward.

As his "general conclusions and indications" are comprised in one paragraph, we quote it, would that we could say with as sanguine a feeling of an ultimate success as that with which it is written. Be that, however, as it may, we have no doubt, if he keep his promise of continuing the inquiry—using all possible care to avoid error in experimenting and in reasoning from results, he will be amply rewarded, if not by that success which he looks for, at least by the discovery of facts interesting to the physiologist and useful to science:

"I have already shown that artificial respiration is of service only when the blood from the heart is being still distributed over the capillary surface of the lungs—or, to return to the simile with which I set out, that the process is simply one of fanning an expiring flame, which once expired will not, in spite of any amount of fanning, relight. The further conclusion to which I am this moment led, goes, however, beyond the process of artificial respiration; returning again to the simile, I venture to repeat that, even when the heart has ceased to supply blood to the pulmonic capillaries, during the period previous to coagulation, the blood may be driven or drawn over the pulmonic circuit, may be oxidized in its course, may reach the left side of the heart, may be distributed over the arteries, and that, thus distributed, it possesses the power of restoring general muscular irritability and the internal manifestations of life. Hence I infer that resuscitation, under the limits named, is a possible process, and that it demands only the elements of time, experiment and patience for its development into a demonstrable fact of modern science."

PART THIRD.

Original Communications.

Art. I.

On the Nature of Eczema and the Law of Elementary Lesions.
By Tilbury Fox, M.D. Lond.

All that has been written of late in reference to eczema has exhibited a certain indistinctness altogether unsatisfactory to the critical student—to the man who wishes to make whatever knowledge he may obtain of practical service to him in his dealings with disease. In the domain of dermatology, clearly, the principles of general medicine have not played their fair share of influence; indeed, it seems to be forgotten that any doctrine, therapeutical or other, derived from a consideration of any special class of facts of disease, is alike applicable within certain limits, to all instances; in other words, that there are certain prime laws which obtain in all cases; and the specialist is in error if he be not guided by the researches of the many, and in turn aid by his conclusions observers in departments of general medicine other than his own. Cutaneous medicine has not been sufficiently gauged by the results obtained in other fields of inquiry.

The doctrines of authority, in some instances, too, have been rather unjustly treated, whilst fashion has had its usual impulsive sway. The introduction of continental views within a recent period—views opposed entirely in spirit to the teaching of our countryman, Willan—has been rapid and pretty successful. I propose to examine the subject of eczema as closely and carefully as possible, and discuss some very home questions as to its meaning and nature, firmly believing that many of the novel points put forward for our acceptance are unsound. The premises of many of the propositions, may be, are true, but the conclusions wrongly worked out; let me add also, that my great wish is to attempt to harmonise much that seems "opposite and irreconcileable" in the writings of various authorities. In my recent work ('Diseases of the Skin'), the outline only of the subject was given; the fuller discussion being reserved for the present occasion. Reviewers have been kind enough, however, to call special attention to the position taken in reference to Willan's system. It really requires a little more than one's usual courage to hold in the face of the expressed opinion of highest authorities anything approaching Willan's views; and it was a relief to read in reference to the protest ventured, that "the older practitioners will perhaps be surprised to find the most recent writer of this subject falling back on Willan's arrangement; but we are quite sure the student will find this the simplest and best arrangement for the purpose of acquiring a knowledge of the various forms of skin disease." And once more: "the conclusions in regard to the independent existence of eczema, which we believe to be sound, are opposed to much of the teaching of modern dermatologists, who tell us that eczema may originate in erythema, papules, pustules, and even fissures, and, in fact, deny altogether its claim to be considered essentially a vesicular disease." We shall be able, by the aid of these two quotations, to ascertain our exact position in regard to the most common of all diseases of the surface, and therefore the more worthy of attention—eczema.

Firstly, it is important to note two currents, running side by side, almost unobservable, except where the commingling of the two has produced slight indistinctness: they are (*a*) the present tendency to alter nomenclature, and (*b*) the different interpretation as to the nature of the disease itself. The result has been to lead modern observers to assert that eczema includes impetigo, lichen, pityriasis, psoriasis, and some affirm, prurigo; the terms pityriasis and psoriasis being used differently by different persons. Some, as Hardy, use these terms in their old sense; others, for example, Erasmus Wilson and Hebra, to designate the scaly stages of eczema, the word alphos being substituted to replace the old application of the words psoriasis and lepra. This confusion, and the special rendering of these terms, we must bear in mind, when comparing together the teaching of our own and that of other countries.

These prefatory remarks may introduce the question—Is there such a disease as that described by Willan under the term eczema, in which vesiculation (of special kind) is the characteristic feature? Subsidiary to the determination of this point are considerations as to the desirability of using or altering this or that name, and the relations which may exist between eczema, whatever it be, and other diseases. These points will now be severally examined.

In reference to the first query, we notice, first of all, that recent writers do not clearly define their meaning. Willan is perfectly intelligible; his eczema was, as we know, a vesicular disease essentially, divided into three species: E. simplex, E. rubrum (inflammatory), and E. impetiginoides, besides local varieties. What do authorities, at the present time, say, and how far have they travelled away from Willan? In France, Hardy's views prevail. According to this gentleman, eczema depends upon a special and peculiar diathesis (the dartrous), and is one of a class to which the unmeaning term dartres is applied, and of which Willan's eczema, pityriasis, lichen, and psoriasis (Willan) are but different members. This class is characterized as presenting different elementary lesions, being non-contagious, hereditary in tendency, apt to recur, accompanied by itching, having a chronic course, and leaving no cicatrix behind. The subjects affected are persons of fair health, with a dry, harsh, surface, which perspires with difficulty and scantily. The eruptions once developed, have great tendency to spread rapidly over the surface, to appear in many spots at the same time, and to be symmetrical. In the dartrous subject, the mucous surfaces are often inflamed. Eczema is supposed to occur in the lymphatic, pityriasis in the bilious, lichen in the nervous, and psoriasis in the sanguineous temperaments. Hardy further divides eczema into varieties according to (a) *aspect*—simplex, rubrum, fendillé, impetigo; (b) *configuration*, figuratum, nummulare, impetigo sparsa, and I. diffusum; (c) *seat*—pilaris, faciei, capitis, &c.

Hebra, as the representative of the German school, in his system of classification, names one class, exudative diseases; this is subdivided into two, diseases of acute and those of chronic course; the latter class is named pruriginous, and includes scabies, eczema, and prurigo. Now eczema is here a comprehensive term, it is subdivided into five varieties: eczema *squamosum*, answering to pityriasis rubra; *E. papulatum* (lichen); *E. vesiculosum* (Willan's E. solare); *E. madidans* (rubrum); and E. impetigo seu crustosum. Hebra's opinions (*les idées Hebraïques*) have been adopted lately by Dr. M'Call Anderson; the latter affirms that which may very naturally be expected to follow from the acceptance of Hebra's views—viz., that eczema is in no way a vesicular disease, but may primarily arise from and be fully represented by erythema, papules, vesicles, pustules, or even fissures. Dr. A. Buchanan has suggested the terms erythematous, papulous, including (lichen and prurigo), vesicular, pustular (impetigo), rimosum (fendillé), to designate Dr. Anderson's subdivisions of eczema.

Mr. Erasmus Wilson, whose opinion must really have greatest weight with us, has lately expressed himself afresh in regard to eczema. He has adopted a new *clinical* classification of diseases of the skin, the first group of which is the eczematous; the latter includes eczema (Willan), psoriasis, and pityriasis (terms used not in their old sense, but to indicate the scaly states of Willan's eczema), lichen, impetigo, scabies, and gutta rosacea (acne rosacea). "Eczema," says Mr. Wilson, "is an inflammation of the skin, accompanied with alteration of its structure and derangement of its functions; it is more *vascular*, and consequently redder than in health, its vessels being in a state of congestion; its *sensibility* is morbidly increased, sometimes taking on the character of itching, tingling, or smarting, and sometimes that of pain; it is *thickened* by infiltration of serum into its tissues, sometimes fissured and œdematous; it *exudes* a serous lymph at various times and in various quantities, sometimes in excessive abundance; its cuticle is sometimes raised into *papules*, sometimes into *vesicles*, sometimes wholly removed, and is reproduced unhealthily, so as to form muco-purulent secretion and squamæ of various size; and sometimes it is replaced by *crusts* of greater or less thickness, resulting from the desiccation of the morbid secretions. In a few words, the characteristic signs of eczema are redness, itchiness, interstitial and sometimes subcutaneous thickening, exudation, papulation, vesiculation, incrustation, and desquamation." It is brought about, according to Mr. Wilson, by assimilative, nutritive, or nervous debility. The characters above described are exhibited in various degrees by different instances; but there are certain principal forms—viz., erythematosum, papulosum, vesiculosum, ichorosum, pustulosum, squamosum; there are many other terms used, and perhaps it will be better to give a chart of Mr. Wilson's subdivision:

1. Pathological forms:
    (*a*) *Regular*, includes the forms just named.
    (*b*) *Irregular*, fissum, sclerosum, verrucosum, œdematosum, mucosum, neurosum.
2. Forms of distribution:
    Universale, figuratum, diffusum, nummulare, marginatum.
3. Local forms: capitis, faciei, &c., some seventeen in all.

We see, then, that Hardy has a class dartres including eczema, lichen, psoriasis (alphos) and pityriasis. Wilson and Hebra agree almost entirely. The former, however, does not include prurigo under the term eczematous, whilst there are slight differences in the appellations attached to the subdivisions of varieties *quoad* their aspect, and he adds gutta (or acne) rosacea: agreeing with Hebra in classing eczema, scabies, impetigo, lichen, and pityriasis together.

Now the local forms, the forms of distribution, and the irregular pathological forms, of Wilson, may be dismissed from our notice, because, whatever be the nature of eczema, they will remain the same, or be described by other terms suggested by the fancy of the observer. We must only remember that these names describe stages or phases only, of the affection, and are used for convenience, and not as descriptive of the nature of the disease. Not so is it, however, with what Mr. Wilson has termed principal forms—viz., the erythematous, papulous, vesiculous, ichorous, squamous, and pustular varieties, because these include diseases which have been generally regarded as distinct. True eczema may be found in all the phases which the above terms imply; it may be represented by an erythema, a quasi-papulation, a squamation, an ichoration, but the erythema, the squamæ, the pustules, the ichor, &c. in eczema are special in kind, mostly the results of vesiculation (abortive or not), the tendency being to form vesicles, which latter form the characteristic lesion. The elementary lesions of different diseases are not convertible; the papule, for example, of lichen can never become the vesicle, or even the papule, of eczema. This doctrine of the convertibility of elementary lesions is at the bottom of the whole matter, and involves, besides the truth of Willan's system, more than at first sight appears. Now Willan's system is but an improvement upon the arrangements adopted by the ancients, and it is excessively unlikely that a form of system which has stood its ground, and which on the face of things is so manifestly true to life, can be given up easily—nay, it is the only scheme which is in harmony with others, which have formed the foundation of systems in the various collateral branches of knowledge. This question must be looked upon from an enlarged point of view. The best understood class of diseases, in which there is textural alteration of the surface, gives a large degree of opposition to any attempt to overthrow Willan's principle of classification—viz., the acute specific diseases. A poison finds its way to the blood, and shows its action most decidedly upon the skin; we diagnose, and even treat by the naked eye characters. If there be a point in their history which we are most certain about, it is the fact that each has its special elementary lesion. A plausible argument has been derived from the case of small-pox, where papules, pustules, and vesicles are followed the one by the other, in proof of the identity of what we have regarded as separate affections, but is wholly valueless. In urticaria, pemphigus, herpes, prurigo, ecthyma, purpura, erythema nodosum, are not the lesions special and characteristic? If so, why is eczema an exception? We should bear in mind that it is the commonest, the most prone of all to complication, and to be modified by accidental circumstances during its course. In general medicine we are not without instances which warn us to be very careful in our estimation of eczema. "Surely, if pneumonia is recognised as a distinct disease, and described as such in all our text-books, though frequently complicated with other inflammations, and often gravely modified in its symptoms and character, we might be permitted to assert the existence of a typical eczema in the same way that we acknowledge a typical pneumonia; nay, of the two, we assert that in practice we meet far oftener with what approaches to our ideas of eczema, as described by Willan, than we do at present observe that rare disease, acute sthenic pneumonia."[1] Now if we admit that, as a rule, an erythema may become a papule, vesicle, a pustule, or a fissure, *without drawing very close distinctions between different erythemata, papulæ, &c.*, we overthrow the whole of Willan's arrangement, and by a *reductio ad absurdum* disavow the distinction between a pemphigus, a herpes, an ecthyma, a lichen, a psoriasis, &c.; indeed, there is then no assignable limit to be placed upon the relations of any disease. In scrutinizing the nature of eczema we have failed to estimate its history as a whole, and mostly concentrated our attention upon some part, of greater moment and prominence than others, and thence drawn conclusions. Assuming for the moment that each disease has its peculiar lesion, has the latter (or evidence of it) been carefully looked for in eczema? has it not been mostly overlooked? Vesiculation occurs at the earliest period—at a period of disease which clinical reports tell us does not often come under the cognizance of the physician. In recent instances, and in those of later date, along the edge of extension you will assuredly see vesicles; they are especially delicate, soon rupture, giving rise to little superficial ulcers, which exude a peculiar secretion. Significant indeed, touching this point, is the fact that of 859 cases of eczema (made up of 561 of Devergie's and 298 of Wilson's) the duration of the malady before coming under treatment was—

Less than a month	48
Between 1 and 6 months	223
„ 6 and 12 „	81
„ 1 and 10 years	404
More than 10 years	103
	859

What have we here? Of 859 cases, in all but forty-eight cases the disease was not seen by the reporter until it was more than a month, and in the majority of cases a year old. What wonder then that vesicles were not seen? and by no means could their absence be a proof of prior non-existence. When the vesicles burst they leave behind a red, raw, oozy or scaly surface, and it is only at the edge of extension that in old cases we should expect to find them, and not many old cases do extend in their chronic

[1] Dublin Medical Press.

state. Things get to a certain pitch and remain *in statu quo*, as regards extent of surface involved, though perhaps not the aspect of the patch itself.

It is astonishing what weight plausibility holds in argument in recent times, and how little inclination there is to work away at hard facts patiently and quietly. We are discussing and concluding with a profusion that surprises one's cool review of things, the nature of the phases of eczema, giving a flat denial to certain occurrences in regard to its early history, which, *à priori*, and by analogy, from watching the course of other diseases, we should be led to expect. We debate largely about clinical teaching, when, in fact, we are utterly ignorant and have no opportunity in England, as Hebra pointedly observed to the writer, of watching the natural course of cutaneous maladies. For, without almost an exception, our schools offer no opportunity whatever for anything of the kind; the very men who teach the specialty of dermatology are specialists of other bent. Further, one constantly hears the remark, even from leaders, that little else is to be learnt about skin diseases, and that it is of little moment whether eczema be or be not vesicular. We know, I believe, little of the true pathology of skin diseases, but the time will come when the connexion between certain lesions of the surface and certain mal-conditions of the nutritive functions will be made out more certainly, perhaps, than as regards other morbid states, and that data thence derived will be esteemed most valuable and of greatest use in unravelling difficult problems connected with the existence of diseases of internal parts.

So far I have merely attempted to show that no very trustworthy conclusion can be drawn as regards the non-existence of a vesicular stage from actual observation, the first demand on clinical research, but that it is very likely that it frequently escapes notice. Until we can get the patients under our immediate eye, we cannot settle the point. In almost all those cases in which the early stage of eczema has fallen under my own observation, vesiculation has certainly been more or less distinctly marked. So much for the vesicle in relation to clinical observation. We next ask, Is eczema simple inflammation, or dependent upon a special blood state? Mr. Wilson, I presume, holds to the latter view, though, if reference be made to the description before given, this does not appear in explicit language. Hardy is the one who contends most thoroughly for the specialty of the disease, in so far as he holds the secretion of eczema to be special in character and behaviour. The query I have put is merely another way of expressing what has also been before cursorily alluded to, whether the erythema, the papulation, the vesiculation, the pustulation, is special in kind or identical with like states of other diseases.

Now what are the arguments that have been adduced to establish common relationship between Willan's eczema and other diseases, thereby breaking down the separateness and distinctness of elementary lesions? They are of three kinds: (*a*) analogical; (*b*) experimental; and (*c*) those derived from co-existences and developmental changes.

Analogical.—The leading authorities, Wilson, Hebra, and Hardy, state that in scabies, papules, pustules, and vesicles are to be observed as the result of one and the same irritation, or in a given family certain members may be differently affected, one exhibiting papules, another vesicles, another pustules. True, things are so to be found; but in scabies there is a local irritant at work which may excite any eruption according to the state of the patient; the acarus may produce eczema, would you say then eczema is scabies? The vesicle which is characteristic of scabies differs from all others of its kind. The vesicle of eczema is the expression of the action of a certain *materies morbi*, to use a convenient exponent, and has no analogy, *quoad* real causation, with that of scabies produced by a local irritant; nor, indeed, in regard to distribution, behaviour, or contents. The effect of the sun's rays produces now an erythema, now a lichen, now a vesicular disease; this is adduced as an argument also, but does not prove that these are the same. The eruption of pellagra is brought out by the sun's rays; yet it is still the tell-tale of a peculiar state of blood and tissue action.

Experimental.—Hebra draws a like conclusion from the use of croton oil to the skin, for here we see erythema, papulation, pustulation result the one after the other; but it does not prove that in eczema these are identical with the like states in other disease. Croton oil would in eczematous subjects bring out eczema. Would we think the croton-oil friction the *vera causa* of eczema? Certainly not.

Co-existences and Developmental Changes.—From co-existences which we frequently see, surely one is not justified in asserting identity; if so, then is scabies pruriginosa an evidence that scabies and prurigo are the same, and the like may be affirmed of strophulus and prurigo, lichen and urticaria, purpura and urticaria, in so far that strophulus pruriginosus, lichen urticatus, and purpura urticans are existent. As regards developmental changes, if it could be proved that all papules, vesicles, pustules, and erythemas were but stages of one common form of inflammation, then the new views upon eczema and much more must be at once agreed to; and in such case I do not see how we are to avoid a gross absurdity; and in regard to all inflammatory forms of disease (all except those which are structural heterogeneous alterations), the whole Willanean system must fall; and where is the distinction between the squamous, bullous, and tubercular diseases even, because the conclusions drawn from the arguments already noticed tend to prove the identity of elementary lesions? What could be the difference between impetigo and ecthyma or the scaly stage of eczema and alphos? You are told at once the pathologies are different; true, but apply this principle fairly. For example, is the pathology of the papule of lichen, the quasi-papule of eczema, and the papule of scabies, the same? Have observers carefully gone into the relations and tendencies of the apparently similar lesions? Has the microscope played its part, the qualities of secretions been ascertained, the state of the circulation, and the minute seat of disease been studied? Why, authorities are not agreed yet as to the seat of these lesions; some say papules are erected follicles, others upraisings of the cuticle by lymph, others enlarged nervous or vascular papillæ. It is true that apparently a small erythematous spot may become a papulous, a pustulous, or a vesiculous elevation; but is there but one of each of these kinds of lesions? This is the point. But we need ask first of all, are we sure we do not mistake through superficial examination? Take the so-called papule of scabies (not occurring in a lichenous subject, of course); in many we shall find that it has no title to the name papule—it is an early state of vesicle, and not a new solid formation; the pustule of scabies, again, is sero-purulent, not primarily purulent. Now, let the case of small-pox illustrate our main point. Well, at first sight, it appears to prove that elementary lesions are not characteristic of this or that disease, for you see the papules become pustules, and so on. The erythema, the papulation, the pustulation of small-pox are potentially and in the total disease wholly different from similar states in ordinary erythema, lichen, or impetigo. Is there not a quality of action and of product essentially peculiar in these cases? If these lesions are the same, what is to hinder us from affirming that variola and eczema are the same? But if it be answered that they are produced by different causes, why should you allow the case of small-pox to establish the *identity* of eczema and lichen, or any other two or more diseases where *similarities* are perceptible? We have only to look at the group erythemata to see our error. The erythemata of rubeola, of scarlatina, of strophulus, of erythema nodosum, in contrast, afford most capital illustration of the varying aspect of the same kind of lesion. In like manner, though we may not recognise the difference clearly at the present moment, the papules of lichen, of eczema, of scabies, of prurigo, are, in their properties and tendencies, distinct. The vesicles of herpes, of eczema, of miliaria and sudamina, the pustules of impetigo, and of ecthyma, severally distinct. The scales of a pityriasis, an ichthyosis, a psoriasis (alphos), are not identical. When we assert that an erythema may give rise to a papule, a vesicle, a pustule, and this is quite true, we cannot at once hold the identity of all erythemata, papulations, &c.

A little care will set us right here. We see clearly that there are certain diseases of the skin in which inflammation plays a part, but all have different causations and tendencies; congestion, papulation, vesiculation, &c., are but modes of expression of inflammation. Now, certain diseases may require the aid of all these states, or only some of them; and thus a disease in reaching its full development, follows as closely as possible a mode or type of action appointed by nature, and passes through certain phases, any of which may represent in general outline the fully-developed condition of other diseases. This is one of Nature's very best pronounced laws, whether seen pathologically in the human body, or more especially in the developmental stages of early life. It by no means implies identity in nature—only in the *modus operandi.* Surely in the attempt to undervalue the significance of elementary lesions, we lose sight of a very beautiful and grand law, in which Nature plays the part of a great economist; that when diseases of inflammatory kind, though they be many, attack the skin, yet Nature carries on her curative operations according to one type, which is perfect—*quoad* the result to be obtained and desired; that she modifies this plan of action, however, to suit the differences in the morbific agencies which are antagonizing healthy action, and that in the modified results, though there must be a general resemblance, yet there are peculiarities which are characteristic; or, to put it in another way, inflammation as a modified state of nutrition, comprises congestion, papulation, vesiculation, pustulation, exudation, crusting, &c.; this collectively is the *modus operandi* of cure. Well, different morbid agencies, as small-pox poison, medicinal agencies, shell-fish, the eczematous crasis, &c.—if they produce any visible effects, produce it in and through the mechanism of such inflammation; but inasmuch as the latter is not a mechanical but a vital process, it is modified in each instance. In addition, the greater must include the lesser expressions of this inflammation; hence a disease, in becoming vesicular, necessarily possesses more or less congestion; or in becoming pustular, tends to pass through papulation and vesiculation, the type being recognisable in each instance. And this is only what one would expect from the analogy of the nutritive function in general. Of course nothing is more likely than our imagining a close relationship between the several stages of different diseases, if we lose sight of the above law or type of action. What makes the lesion characteristic is its actual and potential qualities; the erythemata of small-pox, of eczema, of E. nodosum, of pellagra, of rubeola, &c., are different in tendency, quality, and behaviour, and so we believe are the papules, vesicles, and pustules severally of other diseases; their several characters have not been observed to interchange, for the law of limitation is illustrated by them.

I do not believe, then, that the doctrine of the identity of elementary lesions can hold its ground. The fact that Willan classed small-pox and impetigo, ringworm and scabies together, is no proof against the correctness of the principle upon which he classified. Now thus much is certain, that eczema commences as an erythema, and may pass through a state of papulation, vesiculation, pustulation, ichoration or squamation, but it has been further argued that any of the different states may be the starting-point of the disease. This is hardly consistent with clinical observation. Any one of the early phases may be rapidly produced and pass away, and the chronic aspects of the disease reached; for example, the commencing patch of eczema may be congested, vesiculation may fail, secretion may even be less free than usual, and incrustation or fissuring result as an early feature, but surely this gives us no right to affirm that eczema in this case has no *tendency* to produce vesicles, and that it *begins* by crusting or a fissure. In it, crusting or fissuring is the *prevailing* feature. Any of the stages of inflammation may so prevail as to give the idea of its being primary. Eczema always tries to work itself out in accordance with the law or type of action I have attempted to elucidate. The vesicle, however, is the thing aimed at; it is special in character; in other words, the great sign of eczema is the exudation of a peculiar serosity (the result of inflammation set up by a special crasis), which will, if not hindered by some active cause, elevate the cuticle into vesicles which are aggregated, small, delicate, and easily ruptured.

Willan's system must be regarded to a certain extent as imperfect; it is the flower only in bud, and though it may be desirable to alter and amend somewhat, yet as the basis of any new system must be recognised the rough outline which Willan drew. We have removed small-pox from the pustular group, and classed it with acute specific diseases—scabies, from the same category, and ranked it with animal parasitic diseases, and the time may come when we shall most likely rank eczema not under the head of vesiculæ but eczematous or dartrous diseases. Still, in any definition we give, the vesicle will occupy a prominent part, in the same way that the pustule does in small-pox, or the acarus in scabies.

Now, are the above ideas borne out by all we know of disease in general? Does not disease elsewhere follow a certain type of action, and yet present modifications innumerable, according to varying complications, habits, age, sex, climate? Take meningitis. Is idiopathic uncomplicated meningitis a common thing? Is typical pneumonia or pericarditis common? Are they not strangely modified? Then why not argue in regard to these in the same way as is done with eczema and its vesicles? Partial examination has been active in the matter of eczema. Take any two resemblers again, typhus and typhoïd. How many characters they have in common; they are even held to be identical by many, but the distinction is very apparent if we take into comparison the entire history of either disease. In botany the same thing is apparent in every group we make, or whose existence we maintain.

If we shift our position, then, from the discussion of elementary lesions, and compare together the four diseases most commonly said to make up eczema, we find their general outlines very different. Has the thin, light, delicate, irritable skin, with the aggregated vesicular exuding surface of an eczema, any likeness at all to the harsh, dry, semi-thickened, dirty surface with the disseminated character of the lesion of lichen, or the hypertrophic state of cuticle in the apparently healthy-looking psoriatic patient?

Again: we have no proof that one can assume the characters of another disease, a lichen become an eczema, &c. Intermixture certainly may occur. Lastly, we have no proof that a person affected by one may transmit another form of the diseases which are supposed to be modified forms of eczema. Hardy's class of dartres has been criticized by the foregoing remarks, and if the latter be true, then the former must fall as an unacceptable hypothesis. It is highly important that some discussion should take place as regards this point, because recent writers in this age of book manufacture have inundated us with foreign views which are contrary to the English habit and constitution.

For the purpose of illustration in regard to the matter under discussion, I may refer to a paper which appeared in the 'Lancet,' a little time ago, from the pen of Dr. Habershon, the opening remarks of which were directed against the system of Willan, and in favour of the truth of the dartrous diathesis. "Whatever division," says Dr. Habershon, "may be made in these maladies it is more important to bear in mind their character, whether having a local or a general origin, and to consider how far what is purely local is thus modified, than to decide whether the affection is lichenous, vesicular, scaly, pustular, or tuberculous. I do hope to have shown with regard to diseases of the surface, that it is of the first importance to determine their elementary lesions; further, it is to be observed, that in the future our duty as regards the latter is to determine their special properties and qualities, specially those of the vesicle of eczema, and in addition the state of general nutrition upon which the latter disease depends. At any rate, whatever we do, it behoves us not to take a narrow view of matters; we must look at the disease in its totality, and not concentrate our attention upon the lesion or the general state. There are a great many who regard these details of eczema as of little value, and merely ask for the general outline of the case, for therapeutical purposes, but as we advance in knowledge so will each distinction, however little, advance in importance our therapeute. At present, empiricism reigns dominant. Guides we have: debility, for instance, or such as tends thereto, is often an association, and points the road in which we should travel. Till we arrive at the intimate nature of eczema we shall never treat our cases *fairly*, not to say most successfully, and the meaning of the 'vesicle' is one of the most important preliminary problems we have to settle, hence the reason why I lay so much stress upon it. If it be (in the sense I have taken it) significant, then is the treatment of eczema special in kind; if it be not significant, then is the treatment the same in kind as that of a psoriasis, a herpes, a pemphigus, or a lichen."

The tendency of the present time seems to be this: by the adoption of general measures, on the one hand to pull down any excess of action, and on the other hand to restore the powers when exhausted; this is the result of two different kinds of doctrinal policy, which has each had its day. Specifics are gradually losing somewhat their old, and acquiring a new character. Arsenic, to wit, is now looked upon as a general nerve toner; at all events this is judicious, and much good will arise from the caution displayed. The elaboration of preventive measures, the judicious exhibition of general remedies to control the systemic actions generally, and the better definition and appreciation of specifics, must await for their fuller development a better knowledge of the causes of disease, and much that will help us on our way may be gathered from a careful criticism of the manifold ideas which have been held in regard to causation. It is necessary that very plain words should be used here, because the use of philosophic language and scientific dogmata have so surrounded the little we do know with an air of great importance, that, at first sight of the matter, we seem to have arrived at ultimate laws in some instances. Touching eczema, now, we venture to assert that no one definite point has been made out fairly in regard to the *vera causa*, admitting, of course, that it is *per se* an essentially distinct disease. Debility, we affirm, is present in perhaps ninety-nine of one hundred cases; but debility helps out a thousand ills of all sorts and kinds. We want something more definite than this, which is a predisponent only—something that shall be felt to furnish knowledge as precise, *quoad* causation, as that of such poisons as small-pox, rubeola, scarlatina, or typhoïd. Causation, I am aware, is a multiple quantum; many things combined together contribute to the production of such and such an end. At the same time, too much attention is paid to the accidental minors, to the neglect of the *vera causa,* the efficient cause, that state of thing or things out of which immediately and from which alone the result comes. It is so with eczema; a host of things have been called causes, derangements of the most opposite kind, errors of diet, cold, heat, moisture, mal-kidney action, uterine disorders, local irritation, mental upset, &c. &c.; these, at different times, and under different circumstances, produce very varied results. Take the action of the sun upon the surface; it is said to be the *cause* of eczema solare, but it also produces erythema, lichen, pityriasis, pellagra. All depends upon the state of the patient's health. Remembering, then, not to lay too much stress upon these excitants and predisponents, and it were better not to use to them the word causes; there still remains, over and above these, a something upon which the disease essentially depends. What this may be, hard work in the wards of our hospitals, chemical, microscopical, and pathological researches, expended over a long time, and a large number of cases, will alone solve to us. True it is, that a knowledge of excitants and predisponents enables us to invent a tolerably successful plan of treatment. "To treat eczema judiciously and correctly we must know," says Mr. Wilson, "the sex and age of the patient, together with the predisposing cause, the remote predisposing cause, and the exciting cause of the disease. It may be that our treatment must be directed to the restoration of digestion and secretion, or to the strengthening of an exhausted vitality, or to combating an inherent or inherited weakness, or to the improvement and sustainment of defective powers, or to the regulation of disorders of the female system, or to the cure of visceral or organic disease. In a word, the highest and best qualities of the medical art and science must be put in practice with foresight and discretion for the treatment of an eczema; the universal must submit to become the handmaid of the special." This means, that given a case of eczema, and you find anything wrong in the patient, treat and rectify it; advice sound, practical, successful. But my point is this, that it is not every derangement of secretion, of digestion, of exhaustion of vitality, or weakness, or defective power, &c., that has connexion with eczema—nay, it is something very special in its kind; further—and this is important—the very defects which we regard as "causes" (predisposing generally), are themselves not unfrequently the result of the eczematous crasis, not necessarily, certainly. We succeeded in our therapeutics because, fortunately, the deviations from the point of health which exist, being corrected, Nature is enabled to work her own way unchecked, and general tonics aid her; but we should do much better if we knew a little more of the different states of system upon which the excitants play. A person catches cold, or gets over-heated; a bronchitis, an eczema, a herpes, something renal, venal, or laryngeal may result. Another, committing an error of diet, is attacked by diarrhœa, eczema, urticaria, or other malady. The state of system upon which the heat, the cold, or the dietetic error acts is the thing we want to know, and it is what we often lose sight of. What is it in the eczematous subject? Not knowing it, we are by no means sure that we do not sometimes hinder rather than help the cure. Lastly, as regards local remedies, ideas are not clear. We are not as yet agreed as to the natural course of the disease, and cannot, therefore, have watched Nature's procedures in such a way as to be able to imitate her in conducting our therapeute. As far as we are able to judge, there are three stages: *inflammatory*, in which we should exclude the action of the air, and employ the most soothing remedies; an *exudative*, in which slight astringents, and a *scaly* (or chronic) stage, in which stimulants, are valuable. This plan is by no means taught nor acknowledged as correct.

My chief object, and I selected eczema for illustration, has been to show—

1. That there exists a law in regard to the inflammatory diseases of the skin, in accordance with which eruptions tend to assume a certain likeness to each other in many instances. Nature carries on her cures after one type of action, which we call inflammation; the latter, in its perfect state, includes congestion, papulation, vesiculation, exudation, pustulation, and crustation. The occurrence of the greater includes that of the lesser degrees of expression in more or less perfection: the nature of a disease modifies this plan of action, and produces results which, while the same type or *modus operandi* may be recognised in them, are in each case always distinct and characteristic.

2. That the tendency in eczema is to produce vesiculation; it is special in regard to the aggregation, the situation, the delicacy, and the nature of the contents of the vesicles. The disease may be abortive, and merely reach the erythematous stage, or be papuloïd, or become sero-purulent, exudative, crusted, or fissured, according to circumstances.

3. That the elementary lesions of different diseases are not convertible.

4. That eczema is dependent upon a state of nutrition *sui*

generis, and its definition includes this as much as the character of the lesion and secretion.

Lastly, the word Willanist has been coined and used by recent writers almost as a term of derision. In want of a better, I will stand, feebly enough it may be, as the champion of Willan, confessing my admiration of him and the claim he has upon our esteem. His system contained the germ of that law which I have here briefly propounded and illustrated, which is opposed to the teaching of continental authorities, who have failed to appreciate it, and which I put forward for acceptance as the true foundation of a British School of Cutaneous Medicine.

ART. II.

Essay on the Medicine of the Greeks. By T. CLIFFORD ALLBUTT, B.A., M.B. Cantab., Physician to the Leeds Infirmary, &c., &c.

Εἰσι δὲ οὗτοι οἱ οὐδὲν ἄλλο οἰόμενοι εἶναι ἢ οὗ ἂν δύνωνται ἀπρὶξ τοῖν χεροῖν λαβεσθαι, πράξεις τε καὶ γενέσεις καὶ πᾶν τὸ ἀόρατον οὐκ ἀποδεχόμενοι ὡς ἐν οὐσίας μέρει.

Καὶ μὲν δὴ, Ὦ Σώκρατες, σκληρούς γε λέγεις καὶ ἀντιτύπους ἀνθρώπους.—*Theætetus*, p. 155. Ed. Steph.

THOSE persons who study antiquities, or the relics of men of former times, may be divided into two classes. The first class consists of mere pickers of curiosities, and these are led to that pursuit for the sake only of a certain agreeable sensation which is produced in us by the sight of that which is new or strange. This sort of mental titillation is much affected by persons whose lives are otherwise monotonous or tedious; when the sudden acquisition of a rare or curious object relieves them from that pressure, and produces a gentle and harmless kind of excitement. It is thus that many town ladies take pleasure in collecting curious china and bricabrac, as others will seek for strange birds or dogs; while country gentlemen, also for want of a better ambition, will come little short of them in busying themselves about uncouth trifles. There is, on the other hand, a class of persons who study antiquities for the gratification of a fine taste, or for the better understanding of the history of the human race, both of which purposes find an ample justification in the advancement of liberal culture. To become familiar with the thoughts and works of men who lived under conditions which widely differed from our own, is a most wholesome exercise; and the absence of such familiarity, and of that kind of imaginative sympathy which grows upon it, often results in narrowness and intolerance. The study of the so-called classical literature has singular advantages in dispelling self-complacency, in enlarging the mind, and in giving generosity and elevation to the feelings. As we read the works of that literature we gradually familiarize ourselves with the acts and the speculations of two principal nations which flourished in times long past, and the richness and variety of which were developed under conditions of climate and society which are well contrasted with those of more northern nations. Thence, beyond the genius with which we are daily infected, we receive into our minds new orders of thought, and are led to admire other conceptions than our own. For those, moreover, who take pleasure in tracing the growth and travail of the human mind, there is found in a survey of the past, a large picture, as it were, of that development which also finds its place in the lives of individuals; and the one may, after some sort, find its illustration in the other.

We learn, then, as it appears, both directly and indirectly, from the ancients. We find in them an infinite nobility of sentiment, a keen and delicate wit, and a certain grandeur of passion, which are of immediate necessity for us, who often lack these qualities. We may use them, also, for those indirect results which generally derive from experience and comparison; so that, by studying the characters of the ancient societies, we may have the better understanding of all.

It would seem, therefore, as scientific men employ their thoughts, let us say, upon Greek literature, that they do so rather for the sake of a study of the laws of mental advance than for the sake of adding from those records to their store of positive knowledge. As Mr. Lewes has well pointed out,[1] the Greeks did never attain to that method of verification which alone can assure the success of science properly so called. As soon as this method was attained, men became less willing to submit to the words of a master, and sought rather to test every statement by direct investigation. We do not wonder that those who desire especially to extend the limits of exact science should sometimes forget to care for those historical details which can but little affect the validity of their results. In proclaiming the insufficiency of authority in matters of opinion and knowledge we have, perhaps, erred, on the other hand, in withholding a kindly and reverent interest from those who have earnestly sought after what we are attaining. We acknowledge, then, with much satisfaction an increasing desire to search the traditions of the ancients, not to an end of establishing them as authors in disputation, nor to our own vain-glory, but with the modest and reverent wish to build them a genuine renown on the ruins of falsehood or in the wastes of oblivion.

[1] See his Essay on Aristotle.

And, indeed, in thus speaking of the exacter sciences, we would show how much more the same historical study is due to those who have exercised their faculties upon questions which less admit of positive solution. In medicine, as in the affairs of social and political life, while we act upon a foundation of exact knowledge, and are ever engaged in widening and in raising up that sub-structure, we are, nevertheless, constantly called upon to act in great uncertainty, and to determine our course upon a balance of probabilities rather than upon clear manifestations and proofs. As Dr. Butler has said in his Introduction,[1] we are often laid in matters of practice under an absolute and formal obligation, in point of prudence and of interest, to act upon a presumption or low probability, though it be so low as to leave the mind in very great doubt which is the truth. Seeing that the practice of medicine does surely depend as much on a certain sagacity and fine temper of mind as upon that most necessary furniture of it which is gained by the accumulation of exact knowledge, so far as that be attainable; it would then appear that much wisdom, of direct and daily utility to ourselves, may be learned from the works of those ancients who were distinguished for those faculties. No people were more remarkable for a flexibility and openness of intelligence than were the Greeks. Singularly free from any tendency to cling to routine, which blinds the common reason, and from any mental indolence, which prevents the discipline of philosophy, so we find in their writings the evidence of a vivid and active thought and of a delicate and sensitive perception. It cannot, therefore, be ill worth our while to consider shortly some part of so large an inquiry, that by making a sketch of the states of medical science and art during the time of the Greeks we may serve a present necessity, and excite a fuller research.

Greek medicine has found a most able expositor in M. Littré, the editor of an excellent edition of the Hippocratic writings; and to his work every subsequent writer must be deeply indebted. The historian of medicine, from the earliest times down to the present day, is, however, yet to be sought—a historian who can bring learning enough to the task, and also a mind trained upon the doctrines of positive science.[2] Learning alone may bring forth a most excellent book, but to this must be added a precision of scientific thought, a candour and freedom of judgment, and a familiarity with genuine results of modern research in criticism and in science, if this excellent book is to

[1] Analogy of Religion to the Constitution and Course of Nature.

[2] The second edition of Hæser's history (8vo., sma., 1853) is highly esteemed by competent authorities, as by Dr. Ravel of Montpellier. It must necessarily, however, be very brief. Dr. Greenhill tells me that it contains most useful bibliographical indexes. May we hope that Dr. Daremberg will give us a full history of medicine, and satisfy our desire not only for excellence but also for completeness?

be also an excellent history of medicine. Sprengel's history, the best we possess, wants much of the spirit of modern scholarship and of modern scientific results. Admirable for the time at which it was written, it now seems deficient, when compared with the achievements of subsequent years.

At the present time, however, all that we can propose is to represent that kind and that amount of knowledge of the history of Greek medicine which should take a familiar place in the minds of most. In this survey, which should begin from the earliest days of Greek civilization, and end at the rise of the schools of Alexandria, we obtain most valuable assistance from the volumes of M. Littré. Up to the time of M. Littré's work the standard edition of the Hippocratic writings was that of Foesius, published in the seventeenth century. Admirable as that edition is, M. Littré has dealt with a far wider field of inquiry, the work of Foesius being almost purely one of critical scholarship. The lexicon entitled the 'Œconomia Hippocratica,' which we owe also to the labours of Foesius, is still a work of surpassing value, and leaves, perhaps, little room for improvement by modern scholars.[1] Without it this article would have been almost an impossibility. The learned and most useful treatise of Dr. Ermerins on the 'Prognostics,' that of Dr. Coray on the 'De Aere Locis et Aquis,' and M. Houdart's 'Etudes,'[2] have been also of much service.[3] By far the most important contributions, however, to the right understanding of Hippocrates are the various *excursus* which are inserted by M. Littré in his indispensable edition. Their only fault, as it appears to us, is their sad prolixity, which may come of the fatal facility, the *mortifera facundia,* of the French tongue. The general introduction consists of thirteen chapters, comprising little short of six hundred octavo pages; and the several arguments which preface each treatise, and likewise the notes, are conceived on a similar scale. Happily, that divine gift of skipping is not denied to suffering mortals; but this faculty finds occasion enough to rejoice in its activity without seeking them in M. Littré's 'Hippocrates.' The introduction, in its several chapters, treats of the following subjects:—the early History of Medical Science; the Life of Hippocrates; an Account of the Hippo-

[1] Dr. Greenhill has begun in this Review a series of "Adversaria Medico-Philologica," which promises to be of great value, and covers a larger ground than the lexicon of Foesius.

[2] Etudes Historiques et Critiques sur la Vie et la Doctrine d'Hippocrate. Paris, 1836. In 8vo.

[3] Dr. Ermerins has now completed and published a magnificent edition of the Hippocratic treatises, of which more hereafter. His treatise on the Prognostics was published at Leyden, in 1836, in 4to. In the above slight reference to some of the more handy treatises on Greek medicine, I have omitted to mention the indispensable "Œuvres Choisies d'Hippocrate" of Dr. Daremberg. 2nd Ed. Paris, 1855.

cratic Writings; Contemporary Reference to these; their Preservation, and the Commentaries written upon them; the Value of Ancient Criticism; of Modern Criticism; a Discussion on certain Points of Medical Chronology and History, as bearing on the Authenticity of the various Treatises; the Internal Evidence deriving from these, and their Relations to each other; their Collection and Publication; their individual Contents; and, lastly, a very able analysis of the Character and Works of Hippocrates himself. He adds some most useful information on the dialect of Hippocrates, and on the various manuscript texts and printed editions of the collection in ancient and in modern times. We may say, however, that M. Littré's classical scholarship is not among the strongest of his powers as an editor.[1]

From the above-mentioned and some original authorities we shall put together our short account of Greek medicine down to the time of the school of Hippocrates; and then, taking this school as expressive of all that was best in the medicine of the age, we shall choose what is best again in its records for analysis and description. We hope by these means to present a fairly accurate conception of the state of medicine at its dawn and during the time of its midday—the time when Hippocrates, Socrates, Aristotle, Plato, Phidias, Thucydides, Pericles, were among the leaders of that wonderful people who dwelt on the myriad shores of the eastern Mediterranean.

We pass by any reference to the state of medicine among the Hindoos and Egyptians; for in those nations the art never found a positive foundation, but was confined to the caste and to the craft of the priests. In those nations diseases are even now believed to some extent, and formerly were always believed, to be the evil works of demons, and had therefore their appropriate remedies in magic and incantations. At the present day the Brahmins seem to possess certain sublunary medicines, but their application is veiled in too much mystery to have interest for us. Unlike Hippocrates, and like the Chinese, they have some knowledge of the use of the arterial pulse, or at least they constantly seek such knowledge by invariably feeling for it. These nations, and the Chinese also, are nevertheless wholly ignorant of anatomy, the place of which is supplied by certain fanciful pictures of the interior of man. The pulse is examined by the Chinese in various vessels and with absurd gesticulations. Their principles of treatment (which is often very severe) are too ridiculous to deserve even a cursory mention. The elaborate and even laudatory accounts of Chinese medicine, like their reputed knowledge of the circulation of the blood, will not bear any severity of criticism.

[1] In saying this, I am not intending to slight M. Littré, but to draw the attention of Greek scholars to the most important duty of studying the Ionic Greek of Hippocrates in the interests of pure literature.

There is absolutely no evidence that the knowledge of the Chinese is such as many writers represent, while on the other hand the works of those writers supply much evidence to the contrary. Judicial astrology is closely associated with all their theories, and it is easily seen that much of the credit given to the Chinese derives from the interested motives of those who describe them. The early Jesuit missionaries were deeply imbued with a trust in the excellence of theocracy, even in idolatrous peoples; and they had to explain also much ill-success among nations supposed by Europeans to be raw material to their hand. In modern days other schools have also found in glorifying this people some interest and support for peculiar doctrines. In truth the conditions of the Chinese societies were not complex, and a social equilibrium was soon reached. Of this equilibrium, such as it is, they have the advantage—an advantage which merits no emulation. The conditions of Greek society were infinitely more complex, and the results of it therefore more various. In its early stages it, of course, presented phenomena bearing resemblance to corresponding stages in other societies. As in India and Egypt,[1] medicine, like other arts, has always been in the hands of the priesthood; so in Greece and in mediæval Europe did it issue from the gates of the temple. Moreover, medicine in the early times of Greece was not yet differentiated as an art from the arts of policy, of war, and of music. David, the statesman, the warrior, and the harper, who brought peace to the frenzy of Saul, has his counterpart in the heroes of Greece and of Scandinavia. Podaleirius and Machaon, who warred before the gates of Troy, called upon Apollo when they bound up the wounds of their comrades. As a peculiar divinity has always been supposed to attach to the more mysterious operations of nature, so, as was likely, we find in all history a firm alliance between the priest and the physician, or, to speak more accurately, between the priestly and the medical functions. In early times the two, as we have said, are invariably associated; and still, in modern Europe, each is constantly striving to supplement itself with the other. Bassompierre writes, "Les médecins en desespèrent; depuis ce matin on a commencé à user des remèdes spirituels, et à faire transporter aux palais l'image de Notre Dame d'Atoche," &c.[2] Now-a-days in the country parish it is the parson's wife who keeps the medicine-chest; and even in our towns the priest is seldom content with the care of the soul. Medicine then

[1] I cannot help quoting from Herodotus the following passage descriptive of Egyptian medicine, though I dare not translate it, lest some of my friends should think it personal. He says (Bk. ii. 84) Ἡ δὲ *ἰητρικὴ κατὰ τάδεσφι δέδασται. μιῆς νούσου ἕκαστος ἰηιρός ἐστι, καὶ οὐ πλεόνων. παντα δ' ἰητρῶν ἐστὶ πλέα. ὁι μὲν γὰρ, ὀφθολμῶν ἰητρὸι κατεστέασι· ὁι δὲ, κεφαλῆς· ὁι δε, ὀδόντων· ὁι δε, τῶν κατὰ νηδύν· ὁι δὲ, τῶν ἀφανέων νούσων.*

[2] Disps. March 17, 1621.

for the early Greeks was a gift of the gods, and available only through their ministers. During the state of theocracy the priests were lawgivers, prophets, and medicine-men. Fully impressed as they were with the absolute sanctity of these functions, they exercised them with a pomp and a ritual which afterwards became the more or less conscious imposture of a failing authority. Certain priestly families, however, as we shall see hereafter, introduced into their medical art a spirit of scientific observation and a practice of physical agencies. But this was far from being the case at the outset. Galen and Pliny fall into an error, which in us would be absurd, when they attribute books on healing and on pharmacy or botany to such a caste as the Orphic priesthood. Aristophanes says (Ranæ, 1033) that Orpheus taught the mystic rites, and Musæus the "ἐξακέσεις νοσῶν,"[1] the two functions being clearly looked upon as kindred in character. The Orphic hymns (which are very ancient, whatever may be their date or origin) are probably some remains of the sort of incantations used at an early period. Incantations are means by which nervous disorders might more readily be kept in check, and such diseases, being the most awful and the most mysterious, were to a late period known as "sacred diseases." We find that the early efforts of medicine were of this kind, and were to some degree successful in such disorders. The mythical story of Melampus, a physician and "χρησμόλογος,"[2] who cured the daughters of King Prætus of mania, illustrates this statement; and such stories are numberless. It was at a somewhat late time of mythological development that medicine became distinct, even as a divine art. In early times healing seems to have been embraced and, as it were, lost in the general sense of divine favour, or of cessation of divine persecution, until it was gradually distinguished as an attribute, first of Pœan and Apollo, afterwards of other gods or heroes. Modern commentators, however (with whom, strangely enough, Sprengel agrees), are undoubtedly in error when they bring from the Orphic hymns the epithet ἰήιος as expressive of the healing power of Apollo. This word certainly belongs to the root ἴη or ἴω, as preserved in the cry Io Pæan, and has no kinship with ἰάομαι. Æschylus applies the term ἰατρομάντις to Apollo-Loxias;[3] and Pindar, in places too numerous for quotation, couples together healing and music as kindred but separate attributes.[4]

[1] The cure of diseases. [2] Soothsayer. [3] Eumen., v. 62.

[4] Take for instance the following passage from the fifth Pythian ode:—

ὃ καὶ βαρεῖαν νόσων	"Who doth for man and womankind dispense
ἀκέσματ' ἄνδρεσι καὶ	Against diseases dire his healing influence;
γυναιξὶ νέμει· πόρεν τε κίθαριν·	He who the tuneful harp hath given;
δίδωσί τε μοῖσαν οἷσ ἂν ἐθέλῃ	And unto whom he will the muse imparts,
ἀπόλεμον ἀγαγὼν	Breathing into their hearts
ἐς πραπίδας εὐνομιάν. κ.τ.λ.	The love of quiet rule and order even," &c.

Cary's *Translation.*

There can be no doubt that at the beginning of the 5th century A.C., the idea of healing as a distinct art was fully recognised, and belonged to Apollo. No beautiful theory is less true than that one of the thousand and one false doctrines of mythologers, to wit, that healing was attributed to Apollo as the Sun—the life-giver. No confusion can be less excusable, or more common, than to confound Helios (Ὑπεριονίδης ἄναξ) with the son of Latona. If a fundamental idea must be discovered, that idea is Harmony. The even-flowing function of mind and of body in rhythmic motion is, probably, the bond of thought which united medicine and music. This much is at least certain, that modern chemists cannot be allowed to convert Apollo into Helios for the ends of philosophy. Plato, in the etymological argument of 'The Cratylus' (p. 406), classifies the attributes of Apollo as the τέχνη μαντικὴ, μουσικὴ, ἰατρικὴ, τοξικὴ.[1] Artemis, probably, had little or no real connexion with medicine. At comparatively late periods she seems to have been to some degree a patroness of women in childbed; and we find her called ογοστόκος by Theocritus (Idyl. 27). The same attributes, however, belong in the 'Iliad' (xi. 270) to Eileithyia,[2] with whom Artemis was sometimes confounded, as she was also with Selene and Diana. At the time of Hippocrates, Apollo was the recognised god of healing. Euripides, in the 'Alcestis' (l. 969), represents Apollo as instructing the Asclepiadæ, his priests; and the so-called Hippocratic oath, which is of a still later date, opens with an adjuration to Apollo, the god of the physicians.[3] After the gods come the heroes to whom the principal gifts of the gods were entrusted, and whose inspirations were the direct means of benefiting mankind. The two heroes who principally concerned themselves in the practice of medicine were Chiron and Æsculapius, or Asclepius. Chiron is the first link connecting medical gods with medical men; and he was the earliest mediator through whom the divine esoterics were handed down to the jealous care of the priesthood. In Chiron, again, we find music still associated with medicine. Achilles was a medical student under the teaching of Chiron, and accordingly, in the eleventh book of the 'Iliad,' we find Patroclus performing a most creditable piece of surgery upon Eurypylus. Indeed, all early benefactors of men had credit for some power over disease—that great source of his suffering and sorrow. Hercules was in some cases honoured as a healer; and that indefinite hero, Aristæus, was certainly honoured as skilful in the art of medicine, which he was said to have received from Chiron. Of all the pupils of Chiron, however, no one as a physician turned out so well as Asclepius. This hero took his

[1] Prophesy, music, medicine, archery. [2] Vid. Pind. Ol. vi. 79.

[3] The Cyclic poets looked upon Pæan and Apollo as distinct persons; but the distinction was effaced even in the time of Pindar. Vid. Heyne in loc.

mythical descent immediately from Apollo himself; and about him, as about all heroes, we find that endless fables have gathered themselves together. Traditions wholly false, and often relating also to other personages, are found in Hesiod, Pindar, and many later writers. Homer calls Asclepius a Thessalian prince, and Hesiod makes no mention of him in the divine hierarchies; so it seems likely that his deification was a figment of later authors.[1] Pindar, indeed, speaks of him in the third Pythian Ode, as indulging in the very ungodly weakness of looking too closely to the main chance. "Ἀλλὰ κέρδει καὶ σοφία δέδεται:" so that the money-grubbing doctor was induced for dirty gain to keep alive a scoundrel whom all honest men must have wished to see decently damned. Everybody saw at once that this sort of thing would not do; and all respectable gods, infernal or supernal, felt that a stop must be put to so unheard-of an interference. "Nec satelles Orci callidum Promethea Revexit auro captus." What could be the use of keeping up Hades at great expense, if doctors were to be bribed into immortalizing their patients? Zeus, therefore, says Diodorus the Sicilian, listened to the very reasonable expostulations of Pluto, and pulverized Æsculapius with a thunderbolt, as a warning to all doctors to come—a warning which, it is said, has had all the effect that could possibly have been desired. It is quite clear, then, that before the fifth century A.C., medicine was practised with more or less regularity; and it is worth our while to inquire what that medicine was. Æsculapius is declared in many places to have founded clinical medicine. It seems, however, equally certain that therapeutics in our sense of the word were wholly unknown, and that the treatment of bodily disease did not find its simplest beginnings until the dietetic systems of the Gymnasiarchs gained some completeness.[2] Plato, in a passage abounding in exquisite humour, says that Herodicus of Selymbria, by inventing the treatment of internal disease, laid up infinite misery for himself and for sickly people in all ages to come. Æsculapius, he adds, limited his practice to potions, external applications and incisions—not, of course, that he was ignorant of the treatment of internal disease, but as seeing, in his wisdom, that the sooner sickly people died out of the way the better for themselves and for everybody else. Besides, luxury is encouraged by the hope of bodily impunity, and the unlucky physicians are thereby called upon to invent all sorts of uncouth appellations, such as catarrhs, fluxions, and the like, for affections which would not and did not otherwise dare to exist. Why, indeed, should people with no constitutions have their lives preserved to the protraction of their own miseries,

[1] M. Littré says that the worship of Æsculapius came from the East, a kind of assertion much too common in mythology. We need not add, however, to what we have said above.

[2] Republic, Dial. iii.

the annoyance of their neighbours, and the begetting of unsound progeny? We must, however, admit, with Dr. Renouard,[1] that in this "the ancient sage" was most certainly wrong. Being ourselves valetudinarian, we cannot find a word to say for him.

It seems then clear that the earlier Asclepiadæ were supposed by Plato to have practised surgery only—to have treated accidents and not diseases. This view is supported by all early testimony. Pindar in the third Pythian Ode, to which we have referred, enumerates calming drinks, external applications, and incisions as the means by which Æsculapius cured wounds and sores: he, perhaps, administered cooling drinks and other simple alleviations also in certain of the familiar *ἐπέτεια νοσήματα*.[2] He refers to Æsculapius also, in the third Nemean Ode, as practising "*τὸν φαρμάκων μαλακόχειρα νόμον*,"[3] which clearly means surgery. In the 'Iliad' we find that Podaleirius and Machaon treated wounds by cataplasms of pounded herbs and by ointments, also with cooling drinks. They drew out weapons from the flesh (Il. iv. 214), or cut down upon them (Il. xi. 829), or, if necessary, thrust out the weapons through the parts (Il. v. 112). Villoison, however, does not seem to think that the wounds of Homer's heroes were very bad, and that, like Aphrodite, they made a great noise about small matters. The efforts of Eustathius and of other scholiasts and commentators to show that Podaleirius practised medicine, and Machaon surgery, help by their ill-success to prove that no such distinction existed.[4]

Epidemic diseases caused much alarm in early times, and were supposed to be direct visitations of the gods; they were accordingly met only by incantations and other theurgic methods (see Iliad A.). A semblance, however, of rational practice we find in the tale of the wall built by Empedocles to ward off a pestilential wind, and also in the reported success of his endeavour to arrest another epidemic by the drainage of a marsh. The conclusion, however, at which we must arrive is this—that before the time of the Hippocratic school medicine had not been fixed upon any positive basis, but that the whole art up to that time consisted in simple surgical operations and in dressing of wounds, while any unforeseen difficulties were met with a sufficient quantity of "*μαλακαὶ ἐπαοιδαί*."[5] No belief whatsoever is to be given to the repeated statement that Æsculapius, or any other physician of the pre-Hippocratic period, raised medicine to

[1] Hist. de la Médecine.

[2] Epidemic seasonal diseases.

[3] The gentle-handed management of remedies; compare Iliad, 4. 191. *ἐπιθήσει φάρμαχ' ἅ κεν παύσῃσι μελαινάων ὀδυνάων.*

[4] There has been much discussion on this point—viz., on the date of the division of medicine into distinct departments. It would have been abhorrent to the intellectual Greek; and if we except those wretched Egyptians mentioned by Herodotus (v. s. p. 175 n.) it was probably unknown until the "dark" ages.

[5] Soothing incantations.

the dignity of an intellectual art. Nor is this conclusion contrary to what we should expect. The simple precedes the complex, the evident is subject to observation before the mysterious, and the art of medicine recognised its power over the former, while the latter was still considered as especially belonging to the gods. Hippocrates was the first to declare that obscure diseases are divine in that sense only in which all phenomena whatever are also divine.[1]

It is important at this point of our inquiry to ascertain the use made of the temples in the treatment of disease. The priesthood, being the depositories of divine mystery, were sought in the temples at times of doubt and distress. The temples dedicated to the gods and heroes of medicine became, therefore, hospitals whereto the sick made their way. Not that they were in any sense homes for the stricken poor, as are our modern hospitals. Charity, either as a sentiment or as a doctrine, was unknown before the spread of Christianity. It would have been a strange thing in the ears of a pagan sage to hear that "blessed is he that hearkeneth to the prayer of the poor and of the needy in his distress." To him it seemed that the sooner a being so useless to the state could manage conveniently to himself to die, the better for everybody. He would not have urged him with any indecency of haste to "clear out," but would have hinted unmistakeably that there was a risk of his becoming a nuisance. Even in the later Stoics—from whom, as from mountain peaks by the sea, was shed a faint radiance of the coming light—even in them we fail to discover the idea of the sanctity of personal life apart from political relations. Keeping a shrewd eye then to possible *ἀναθήματα*, the priests of Æsculapius encouraged the sick to resort to their temples. And, whatever be the explanation, it seems certain, from all accounts, that these temples were built in airy situations and amidst imposing landscape, near good water also, and sometimes to fountains of peculiar properties to which we may, perhaps, apply the title of mineral springs. In these high places and groves Æsculapius was celebrated with symbolic and pompous worship. The serpent was sacred to him—an animal whose subtilty has won the awe of mankind, from Eve to Moses, from the Egyptian priesthood to the fetish worshippers on the coast of Guinea. Serpent-charmers were the earlier Asclepiadæ, like the more vulgar Asclepiads in fur caps and fustian, cullers of simples, who still lurk in our lanes by the light of the moon.

Before the fifth century A.C. it would seem that the Asclepiadæ prescribed to the devotees of the temple fasts and purifications, solemn processions also moving in rhythm with the tides of a majestic chant, which from clouds of burnt offering and incense proclaimed the glory and the power of the god. Such a disci-

[1] De Aere Locis et Aquis. cap. xxii.

pline as this, calling responsive pulsations from the profundities of emotion, must have had prodigious power over certain forms of disease, and must have brought much honour and reward to the temples. Among other *κειμήλια*, casts or pictures of the parts restored, and tablets upon which the disease and the results were inscribed, were laid up for an eternal witness. Any of our readers who have visited the shrines of celebrated saints in Italy and elsewhere (such, for example, as that of St. Antony of Padua) may yet see observances almost precisely the same, fallen, indeed, from their great and ancient estate, but interesting as having been handed down by continuous tradition from the earliest days of Mediterranean civilization. In addition to liturgical influences, further treatment in many cases was practised. During the state of spiritual excitement, dreams or visions were sought and readily obtained, as they were by the mediæval ascetics, and are by our modern and more sensual "mediums." The laws of the evolution of ideas being utterly unknown, these dreams and visions were attributed to the direct interference of the god, as they are by the equally ignorant at the present day. Probably the priests had some hand in guiding the apparitions and in dictating the oracular commands. These commands were generally of a harmless kind enough. Grapes were to be laid on the wound, or the patient was to bathe in a certain fountain; sometimes, however, they were very severe, perhaps when Pluto began to get impatient, and wanted work. If death resulted, of course it was the patient's fault, who by omission of some important detail had deranged the system of renovation. We learn something of the sort of treatment used in the temples from the descriptive tablets above mentioned. Pausanias speaks of many as existing in his day, and a few have been recovered in modern times. Gruter gives transcripts of these in the "Inscriptions;" there are five of them, and they were found in the island of the Tiber.[1] It is not worth our while to repeat these, as they only show that the medical art of the later Asclepiadæ was a mere system of superstitious formalities. We will, however, translate the most practical of the five. "A blind warrior, named Valerius Aper, having consulted the oracle, was ordered to mix the blood of a white cock with honey, and to make an ointment, with which he was to rub his eyes during three days. He recovered his sight, and thanked the god before all the people." In the other tablets we only find mystic perambulations, strange gestures, certain rites of liturgy and sacrifice, and the like, as means to the restoration of health.[2]

Slowly, then, did the idea of medicine as a peculiar art unfold

[1] A temple was built to Æsculapius on this island.

[2] See also Hieron. Mercurialis De Arte Gymnast. Amstel. 1672. pp. 2, 3, as quoted in Smith's Dict. of Antiquities.

itself and become explicit, gradually separating from the more general ideas of harmonious function, and embodying itself in special impersonations of the divine with special ministries for the exercise of its power. As the field became thus narrower, and special relationships more definite, the supernatural and vague was reduced to the natural and coherent, this process beginning with those classes of phenomena which were most readily subject to repeated observation. As we have said, external wounds were first seen to arise from natural causes, and in some degree to obey a natural treatment. These, therefore, soon became detached from the diseases whose causation was less manifest, and whose issues seemed to defy calculation. That only was natural which completed the cycle of its evolution within the limits of the popular mind; that which travelled beyond these limits was not natural, and was therefore divine. The natural was the comprehensible, the incomprehensible was the supernatural, as it is to-day. That which was first declared to be natural was that which was first comprehensible; and the division of phenomena into natural and supernatural classes was not made in accordance with the incapacity of man, but with some supposed definition of the cosmical scheme. Such definitions, having no objective existence, necessarily varied as the knowledge of particular observers; and we find, as the general mind of the Greek peoples enlarged, so did the class of natural phenomena. This the priest-physicians were compelled to acknowledge, and did indeed themselves most readily discover. Like the priests of Egypt, however, they formed a caste apart, and with the awakening of the scientific spirit many rivals without their body began to spread themselves in many directions. The regimen of the Gymnasiarchs was perfected into a system of physiological medicine, and certain practitioners (ὁι περιόδευται), inheriting some remains of the philosophy of Pythagoras, began to travel from place to place, more especially in Sicily and the Italian peninsula, and to work cures. Great, of course, was the scorn which the regulars felt for the irregulars;[1] and all men of reverential and proper habit no doubt preferred death itself to a cure by a wandering quack in a caravan. In truth, a little bad Pythagoreanism was about the worst conceivable foundation for the practice of medicine. To

[1] Such at least is the impression received from reading most of the records of the time. We read, however, in Diogenes Laertius, that Eudoxus, who was educated under the patronage of the Cnidian priests, learned τὰ ἰάτρικὰ from a Sicilian, who was in all likelihood a περιοδέυτης. Indeed, the inhabitants of Crotona, which was the principal school of medicine in Italy, were Achæan colonists, and Democedes was a Crotoniat, who made so good a cure of Darius' ankle, having been called in consultation in a somewhat pressing manner. See the whole story in Herod. iii. 124–132; who adds, that Democedes became "μέγιστον πρῆμα παρὰ βασιλέϊ." Democedes, however, being in love with a prizefighter's daughter, sneaked off home again on the first opportunity.

their honour, however, the Asclepiadæ were not content to overcome by denunciation, but sought a supremacy of learning and skill. The first family who had the boldness to cast aside mysterious initiation, and to publish the results of a careful observation of disease, was the priestly order established at the temple of Cnidus. This Cnidian school published a collection called the 'Cnidian Sentences,' which was a much-esteemed authority in medicine in the early part of the fifth century. The Cnidian Sentences are now lost, but it seems from much valuable testimony that the Cnidian physicians failed in recognising any subordination of symptoms. They seem to have attached equal importance to the accidents and to the essentials of diseases, and not to have conceived anything like definite nosological processes. The priests of the temple of Cos—the family of the Hippocratidæ—soon rivalled and quickly surpassed the Cnidian priests, and to their industry and admirable method we owe the most truthful records in the whole history of medicine from the earliest up to modern times. Seeing that the whole history of man seems to condemn sacerdotal castes as the very incarnation of all that is opposed to the increase of rational knowledge, it may seem strange that a priestly family should be the first in Europe to declare the utility of the inductive method. We must not, however, measure all priesthoods by the Semitic priesthoods, the chief types of sacerdotalism, and for that reason most vividly showing the vices of the system. The Catholic Church during the Middle Ages was the depository of all that was best in the knowledge of the time, and during a long period of its earlier history was foremost in the endeavour to extend that knowledge. Moreover, the Greek people was not a people of intense inner life. Their schemes of thought and of imagination were dependent upon external phenomena. Their religion was a transfiguration of nature, and rivalled nature in variety and luxuriance. Their philosophy chiefly busied itself with efforts to construct symmetrical ideas of the operation of external agencies. Their art, again, was an objective art—an art of the eye. Greek music only existed as an expression of choral movement; their poetry was almost wholly dramatic, and their architecture and statuary sought chiefly a perfection of limited form. The French, though far inferior in order, yet show much of this preference of the symmetrical to the mystic, both in their art and in their philosophy. The character of their mediæval architecture alone seems to disprove this, but it will be remembered that this is no more French than it is English in origin.

Our kind readers will not look upon these remarks as digressive; for it is the absence of an overpowering impulse to the unfolding of thought from within, and the presence of a tendency to reason upon that which is without, which partly accounts for the sudden bloom of the science of observation about the time of Pericles, and which also accounts for the stranger spectacle of a priesthood in the front of the battle. But, as we have before hinted, there may be another reason for this in the proper function of the Asclepiadæ, which led them to the study of disease. There is what seems to us a remarkable likeness between their position and that of the Franciscans in the thirteenth century, whose priestly functions also led them to the same study. In them, as it has well been said,[1] we find "a religious corporation established for purposes purely spiritual, but whose members, in the course of carrying out those purposes, were compelled to observe facts, to devise remedies, and to adopt methods which were not the methods of the schools"—all which "encouraged physical studies as a means of alleviating the sufferings to which they were compelled to minister, and created a school of pure experimentalists." Partly, then, by the happiness of office, partly by the free spirit of their own nation, and partly in scorn of irregular emulation, the priests of Cnidus, and after them the priests of Cos, discovered a truer method of medical research. This innovation was vast both in its actual and its historical aspects, and even now needs no revolution of principle. The Hippocratic school, as we have said above, declared that the science of medicine must rest on pure observation; and they also showed, in their lives and in their writings, the value of those qualities of personal character which are inestimable in the practice of arts like policy and medicine, where actions cannot be limited to the circumference of positive knowledge.

Henceforth, then, the history of Greek medicine becomes the history of the Coan Asclepiadæ. From among them looms out, with more or less distinctness, at a period rich in greatness of every order, Hippocrates, one of those great men who best represent their age, being at once the product of it, and by reaction having had vast influence upon its development. From indolence partly, and partly from reverence overwrought, those who have become great often receive an honour due rather to an occasional eminence than to permanent desert. Of really great men, however, there are some whose vigorous genius has broken the bonds of traditional system and discovered new kingdoms of thought; there are others, again, who have sprung up in the midst of an active time, and who have advanced knowledge more by the extent than by the daring of their achievements. Personal isolation or social resistance may much or altogether prevent the proper effects of the former, so that their fame may surpass their influence upon posterity, and the records of them have a dramatic

[1] See Westminster Review, Jan. 1864; art. on Roger Bacon.

rather than a positive value. The works of the latter, however, being forwarded by circumstance and brought to ripeness, remain among the enduring possessions of time, and are directly or indirectly subservient to daily use. The reverence of posterity is paid to the lives of the former, to the works of the latter. Those then, whose names mostly live in the mouths of men are those who, like Hippocrates, seem to have joined to transcendent mental qualities such accidents of birth and condition as to give them together the glory of discovery and the riches of success. Their descendants follow the paths newly found; industry forwards what genius initiated, and knowledge is increased towards some partial fulfilment. The great reform in thought which Hippocrates was perhaps the first to proclaim—viz. that general laws can only be established upon repeated and multiplied observation,[1] was scarcely seen by his contemporaries and followers when it was lost in the dissolution of Greek national life. For the world, as we see it, is not a system of continuous evolution, but presents a series of partial completions, each in its turn, like the peach on the wall, passing away to give place and nourishment to a more perfect successor. The life of the world seems to depend upon the growth and decay of nations, as the life of man depends upon the growth and decay of the fruits of the earth and of the beasts of the field. He, therefore, whose genius and labour have marshalled the thought of his fellows, whose activity has given birth to lively progress, and whose doctrines have served for enlightenment and direction, he and his works must suffer eclipse with the nation which gives him existence. Transplanted into a new and a foreign soil, these can never again have the life which they had in the old tree: the broken branch has no place in the growing plant; it is laid up for our admiration, but springs no more into bud. The new order comes of native forces; the ancient stock receives no additions, and gives occasion to little growth beyond the accretions of those busy wits who mistake the incrustations of curious learning for the orderly development of positive knowledge. The doctrines of the Hippocratic school can never have any authoritative value for us, and have perhaps little organic connexion with our modern systems of thought, yet, on the other hand, some knowledge of them has many indirect uses, and cannot fail to give pleasure and profit.

[1] M. Littré gives the repute of priority on this question of method to Alcmæon of Crotona, in which he simply follows Plutarch, whom he quotes. From what we can know of Alcmæon, he seems in practice at least to have pursued a subjective method, while in practice Hippocrates pursued the reverse. Indeed, all that Hippocrates took from the Pythagorean school he took to his injury. We prefer, then, Galen to Plutarch, both as a better authority and also as better supported by evidence. He says, "*καὶ μάλιστα τῇ πείρᾳ προσέχων τὸν νοῦν, καὶ πάντα ταύτῃ δοκιμάζων, ἵνα δ' ὅλως ἀφίκηται πολλαῖς ἐπινοίαις χρώμενος λογικαῖς.*"—Galen. Com. 3. in lib. de Articulis.

(To be continued.)

Art. III.

Observations on Fever accompanying Surgical Affections. By F. W. Gibson, M.D. Lond., House Surgeon at the Taunton and Somerset Hospital, Taunton.

I have ventured to consider the results of a series of observations on the fever following surgical injuries (traumatic fever) not unworthy of publication; first, because, as far as I am aware, no one has treated of this variety of fever since Billroth's Researches.[1] Secondly, because, although my observations lead me, in many instances, to conclusions identical with those at which he arrives, yet, in some others, I have been induced to advance opinions different from his.

I have observed more than a hundred cases of traumatic fever following almost every variety of surgical injury. Of these cases I have selected sixty-two.

The temperature was measured by means of an accurate Fahrenheit's thermometer placed in the axilla, the usual precautions against the obtaining of an incorrect result being employed. The rate of the pulse and of the respirations was observed at the same time, and, in a few of the cases, the urine was carefully analysed.

Proposition I.—After a great number of injuries there is no fever. Of Billroth's seventy-seven cases there were twenty such, but this proportion of twenty cases without fever to fifty-seven with fever is, as remarked by Mr. Windsor, the editor of the 'Abstract,' entirely without any statistical value, since, as soon as it had been shown that fever was often absent in slight cases, Billroth discontinued his observations on such cases.

In my sixty-two cases there were eighteen in which fever did not supervene; this proportion of eighteen cases without fever to forty-four with fever is of statistical value, since I continued my observations on slight cases. The existence or non-existence of any persistent rise of temperature above 99·5° was the criterion adopted in determining the presence or absence of fever.

The following list shows the nature of the injuries in the non-febrile cases:—Concussion of the brain (three cases); dislocation of the hip, simple fracture of the tibia, severe bruise of the muscles of the leg, sprain of the ankle (two cases); excision of the eyeball, scalp wound, removal of epithelial cancer from the lip (two cases); circumcision, removal of a large piece of glass from the popliteal space, strangulated inguinal hernia, injection of hydrocele, amputation of finger.

[1] An abstract of his elaborate Memoir was published in the Report on Surgery in the Year Book of the New Sydenham Society for 1862.

The conclusion at which I have arrived from the consideration of the facts observed in these cases, coincides with that recorded by Billroth—namely, that neither the part affected nor the extent of the wound are in themselves decisive as to the occurrence of fever.

In two cases only, however, were the injuries at all severe. In one, that of dislocation of the hip, other lesions of a very severe nature co-existed—viz., fracture of the pelvis, effusion of blood into the cavity of the peritoneum, and peritonitis of a low type. The absence of any abnormal increase of the temperature in this case, notwithstanding the presence of a pulse of 136, led me to the diagnosis of internal hæmorrhage, which was proved to be correct by post-mortem examination. In the second case, that of the removal of a large piece of glass from the popliteal space, the non-supervention of fever was due to the fact that the wound healed by primary union. In all of the cases of wounds in which fever was absent, suppuration was also absent. I should not, however, be justified in asserting that the occurrence of suppuration is always marked by an abnormal degree of temperature, and that, where the temperature is normal, suppuration is not going on.

Billroth is of opinion that suppurating wounds may be unattended by fever.

I now proceed to the consideration of the forty-four cases in which fever followed the injury.

Proposition II.—In the majority of the cases the fever began very soon after the infliction of the injury.

I have no sufficient data from which to derive any accurate statistics as to the average time of the commencement of the fever, but I am able to state that in *all* the cases the thermometer indicated an abnormal degree of temperature within twenty-four hours after the injury, in twelve within twelve hours, in twenty-three within six hours, in three within three hours, in two within two hours. In four cases only was the indication of an abnormal increase of temperature delayed beyond twelve hours. It would appear, therefore, that in more than half of the cases the fever began within six hours after the infliction of the injury.

The fever generally augments very rapidly. In thirty-two cases the maximum temperature was attained within twenty-four hours after the commencement of the fever; in five cases within forty-eight hours; in three cases on the evening of the fourth day; in one on the evening of the fifth; in one on the evening of the sixth; in one on the evening of the eighth; in one on the evening of the twelfth. In all these last cases the maximum point of temperature was reached by regular morning remissions and evening augmentations.

Notwithstanding the rapid augmentation of the fever it rarely commenced with a rigor. In three cases only were rigors observed on the first day of the fever; in two of these three cases the rigors were severe, in one slight.

The maximum temperature in all the cases occurred in the evening. The mean of the maximum temperatures of the forty-four cases was 102·1°. The maximum temperature observed was 105°. In twenty-four cases the temperature was between 100° and 102°; in fifteen cases between 102° and 104°; in five between 104° and 105°. Rigors occurred in three cases during the course of the fever. In all the cases in which rigors were observed they were very speedily followed by the commencement of the process of suppuration, either in an abscess or between the edges of a wound. The temperature invariably rose during rigor, and remained high until the suppurative process was complete, when it fell in the majority of cases.

In all these cases the defervescence was gradual; in all sweating, to any considerable extent, was absent.

My observations tend to confirm Billroth's opinion that "any considerable height of temperature during the first day is, in general, without any importance. If, however, after the fever has been moderate during the early period, there should occur a considerable increase in the temperature, it is tolerably certain that some accidental local inflammation, or some constitutional affection, is in process of development."

A continuous rise of the temperature without any considerable morning remissions is of evil augury. The defervescence began on the same day as that on which the maximum temperature was attained in thirty-five of the forty-four cases. In the remaining nine cases the defervescence began in five cases thirty-six hours after the acme of the febrile attack; in two, forty-eight hours after; in one, sixty hours after; in two, fatal cases, there was no defervescence. In fifteen cases the defervescence coincided with the discharge of pus either from an abscess, or from an ulcer, or from between the edges of a wound. In sixteen cases the defervescence was coincident with sweating. In the remaining cases no causes for the diminution of the temperature was discovered.

In seventeen cases the fever terminated suddenly, by crisis; in twenty cases the fall of the temperature was gradual from day to day, until the normal standard was attained (lysis); in the remaining seven cases the temperature fell suddenly to a certain point, and then gradually to the normal degree (lysis and crisis combined.)

In the majority of the second class of cases, and in all of the third class, there were morning remissions and evening augmentations of temperature. Nine of the first class of cases were wounds of the soft parts. In four of those nine cases, in which the wounds healed by primary union, the fall of the temperature appeared to be due to the completion of the healing process. In all of these cases the fever was slight and of short duration. In the five remaining cases in which the wounds healed by granulation, the fall of the temperature was coincident with, and seemingly due to, the appearance of suppurative discharge from the wound.

The fever was greater and its duration longer than in the former cases. The remaining cases included in the first class were slight injuries of joints and fractures, in none of which was any cause discovered for the sudden fall of the temperature. The mean duration of the seventeen cases, which terminated by crisis, was 4·1 days; the mean temperature, 101·4°. Of the twenty cases in which the fever terminated by lysis, eleven were wounds of the soft parts, all of which healed by granulation. In all the appearance of suppurative discharge was noticed by a diminution of the temperature, if not immediately, yet in a very few hours after. The remaining cases consisted of fractures and of severe injuries of joints. The mean duration of these twenty cases was 9·9 days; the mean temperature, 102·4°.

Of the seven cases which terminated by crisis and lysis, four were wounds of the soft parts which healed by granulation, three were cases of fracture, and one of severe injury to a joint. The mean duration and mean temperature of the first class of cases (the mean of the third class, on account of the small number of cases, is not given) is much less than the mean of the second class. This is due to the fact that in the former are included a large number of cases of slight injuries and of wounds which healed by primary union; in the latter, the majority of cases are severe injuries and wounds which healed by granulation.

Duration of the Fever.—Billroth states "that in most cases the fever lasts two to seven days; ten cases continued five days, eight six days, and nine seven days."

The mean duration of forty-seven cases observed by me, the number of which is nearly equal to that of those treated of by him, was 7·7 days; the minimum duration 2 days; the maximum 36 days. In 18 cases it did not exceed four days; in 20 cases it was not greater than eight days; in 5 cases it did not amount to more than sixteen days. Of the remaining cases, it reached in two, twenty-four days, in one twenty-nine, and in one thirty-six.

The mean duration derived from the foregoing analysis is greater than that given by Billroth. In the third and fourth class of cases the duration very considerably exceeds that recorded in any one of his cases. The cause of this difference may be thus explained. Billroth is of opinion that "after the traumatic fever has ended, or in other words, after the temperature of a patient has been below the maximum of normal temperature for at least twenty-four hours, there often occurs a fresh attack of fever, although no special complication has supervened. (This second attack he denominates a secondary fever.) When traumatic fever lasts more than seven days we may consider the prolongation to be caused by there being no interval between it and secondary fever."

Amongst my cases I do not find one from which I can derive any fact in favour of either of these statements. Remarkable remissions and augmentations of the temperature, due to various causes, such as the formation and maturation of abscesses, extension of ulceration, and so on, did certainly occur in many cases, but in no one instance did a fresh attack of fever supervene after the temperature had remained at the normal degree for twenty-four hours; nor can I understand what grounds Billroth has for his second assertion. Equally remarkable variations of temperature are to be observed in cases of typhoïd fever. Should we, therefore, be justified in saying that if a case of that disease lasts more than seven days, the prolongation is due to there being no interval between the primary attack and a secondary fever caused by some complication—pneumonia or peritonitis?

These, then, are the reasons which have led me to venture to doubt Billroth's statement in regard to the occurrence of secondary fever as distinct from the primary attack. Hence, in the estimation of the duration of the fever, I have in some instances included days which he would assign to the secondary attack.

Effect of the Nature of the Injury and of the part affected on the Duration and Intensity of the Fever.

Amputations.—Billroth affirms, that whether the wound of an amputation healed by the first or the second intention, made no difference in the duration of the fever. My observations, which are, however, few, do not tend to confirm this opinion, but rather to prove the contrary—viz., that in amputations, the amount and duration of the fever are smaller in those cases in which the wound heals by primary union than in those in which suppuration occurs. This opinion is confirmed by the result of observations of thirty cases of wounds other than those of amputation. In eleven of these thirty cases, there was no fever; in twelve, it was of slight amount and duration (mean temperature, 101·2; mean duration, seven days). In eight cases, it was of much greater amount and duration (mean temperature, 103·8; mean duration, 16·1 days).

In the two first classes of cases, the wound healed by primary union; in the third class, by granulation. I must, however, admit that the cases included in the last class were of a *rather* more severe nature than those in the first two. Notwithstanding this there are sufficient grounds for doubting Billroth's assertion, which is certainly at variance with the *teachings* of all other surgical writers.

Simple Fractures.—Mr. Paget, in his remarks on the repair of fractures,[1] states, that in good instances of repair, and where the parts are kept at rest, the inflammatory exudation usually ceases after the second or third day.

Assuming that the fall of the temperature to the normal standard is coincident with, or at any rate nearly coincident with, the termination of the process of inflammatory exudation, the results of my observations in sixteen cases of fracture are in favour of this opinion; for (leaving out of consideration two cases of fractured ribs, in which pleuro-pneumonia supervened) I find that in all the cases fever was either absent or slight in amount and intensity. It would appear, therefore, that in simple fractures, if no secondary complication occurs, union takes place without the occurrence of any but very slight inflammatory symptoms; and that in those cases in which a high temperature is observed, some secondary complication must be sought to account for this elevation.

Injuries of Joints.—(Thirteen cases.)—In cases of simple synovitis in healthy persons, caused by contusion or sprain, the duration and amount of the fever are slight; in cases in which the joint is opened, the duration and amount of the fever are much greater. Exposure of the interior of the articulation to the air appears to be the determining course of this difference, since, provided this condition is absent, a very severe injury of a joint may not be followed by any fever.

Hernia.—Billroth has observed the temperature in four cases only of strangulation of the bowel, and secondary peritonitis. He says, that in these cases the temperature was either normal or only slightly increased. In two of three cases observed by me the peritonitis and fever were equally slight in amount; in one, a severe fatal case, the temperature reached 103·8°.

Age and constitution "had no essential influence over the duration." (Billroth.)

All *my* cases were *healthy* males, therefore I cannot make any statement as to the influence of constitution.

The mean duration appeared to increase in direct ratio to the age. Thus:—Age 1 to 15 years inclusive (13 cases), mean duration, 5·6 days; age 15 to 30 years (16 cases), mean duration, 5·8 days; age 30 to 45 (13 cases), mean duration, 8·4 days; age 45 to 60 (13 cases), mean duration, 10 days. The cases

[1] Lectures on Surgical Pathology, last edition, p. 180.

aged from 60 to 75 are too few for the calculation of the mean duration. The amount and character of the injuries in each class were much the same.

With regard to the effect of loss of blood, either accidentally or by phlebotomy, in traumatic fever, I can say but little, since in but few of the cases did any accidental hæmorrhage occur, and in none was the lancet employed. Judging, however, from the data afforded by these few cases, and from what I have observed in non-surgical diseases, I think I may state, that loss of blood diminishes the temperature for a time; but that diminution is followed by such an increase of the temperature that it attains a degree very considerably higher than that at which it stood before the hæmorrhage. This opinion coincides with Billroth's.

The Urine.—Müller has examined the amount of urea before and after operations in seven cases. According to him, the general rule was, that at first (owing to the loss of substance from the operation?) the urea lessened, but on the second day increased again from increased disintegration and pyrexia, or sometimes without pyrexia. After a longer time, the urea returned to its normal amount, or fell a little below this.[1]

The urine was analysed by me in four cases of traumatic fever. In one case, (that of a man aged fifty-seven, whose thigh was amputated high up for moist gangrene of the leg, caused by the obstruction of the femoral vessels, which had been injured by a cart-wheel passing over the limb.

The symptoms of gangrene had lasted three days.)

The urinary secretion during the twenty-four hours immediately preceding the operation was—Amount, 1030 cubic centimètres; Urea, 36·8 grammes; Chloride of Sodium, 2·0 grammes. The Temperature on the evening of the previous day was 102·2°. Pulse, 120. Respirations, 36.

The urinary secretion for the twenty-four hours immediately following the operation was—Amount, 370 cubic centimètres; Urea, 8·5 grammes; Chloride of Sodium, 0·5 grammes. The Temperature on the evening of the day of the operation was 102·2°. Pulse, 120. Respirations, 20.

The urinary excretion on the following day was—Amount, 300 cubic centimètres; Urea, 7·8 grammes; Chloride of Sodium, 11·3 grammes.

The amount of food was the same on all three days.

The man died fifty-eight hours after the operation, from exhaustion, in a semi-comatose state.

Can this great diminution of the urinary excreta be entirely accounted for by the loss of substance caused by the operation,

[1] Parkes on the Urine, p. 178.

which loss was, no doubt, considerable? Must not the effect of nervous shock be also taken into consideration?

The second case was that of a man aged fifty-one, whose elbow was excised on July 17th, 1865, for a compound fracture of that joint. From the following table, which gives the daily amount of urinary excretion, with the temperature, it will be seen that on the 18th and 19th the amount of urea was lessened; that on the 20th it increased, probably owing to the augmentation of the pyrexia and to the confined state of the bowels; that on the 21st and 22nd it again fell, owing to the diminution of the pyrexia and to the relaxation of the bowels. On the evening of the 23rd, a slight rigor occurred, and the temperature rose to 103°. The amount of urea on the 23rd was, as will be seen, more than double that secreted on the 22nd; but this increase must be in part attributed to the increased quantity of food given. The rigor appeared to coincide with the congestive stage of the inflammatory process which preceded suppuration. The temperature continued high until the 24th, on which day the wound began to discharge freely.

The amount of urea did not begin to lessen until the 26th, at which time the wound was healing.

Table of the Urinary Excretion.

Date.	Temperature.		Total Urea.	Urea per kilog.	Food.	Remarks.
July.	M.	E.	Grammes.	Grammes.		
18	100·4	101·8	24	0·391	Low diet.	
19	103·8	104·2	13·4	0·218		
20	101·4	102·8	28	0·457		
21	100·2	101·2	16	0·26		
22	101·6	103	12·9	0·21		
23	101·4	102·8	28·3	0·462	Full diet, meat, porter 2 pints.	Rigor.
24	100	101·6	24·8	0·404		
25	100·4	101·4	28·5	0·465	....	Wound discharges freely.
26	100·4	100·4	25·6	0·417		
27	100	101	24·1	0·393	....	Wound healing.
28	100·2	100·2	22·9	0·357		

The third case was that of an injury to the base of the brain, caused by a fall on the back of the head. It occurred in a boy aged fifteen. In this case, although the pyrexia was very slight, yet the secretion of urea was great, considering that very little food was taken; on one day, the fifth after the accident, it reached 0·933 grammes per kilogramme. The amount of phosphoric acid excreted was also large, reaching on one day 0·1256 grammes per kilogramme. How is this large excretion to be accounted for? The symptoms, paralysis of the right auditory and facial nerves, appear to indicate some injury to the nervous fibres at the base of the brain, probably laceration (the absence of any notable pyrexia would seem to exclude the idea of any inflammatory action). Can the increase of the urea and phosphoric acid be attributed to this lesion? The absence of sugar and albumen, and of any increase in the urinary water, is the only, but nevertheless an important fact against this opinion.

The fourth case was that of a boy, aged seven years, who sustained a simple fracture of the femur and tibia. In this case the amount of fever was very slight, and nothing worthy of record was observed in regard to the urinary excreta.

Retention of urine occurred in five only of the sixty-two cases.

The Importance of the Indications afforded by the Thermometer in Cases of Traumatic Fever.—Wunderlich and many other observers have proved that the objective heat or coldness of the skin is no test of the presence or absence of fever, and my observations on cases of traumatic fever lead me to a like conclusion. For example, a temperature of 103° co-existed with a cool[1] skin, a temperature of 100·6° with a hot skin. The like want of correspondence was observed in many other cases.

The frequency of the pulse was almost always proportional to the temperature. In some cases, however, a marked disproportion was observed; e. g., a pulse of 80 co-existed with a temperature of 104°; a pulse of 90 with a temperature of 105°; a pulse of 100 with a temperature of 99·3°; a pulse of 120 with a temperature of 99·4°. The temperature, therefore, is a much safer guide in judging of presence or absence of fever than the rate of the pulse, since the latter may in many cases, either from the effect of shock or from the debility of the patient, be exceedingly rapid, whilst the non-occurrence of any, or of any considerable rise in the temperature, proves the absence of fever; whilst, on the contrary, the slowness of the pulse might lead the surgeon to the diagnosis of the absence of fever, which the augmentation of the temperature proves to be present. In many cases of the formation of abscesses I found a rise of temperature before the pulse gave any indication of their approaching formation, and was thus led to examine the affected part, and to detect disease, which might otherwise have escaped my observation.

The absence of any fever of moment in all cases of simple fracture, proved by the thermometer alone—for in many cases the pulse, from the effect of nervous shock, or otherwise, was very rapid—would appear to indicate the adoption of a nutritious diet from the first in these cases.

General Treatment.—In none of the cases did any active antiphlogistic measures appear to be demanded; on the contrary,

[1] That is, *objectively* cool or hot.

in many, a highly stimulative treatment seemed absolutely necessary. The question of the effect of alcohol, in appropriate quantities, on the amount and duration of the fever, is beset with too many difficulties for me to give any very satisfactory solution. I believe, however, that I have sufficient data for the conclusion, that in no case did the free administration of alcohol tend to increase either the amount or duration, but that the balance of evidence leaned in the opposite direction.

Local Treatment.—In regard to the effect of the application of ice to wounds, which Billroth considers to be a most excellent antiphlogistic remedy, especially in cases where suppurative inflammations are spreading round wounds, I can say little or nothing, since that remedy was employed but very rarely, mainly on account of the difficulty of obtaining it. The evidence is in favour of cold irrigation, which was used in not a few cases, tending to mitigate the febrile symptoms and to check the process of suppuration. Poultices, by hastening the maturation of abscesses and the completion of the process of suppuration, reduce the amount and duration of the fever.

ART. IV.

An External Latero-Angular Dislocation of the Right Elbow-joint: a new Form of Dislocation, with accompanying Dissection of the Limb. By FREDERICK JAMES GANT, F.R.C.S., Surgeon and Pathological Anatomist to the Royal Free Hospital.

THIS remarkable and hitherto unrecorded dislocation occurred in the person of a man aged fifty, whose right forearm was jammed between the "buffers" of two railway carriages at the Great Northern Railway terminus. Immediately after the accident, on March 23rd, 1865, he was brought to the Royal Free Hospital, and admitted under the care of my colleague, Mr. De Meric. I saw the case with him, and assisted in amputation of the arm, the necessity for which operation will presently become apparent; and as Mr. De Meric kindly placed the limb at my disposal, I will first describe the external appearances which it presented before removal, then the conditions discovered by dissection, and lastly, endeavour to connect the two, so as to interpret the former as signs of the dislocation; and moreover, show how its reduction may apparently be effected.

The external appearances of the limb were peculiar, and at once caught my eye. The great transverse width of the elbow-joint was very obvious, and internally the trochlear portion of the humerus could be plainly felt and seen, threatening to protrude through the skin forwards and inwards—the direction which the humerus and arm, therefore, had assumed. More internally, another groove between the innermost ridge bounding the trochlea and the condyle was less conspicuous; and external to the trochlea the remainder of the articular portion, or capitellum, could be felt and seen, although less distinctly, as it receded from the skin, owing to the oblique direction of the humerus, its anterior surface looking outwards. In any form of dislocation hitherto recognised, who ever thus saw and felt the whole articular end of the humerus bare of muscle and immediately beneath the skin?

Continuing the external appearances in this case: just above and behind the internal condyle there was a small contused aperture leading to the articular surface, thus making the dislocation compound. Externally, neither the condyle nor any bony prominence could be felt. The forearm was not only semi-flexed forwards, but its axis formed rather less than a right angle with the humerus laterally, an external latero-angle. The origins of the supinator longus and extensor muscles of the wrist were driven up in a heap, so to speak; and as they, in common with all the muscles, were prodigiously developed, the fleshy swelling above the external condyle was very conspicuous. Posteriorly, the olecranon could be felt, although the arm was, as I have said, half flexed; but this bony prominence was not in its relative situation and direction to the humerus; it was underneath the outer half of the articular surface or capitellum, and it had of course the transverse direction of the whole forearm outwards.

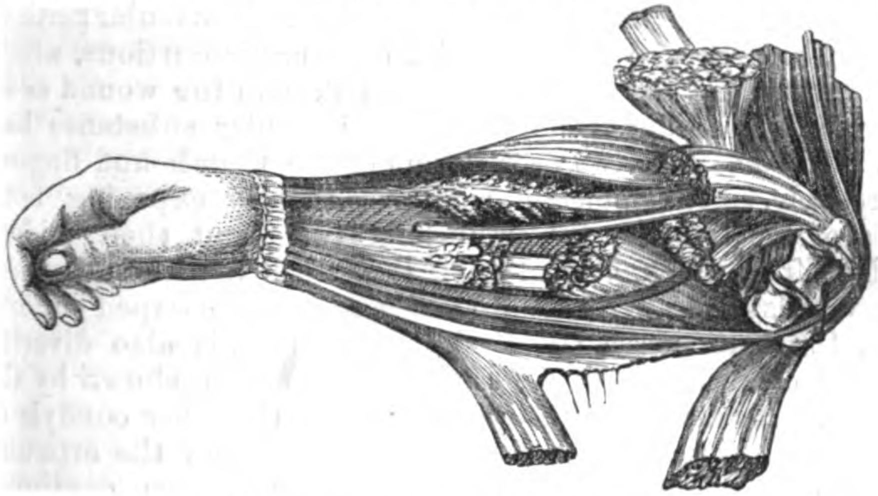
Passing from the outline of the joint and the relative attitude of the arm and forearm, the aspect of the latter was no less remarkable. Its anterior surface looked obliquely inwards and backwards, so that the forearm was half pronated, and the hand hung with the thumb inwards, and the palm downwards.

To complete the description: the forearm remained fixed or locked in this position from the elbow joint to the hand, when the limb was left to itself. I may add, a large lacerated wound in the front, now the back part of the forearm, about its middle, led down to the radius and ulna, both which bones, with the interosseous membrane, could there be plainly felt and seen to the extent of about two inches, between the flexor muscles, now reduced to strings, as if they had been ploughed up in furrows.

All these characteristic appearances will be realized by inspecting the accompanying drawing made by Mr. W. G. Searson, which, representing the relative position of parts as seen superficially before any further dissection, preserves also the *tout ensemble* of the limb ere the skin was removed.

Strange to say, the skin was nowhere broken or bruised but in the two parts named—the front of the forearm, and just above the internal condyle; and there being scarcely any swelling at this early period, the appearances I have described were very striking.

The pathological conditions discovered by dissection were no less remarkable: the skin was almost completely detached from the aponeurosis enclosing the muscles on the front and back of



the forearm, and this lesion could be traced above the joint around the arm. The large tract of subcutaneous cellular texture, or superficial fascia, thus disorganized, was infiltrated with blood in patches here and there; yet this condition was not discernible through the skin itself, for the most part unbroken, and apparently unbruised.

I may here observe, that in the dissection of many other limbs amputated for recent severe injury much involving the soft parts, I have frequently noticed the great extent to which the skin was detached beyond the obvious seat of injury, and the absence of any mark of violence or other external appearance which might indicate this lesion during life, if the limb were seen shortly after the injury. Owing to the accompanying collapse, and perhaps, therefore, in proportion to its severity, the skin may be then even whiter and fairer than before such damage, particularly if supported by much subcutaneous fat; and it retains this complexion until the usual discolorations of bruise supervene. Should, therefore, primary amputation be deemed necessary, the deceptive appearance of the skin, in the first instance, is significant with reference to the *part* selected for operation, lest, while aiming at what is now-a-days called "conservative surgery"—i.e., simply the preservation of as much of the body as possible in operations, the surgeon may unwittingly sacrifice the flaps by sloughing of the stump.

The extent to which the muscles are destroyed by injury is equally misleading, because equally undefined externally. In the present instance the remnants of several muscles were visible through the large wound in the forearm. Dissection showed that all the flexor muscles, superficial and deep, were torn across, partially, or entirely. The flexor carpi radialis, flexor digitorum sublimis, palmaris longus, and the flexor carpi ulnaris—the whole group of muscles arising from the inner condyle, excepting the pronator radii teres, are severed (see fig.) in their muscular portion, sparing, however, the tendinous and aponeurotic portions, which now representing the muscles, appeared deep in the wound as so many shreddy strings, from which the muscular substance had been raked off. The two deep flexors of the thumb and fingers, respectively, are completely ruptured, thereby exposing both bones of the forearm, for an inch or two about their middle portion, from which, at that part, the periosteum was detached. The vessels and nerves have *all* singularly escaped injury. Thus, the ulnar nerve, stretched, but not torn, is also diverted from its course, as seen in the preparation and shown by the drawing. It passes from its groove, behind the inner condyle of the humerus, thence along the posterior margin of the articular surface of that bone, and thus gaining the forearm continues down to the wrist. It stood out in the wound as one of the strings referred to. The inferior profunda artery, accompanying this nerve above the condyle, is also unscathed. So, likewise, is the ulnar artery—a singular escape, considering the severe laceration of the two muscles, between which it and the nerve are situated in the greater part of their course. The median nerve, with its inter-osseous branch, and the corresponding branch of artery, are also uninjured; and lastly, the radial nerve and artery—this vessel beating freely under the finger when first I examined the arm, before amputation. These nerves and vessels are not shown by the fig.

It would appear that muscular texture offering less resistance to the forces of contusion and extension, all the muscles thus injured gave way; while, tendons and aponeuroses, the nerves and vessels, being more resisting to such forces, none of these textures yielded. The latter cannot owe their preservation to position; for they were alike exposed to injury, the ulnar nerve and artery in particular.

I have dwelt somewhat on the morbid conditions of the subcutaneous cellular texture, the muscles, vessels, and nerves, in this case, because the description being authenticated by dissection, throws light on the design and performance of amputation in other forms of severe injury. It is, in fact, a good example of the guidance of what I have termed "Pathological Operative Surgery."[1]

Passing on to the seat of dislocation, the disposition of the muscles, the destruction of the ligaments, and the position of the bones are severally very remarkable.

All the muscles (excepting the ulnar portion of the flexor carpi ulnaris) which arise by a common tendon from the inner condyle of the humerus, pass transversely outwards over the

[1] Medical Times and Gazette, July and August, 1865.

bone just above its articular surface: thus corresponding to the ulnar nerve in its course across the posterior margin of the cartilage, in this case. Between the two, the articular end of bone has made its appearance, directed inwards and forwards, while its anterior surface looks obliquely outwards, so that the inner condyle is most anterior. As before mentioned, it and the trochlea threatened to protrude through the skin. Externally, the biceps and brachialis anticus muscles, both of prodigious size, diverted outwards from their course, exposed the capitellum of the humerus, but their insertions conceal the precise position of the ulnar coronoid process and head of the radius. More externally, or rather, owing to the lateral angle of the forearm with the arm—higher up, the supinator longus and the two extensors of the wrist are elevated as a great muscular cushion over the head of the radius. Of these muscles, the supinator and superficial (or long) extensor of the wrist, are not detached from their origin—the external ridge above the condyle; but the deep extensor carpi radialis (brevior) was partly detached from its origin—the condyle, this muscle being here split into two portions, loosely encircling the head of the radius, which by its dislocation was in contact with and immediately concealed by the superficial extensor. Posteriorly, the insertion of the triceps extensor muscle was not torn but twisted transversely outwards, in the direction assumed by the olecranon and shaft of the ulna.

Of the ligaments; the anterior, the internal and external lateral ligaments, are entirely demolished, the posterior ligament alone remaining, under cover of the insertion of the triceps.

The articular ends of the three bones forming the elbow joint, have the following relative position. Both bones of the forearm have undergone an external lateral dislocation, as complete as is usually met with, the larger sigmoid cavity of the ulna resting against the external condyle of the humerus. But both bones of the forearm have also undergone a further and *peculiar* displacement laterally, whereby their axes form a right angle or less with the humerus; thus constituting an external latero-angular dislocation. The inner half of the large sigmoid cavity embraces the condyle and capitellum, just behind its articular surface; the coronoid process (concealed) resting externally against the one, and the olecranon (as seen in the fig.) being underneath the other as far as the ridge between it, the capitellum, and trochlea. The head of the radius (concealed) is in close relation to, not in contact with, the external ridge of the humerus and just above the condyle, the ridge bisecting the round articular facet. Thus, the head of the radius is neither in front nor behind the humerus; it is not dislocated forwards or backwards. A portion of cartilage was chipped off the articular surface of each of the three bones.

Taking now the pathological conditions of the joint in connexion with the external appearances of the limb, it may be inferred, that the radius and ulna were wrenched outwards to a right angle with the humerus, making an external latero-angular dislocation. Simultaneously, all the ligaments of the elbow joint, save the posterior one, were entirely torn across; while the end of the humerus protruded inwards and forwards, between the ulnar nerve with the olecranon and the muscles arising from the inner condyle.

The *width* of the elbow transversely was thus greatly increased, both by the new position of the radius and ulna, capped by the fleshy bellies of the three muscles overlying the head of the former bone, and by the trochlea of the humerus, which projected inwards and forwards as far as the skin. Above the (former) articulation, the projection of the humerus inwards produced a divergence of the brachial vessels and nerves; the ulna nerve accompanying the bone inwards was stretched around its articular end, the brachial artery and median nerve passing outwards with the forearm.

The brachialis anticus muscle, partly torn off the end of the humerus with the anterior ligament, and diverted outwards, still remained attached to the coronoid process below; and the biceps also, diverted in like manner, still retained its hold of the radial tubercle. Both these muscles therefore, acting advantageously, *flexed* the forearm *forwards*, and rotating the humerus outwards, gave an oblique direction to its dislocated articular end; while the long supinator and the two extensors of the wrist, having alike become flexors, retained the forearm *laterally* bent on the humerus. Even the triceps muscle acted partly as a lateral flexor, its insertion supporting the olecranon outwards, in a kind of sling.

Lastly, the direction of the front of the forearm *backwards*, was produced by the very favourable pronating action of the pronator radii teres, aided somewhat by the extensor carpi radiales, and by the remnants of flexor muscles passing from the internal condyle to the hand, all of which assumed the function of pronators, owing to the extraordinary direction of the forearm outwards upon the humerus. Hence also the pronation of the hand, the thumb inwards and palm downwards.

The relation of the injury in the forearm to the dislocation produced is a question full of interest and importance. It indeed opens up a general inquiry, as to how far the production of this or that form of dislocation is determined by the action of different muscles, rather than by the direction of external force, which apparently effects the particular dislocations commonly witnessed; and whether therefore uncommon forms of dislocation may be referred to the division or injury of certain muscles in a part more or less remote from the seat of dislocation. In short, the question is, the relation of such concurrent injury to dislocation as cause and effect? Thus, if the quadriceps extensor muscle of the thigh were severed by a sword cut, as the person so wounded sprang forward, might there not then be produced a dislocation of the femur into the *perinæum* —a new form of dislocation, owing to this bone being delivered over to the resultant action of the ham-string and adductor muscles of the thigh, as by the momentum forward of the trunk, the femoral head started from its socket?

Thus, then, it appears to me, that both the remarkable outline of the elbow-joint, and the striking attitude of the limb, flexed laterally as well as forwards and extremely pronated, are explained by considering the new relative position of the bones, the destruction of the ligaments, and peculiar disposition of the muscles, subject possibly to the *freedom* given by the laceration of the flexors of the forearm. That the limb should be fixed or locked in the attitude thus acquired, follows, of course, from the peculiar grasp of the ulna on the humerus and the new action of the muscles around the joint.

To reduce this dislocation, I should endeavour to unlock the bones by extension and counter-extension, *not* aided by the knee in the flexure of the elbow. That manipulation would, I think, make the radius and ulna start up still further laterally upon the humerus. Nor would I endeavour to extend the forearm in a line with the arm; but availing myself of the already relaxed position of the chief opposing muscles—namely, the biceps and brachialis, anteriorly, the supinator longus and the two extensors of the wrist, externally—I would draw the forearm down while bent on the arm, and simultaneously giving it a sweeping curve forwards and inwards around the external condyle and projecting capitellum of the humerus, I trust that the bones would snap into place.

Amputation of the arm in its upper third was necessary in this case, owing to the extent of the injury. The operation, having been performed by Mr. De Meric, was followed by an excellent recovery.

Art. V.

On the Use of the Chloride of Zinc in Surgical Operations and Injuries, and especially in Operations for the Removal of Cancerous Tumours. By Campbell De Morgan, F.R.S., Surgeon to the Middlesex Hospital.

The subject treated of in this communication is presented under a twofold aspect. The use of the chloride of zinc was adopted in the first instance after removal of malignant tumours by the knife, for reasons which apply exclusively to operations on such growths. The results which were obtained led me to resort to its application in all wounds, whether made in operations or accidentally. Whether the special object for which it was at first used is attained even partially can only be proved by very long experience, but the general effects have been so immediately and uniformly beneficial in a large number of cases in which it has been applied, as to satisfy me that in the treatment of wounds the chloride of zinc is an agent of great value, well worthy of careful trial by surgeons.

The cancer wards in the Middlesex Hospital are constantly forcing on the minds of the surgeons attached to it the questions, Can nothing be done to remove this fearful disease? Is, in the majority of cases, its recurrence after operation an inevitable necessity? May it not be that the apparent return of the disease is but the continued development of germs which were not included in the extirpation of the tumour? My colleague, Mr. Moore, has recently given expression to the conclusions at which he has arrived. They are so important as to demand the most careful consideration on the part of the profession, and, if correct, will reverse the decision on the question of removal, at which a large number seem to have arrived. I think that the observations and the reasonings upon them are such as to a great extent to justify his conclusions and to encourage the hope that we may yet, in cases admitting of operation, get a mastery over this hitherto unconquerable disease. But take what view they please, it is quite clear that surgeons should endeavour in their operations to remove *thoroughly* every trace of the disease. Probably the fact first shown by Schroeder van der Kolk has not been sufficiently considered, viz., that the germs of disease lie scattered far beyond the apparent limits of the tumour. But in addition to this mode of dissemination of cancer, it frequently occurs that in operations parts of the tumour are cut through, and are left to be removed when the principal mass has been taken away. In such a case it is probable that of the innumerable living cells which are set free and deposited in the wound some will find a nidus in which they will go on developing—they will be scattered like seed, in fact, and grow wherever they find a favourable soil. That this is very often the case has seemed to me shown by the rapid appearance of points of disseminated cancer in and around the cicatrix after operation. Were the return of the disease dependent solely on a constitutional tendency or peculiar condition of the blood, it is probable that the disease when it returned in loco would do so much as it appeared at first, in a single tumour, or that it would return in a similar part to that in which it first appeared, as in the opposite breast. But both are rare occurrences. I know not how far this view may be capable of proof, but every surgeon has seen cases which would be easily explained on such a supposition, and with difficulty on any other. It appears to me that it may be to this mode of dissemination and implantation of cancer-germs that we may attribute the wider and more rapid return of the disease in situations where there is much fat, and in the young, than in elderly and spare persons. In the young, of course, the general activity of natural growth is shared by any morbid structure, and we should expect to find a wider area of disease radiating from the principal mass, in the manner described by Van der Kolk. But something may also be due after operation to the laxity of tissue, and the freedom with which imbibition and infiltration may occur at this age, In the case of malignant tumours forming in the neighbourhood of fat, it is probable that the wide-spread and rapid recurrence of the disease may be owing to the laxity of tissue alone, and its consequent capability of ready imbibition.

A case occurred at the Middlesex Hospital which, at the time, impressed me strongly with the notion that cancer could be thus propagated by implantation. A stout, otherwise very healthy woman, aged fifty-six, was admitted in December, 1863. She had been operated on by my colleague, Mr. Lawson, in the previous September, for a large encephaloïd tumour on the upper and anterior part of the thigh, which she had noticed for the first time only five weeks previously. The mass was connected to the fascia lata, above which it was embedded in fat; part of it had to be dissected away from the fascia after the principal mass had been removed. The wound healed rapidly, and she went out apparently well on the 13th October. On the 23rd November she noticed a return of the disease. On her admission the second time in December there were large nodules lying below the skin in the course of the cicatrix, and several smaller ones scattered about above and below. She had had no pain; had gained flesh since she went out; had a quiet pulse, a good appetite, and showed no appearance whatever of constitutional disturbance. The growth was removed the day after her admission, with a large quantity of the great mass of soft fat in which it was embedded. The outgrowth had again to be traced piecemeal, but it had now penetrated through the fascia and between the muscles. On examining the removed part I was struck by observing that extending widely through the fat were little deposits of the morbid structure in great number, quite distinct from one another, and varying in size from the smallest pin's head to a large grain of wheat; some of them were at least 3½ inches distant from the central masses, and they may possibly have extended still further. But the point of interest was, that while the masses which occupied the situation of the original tumour were large and spreading, these little scattered deposits were small and of comparatively uniform size. The impression conveyed was, that while the larger portions had grown from palpable masses which had not been reached by the knife in the first operation, the smaller ones had originated in minute germs, which had been scattered broadcast, as it were, in the soft structures around, and had permeated them to various distances through the delicate lax connective tissue. Of course, the disease rapidly returned, but still the parts took on a healthy action after the operation, and there was no indication of cancerous cachexia for some months.

I remember, too, a case which occurred very many years ago in the practice of Mr. Mayo, at the Middlesex Hospital. He removed a small tumour from the sole of the foot. At that time there was no appearance of disease elsewhere. In the course of three weeks little nodules of cancer appeared over the whole leg and thigh up to the groin; there were at least fifty of them. The glands in the groin enlarged at the same time; the man soon became cachectic, and died. Now, which of the two explanations is admissible? That which maintains that we had merely evidence of a cancerous diathesis, intensified by the irritation of an operation? or that which presumes that in the operation germs were set free, were carried by the lymphatics or veins, or in the areolar tissue, and becoming arrested here and there, took root, as it were, and were developed into new tumours? The patient speedily died from the effects of the disease.

It is a fact, too, which is worthy of note, that while primary cancer is uniformly single, secondary cancer is most frequently multiple. It is not a little strange that Rokitansky cites this as a proof of the existence of a cancer "dyscrasia." He admits that "carcinomata originate and subsist not rarely as local evils;" but he adds: "Far more commonly, however, they are associated with a dyscrasis, which, in point of fact, often precedes and engenders the cancer. *Hence* the multiple appearance of carcinoma, as a sequel to a single one—as the sequel to the extirpation of a voluminous and hitherto solitary one. Hence, in other cases, the original appearance of cancer in several organs simultaneously, or in rapid succession." [1]

This *original* appearance of cancer in several organs simultaneously can never be proved. But surely the multiple appearance of the disease after the development or the extirpation of a single tumour points to the setting free and dissemination of germs rather than to a "dyscrasis." And a little further on he states himself that "the highest grades of cancer crasis originate through infection—that is, through the reception into the lymphatics, or more especially into the bloodvessels of cancer cells, or of cancer blastema, of a lax, soft, semifluid character." And

[1] Rokitansky: Pathol. Anat., Sydenham Soc. Edition, vol. i. p. 255.

these cancer-cells and blastema are derived from an original tumour, which has ulcerated, or has penetrated into the canals of the bloodvessels.

If the highest grades of cancer crasis arise from infection from an original tumour, and are indicated by the production of multiple growths, why assume a dyscrasis which precedes and engenders the original tumour?

Nothing, in fact, can more resemble the secondary occurrence of cancer than the deposits and consequent secondary formations in purulent infection. The mode of distribution of these formations in the lungs and liver are at times so like, that they might, at first view, be mistaken for one another, dispersed here and there in equal sized nodules; in one or two places, perhaps, larger deposits formed. And when we find the same tendency to multiple reproduction in the neighbourhood of an original growth, whether after operation or not, why, in the face of the fact which Van der Kolk pointed out, and which is now established, refer it to any pre-existing constitutional cause, which even a *constitutionalist* would hardly admit in the case of these lung and liver deposits, and which he would certainly deny in similar local deposits from purulent infection? My own conviction is, that in most cases of recurrence, where the tumour has even been to all appearance entirely removed, the recurrence has been due to the presence of minute cancer elements which have escaped the knife, or, as is perhaps as frequently the case, to germs being set free by section of the tumour or of diseased tissue around it during the operation, and their implantation into the newly-cut structures. That this is the case is, I think, shown by the fact that where a voluminous breast with only a small tumour embedded in it is removed, and the incision is consequently very long in proportion to the size of the tumour, the recurrence frequently takes place along the whole line of cicatrix, not especially in that part which corresponds to the site of the tumour. If, then, the view which has been so ably advocated by Mr. Moore be correct, that cancer is a local disease which becomes disseminated from the point of its first invasion, the practice of early extirpation cannot be too much insisted on. A tumour, as soon as it is recognised as cancerous, should be removed, if practicable, at once. A few days, even, may make a difference; not, perhaps, to any extent in the size of the tumour, but in the extension of its germs beyond its apparent limits. It would be better even to extirpate doubtful tumours, some of which might turn out on after examination not to be cancerous, than to leave them to develope themselves, and then to find, too late, perhaps, for help, the characters of malignancy becoming marked. These views, which had often been discussed amongst the members of the surgical staff at the Middlesex, encouraged Mr. Moore, in a severe case of breast cancer, to apply the solid chloride of zinc to a large portion of the surface of the wound made in its extirpation, as he had effectually done in a case of extensive epithelioma of the face, which he showed at one of the meetings of the British Medical Association in London. This was done in April, 1864.

The patient was a spare, healthy woman, forty-three years of age. The right breast was the seat of small cancer, with adherent skin, cherry coloured over the centre of the tumour. In the axilla was a cluster of enlarged glands, which formed a tumour a little larger than that in the breast. The mammary disease was only known to have existed for four months. Mr. Moore first detached all the axillary glands, and tying them above, cut them off. He immediately touched the stump with the solid chloride of zinc. He then dissected out the diseased mammary gland, and before closing the wound he lightly touched nearly all the exposed axillary tissue with the chloride. The case ultimately did well, but what was remarkable was, while the skin over the breast sloughed to a slight extent, and was inflamed for some distance, due in great measure, according to Mr. Moore's notes, to its being somewhat tightly held together by the sutures, there "was no tension or great discharge from the axilla," and after a fortnight there is a report that the axilla is well filled up. It was clear that, at any rate, the touch with the caustic did not retard the cure.

To me it occurred that one might obtain the benefits which were sought by using the caustic in a less active form, and that a strong lotion of the chloride of zinc applied freely over the whole exposed surface, after an operation for the removal of cancer, would penetrate to some little extent beyond the limits of the section, and would at least destroy any floating particles of the disease which might adhere to it without endangering the vitality of the whole thickness of the flap.

The first case in which I tried this plan was that of a lady, forty-one years of age, who had a cancerous tumour in the right breast. She was well nourished and healthy, and had noticed the tumour about a twelvemonth. In the axilla was a gland slightly enlarged, but not hard, and in all respects the case was a favourable one for operation. The operation was performed in March, 1865. The strength of the solution employed was twenty grains of the chloride of zinc to the ounce of water; the whole surface of the wound was well sponged with it. The blood which still oozed was, as usual, rendered of a bright pink colour, and the contact of the solution at once caused a more free oozing from the exposed surfaces; otherwise no effect was perceptible. The lotion was thoroughly pressed in with the sponge, and in a little time the surface became soft and creamy in feel, and this softness extended to a little depth—a line, perhaps. Here I stopped, not knowing how far I might venture without causing sloughing of the flaps of skin. My impression was that a superficial slough would form, and would be thrown off by degrees during the suppurating stage, which, I assumed, must of necessity ensue, and which, in fact, I rather desired. The edges of the wound were, nevertheless, put together with sutures, save at the outer part, which was left quite open, to allow of the free passage of the supposed inevitable pus. A compress was put over the wound to check the tendency to any further bleeding into the cavity. After recovering from the effects of the chloroform, she complained for two or three hours of smarting pain; not more, I think, than is usually felt, and from that time she was entirely free from any pain at all. I thought it probable that on removing the compress, about eighteen hours after, the parts would be found swollen and angry, although the pain had been so slight; but instead of that, the circumstance most noticeable was the absence of even the usual amount of fulness. It was evident that action, instead of being increased, had been diminished, one might almost say arrested, by the application. The skin, even up to the cut edge, looked and felt exactly like the skin of the other breast. Blood, in much about the same quantity as is usually found after such an operation, had oozed from the wound; but it was pink and creamy in character, and what was especially remarkable was the entire absence of the peculiar odour which is generally found in blood which has been pent up beneath a compress for some hours. There was, in fact, no animal smell at all. But what most struck me in the progress of the case was the absence of suppuration. The whole line of incision united in the course of forty-eight hours, except just at the outer angle, which discharged a *very* small quantity of the same pink, creamy-looking fluid for a day or two more, and then healed. The same absence of animal odour was noticed to the end. I certainly never saw a wound which did not heal absolutely by the first intention go though its process of cure so speedily or so quietly. The patient remains perfectly well to the present time.

In cases of cancer on which I have subsequently operated I have used stronger lotions. In this first case the strength was 20 grains to the ounce. I next tried 30 grains, and then 40 to the ounce. With the stronger lotion a more rapid effect is produced, and the blood exudes more abundantly, but this is only for a few seconds, otherwise much the same course of events has been seen as in the first case during the early period. Some have healed in the same rapid manner, in some there has occurred an after suppuration, but in none have any bad effects been seen.

What the effect of this treatment may be in limiting the tendency to return after extirpation of cancer, can only be determined by time and numbers. If the views of the diffusion and transplantation of cancer germs before expressed have any truth, it cannot but be beneficial; for to some extent it certainly must alter the character of the exuded matters and of the remaining tissues. My own impression is, that in cancer it would be well to go beyond the point hitherto reached, and that this may be safely done, for the effect of the chloride of zinc seems to be limited to the point with which it is brought into contact. It appears to produce very little irritation beyond that point, so that it may be worked into the inner surface of the flap till the tissue is softened to within a few lines of the surface without risk to the vitality of the remainder. In the course of the last summer I removed a large cystic tumour from the breast of a lady. The skin was stretched over it, so that when the tumour was away, there remained two extensive flaps of thinned skin with scarce a trace of adipose tissue. I was almost afraid to use the lotion to these flaps, but I did so until the inner surface was softened. The whole was healed in about twelve days, without anything that could be called suppuration, and with the same quiet inactive state of the skin as I have before described. So far as I have observed, I am well content with the *special* effect of the treatment, though the cases on which I have tried it are too few and too recent to justify any opinion being formed. In three cases of breast cancer which have been operated on for more than six months there is as yet no appearance of return. In one—a case in which the disease made such rapid progress that it had increased by at least a quarter of its size in a fortnight—where it extended beneath the pectoral muscle and ran close upon the axillary vessels, it reappeared in the axilla in six weeks. In this case, however, the disease could not be fairly removed, and I was obliged to content myself with detaching as towards the depths of the axilla, throwing a ligature tightly round the neck and cutting it off. The operation was done in June last, and the improvement in general health and alleviation of pain which followed the operation fully justified its performance.

Another case was one of recurrent cancer in the breast a year and a half after removal. At the sternal part the disease was fixed to that bone. Towards the axilla there was a subcutaneous mass of the size of a small walnut. There were small subcutaneous tumours in the line of the cicatrix, but no deposits in the skin itself. The patient's general health was perfectly good; the axilla was free. I removed the whole of the disease, using the strong chloride of zinc lotion to all the soft parts, and destroying the sternal part, after cutting the disease away, with the actual cautery, and laying over this the chloride of zinc paste; I treated the part, in fact, precisely in the same way as has been so successfully adopted at the Middlesex Hospital, since Mr. Moore tried it in the case of cancer of the face before referred to. All the parts healed rapidly and with scarcely any suppuration, except, of course, at the sternal part. She was at the end of six weeks seized with symptoms of pyæmia and died. The origin of this was clearly in the cancellous structure of the sternum; but on the most careful examination of all the tissues about the seat of operation, no trace of cancer could be found. The final result of this case may seem to invalidate the statement which has been just made, that no bad result has attended the use of the chloride; but it does not affect the question. The pyæmia took place long after all direct effect of the chloride had passed off. The bone was intentionally exposed to some extent below its surface to ensure the removal of all cancer germs which might have developed in it, and was liable ultimately to any accident which such an exposure might subject it to. My mistake here was in not using from time to time a weak solution of the chloride, so as to keep in an undecomposed and healthy state the matters which lodged in the cancelli.

From data so limited it would be absurd to form conclusions. I am myself so satisfied with the results at present obtained, that I trust the plan will be far more extensively tried.

The singularly favourable way in which the wound healed in the first case on which I tried this plan, satisfied me that it might be beneficially adopted in other than cancerous cases. There was one point which especially struck me, as giving it great value in hospital practice; the perfect purity of the discharges from the wound during the first few days after an operation. It is well known that the presence of decomposing animal matter tends to bring any dead animal matter with which it may be in contact into a rapid state of decomposition; and if this take place in a wound, it will certainly interfere for a time with the natural and healthy processes of cure, and may induce erysipelas or pyæmia.

That this decomposition does usually occur is evidenced by the peculiar sickly animal smell which is perceived whenever a wound which has been covered for a few hours is opened. When, on the contrary, a wound has been fairly impregnated with the chloride lotion, there is invariably an absence of any animal smell whatever for two or three days; and, unless some diseased tissue remain in the wound, there may be none throughout the healing. Were this the only advantage, it would be a great one; I believe that in our hospital it has saved many a patient from erysipelas; certainly we have been for the last eight months very free from it after operations, while just before it was very prevalent. But this may be an accidental coincidence merely, and time and experience can alone determine how much is due to the treatment. It is not, however, the only advantage. One of the most striking consequences of the application is the quiescence of the wound. The action which one would imagine must of necessity follow the application of an escharotic so powerful as the chloride of zinc, is never to be seen. The parts, up to the very edges of the wound, retain their natural colour during the early periods after an operation. I can state this confidently after the use of the lotion in varieties of operations, the removal of tumour, amputations, even with extensive and thin flaps, as Syme's and Mackenzie's amputation, operations about the rectum, involving the mucous membrane, and in the perinæum, and in many others, as well as after accidental wounds. In many cases the wounds have healed in twenty-four hours, without the least fulness or swelling, and leaving a line of cicatrix which after a short time could hardly be seen or felt.

Some cases are at this moment under my observation. A week ago, my colleague, Mr. Shaw, performed two operations. One of these was on a strumous subject who had sinuses in the neighbourhood of the heel, running towards one spot. No bone could be felt, but it was thought that there must be some caries keeping up these sinuses. The heel was laid open to its full extent from before backwards through the track of one of these sinuses, but no disease of the bone could be found. The wound was well bathed with a solution of twenty grains to the ounce, and healed by the first intention without pain, swelling, or discoloration.

Another case was one of necrosis of the lower end of the tibia, with sinuses on the outer and inner side of the ankle. On laying open the outer sinus the joint was found to be opened, and the astragalus in part rough and denuded of cartilage, which still covered some portion of the articular surface. In the situation of the lower end of the tibia was a large cavity, communicating with the medullary cavity, in which lay several pieces of necrosed bone. The articular surface was gone. A portion of the shell of this cavity was removed, and the necrosed bone taken away. Here, by Mr. Shaw's permission, I used the forty grain solution, sponging out thoroughly and repeatedly with it the cavity of the bone and all the tissues surrounding the ankle-joint: up to the present time the parts have been perfectly free from swelling, pain, or redness. There has been scarcely any discharge, and what there is is entirely free from smell. The tissues about the inner ankle, which were very much infiltrated, and more than an inch thick, were also freely laid open, and gaped widely. They were put together with sutures; they did not unite, but were found next day to be covered with a thin layer of slough, which speedily peeled off, and left a healthy granulating surface below. There has been throughout an entire absence of all inflammatory action. In such a case as this, there would, I believe, have certainly been a foul discharge for a little time at least after the operation, had the lotion not been used. The tendency to form a superficial layer of slough is seen in every case in which the skin is thus infiltrated from long-continued disease. It is due, probably, to this low state of vitality of the part. The slough separates very quickly, a healthy granulation covers the surface, and there is no inflammation of the adjacent texture. Another and very striking case is now under the care of Mr. Nunn. He removed the head of the femur below the trochanters from a woman with caries of the hip. She is suffering also from considerable disease of the liver, and her general health is much impaired. The acetabulum was perforated, and the finger could be passed into a cavity beyond; but there was no carious bone to be felt. The lotion was very freely used. The operation has only been done three weeks, but the parts are quite covered with healthy granulation, and the discharge has almost entirely ceased. But an unhealthy inflammation has taken place in the opposite groin, over a large resonant swelling, which appears to be connected with some perforation of the bowel. In this case the lotion was injected for some days into the cavity caused by the operation.

This singular absence of action in the wound is, as one might suppose, accompanied by absence of pain. That great pain sometimes attends the immediate application to a sensitive wound need not be mentioned. But this is not lasting. In most cases it subsides in from one to two hours; in some cases it does not occur at all. Where the application is made after an operation done under chloroform, the patient generally remains altogether free from pain. This is particularly the case when morphia has been subcutaneously injected, as Mr. Moore first suggested, immediately the operation is concluded, and before the effects of the chloroform have passed off. After this proceeding the patient often remains calmly asleep for some hours, and wakes entirely free from pain. There seems to be also less tendency to sickness. When, however, the immediate pain of the application has once subsided, the comfort which the patient enjoys is very striking. I have seen cases in which the patients could not tell from their sensations after a couple of hours that any operation had been done. One can explain, perhaps, from observing the action of the chloride, why action and consequent pain should be lessened or altogether prevented. It is quite clear that the chloride of zinc does not act as an irritant beyond the point of contact. If its use is carried so far as to produce an eschar, the eschar will act as an irrriating body, and there will be inflammation and swelling around it; but if applied short of this, and it requires a continued application of the solid chloride to make an eschar in the natural structures, it produces a peculiar pulpy state of tissue, widely different no doubt from the natural tissue; but certainly not eschar, not a charred mass which *must* be removed by the ordinary process of separation below and around it. Were the surface of a large wound converted into an eschar, it could not heal in twenty-four hours. And yet the tissue appears disintegrated; it is rapidly discharged as a creamy exudation, leaving the parts below perfectly natural in appearance. This can easily be seen when the lotion is applied to an open wound. In this creamy surface must be involved all the sensitive and vasculo-motor nerves, and their function must be arrested. With many escharotics, as the actual cautery, the acids, &c., the irritating effect is propagated along the nerves beyond the point of contact. Although the nerves exposed on the surface may be destroyed, the effect is carried beyond this point, increased nerve action and inflammation is the result. It is not so with the chloride of zinc. From whatever cause it may be, the action terminates at the point of contact. The sensitive surface is destroyed, but no irritation is set up beyond the part directly acted upon. Hence there is no pain, no vascular action, no inflammation. Whether this is or is not the true explanation, the fact certainly is as I have stated it.

Of course I do not mean to assert that all this immunity will be found in every case; but I can safely say that I have never seen it otherwise, and that, as a rule, I have never seen the general run of cases go through so favourable a course as since I used this application.

The mode of application has been described in connexion with the first case in which I tried the solution. The first effect is always to stimulate the small vessels and cause a general oozing of blood from surfaces which had been previously dry. The blood becomes pink and creamy by contact with the chloride. On further application of the solution the whole surface is softened and assumes the same pink colour. The blood will continue to ooze out as long as the solution is applied, and for a short time after. Every part should be well saturated with the chloride—the edges of the skin, the adipose tissue, the spaces between the muscles, the medullary cavity, or cancellated structure of bone. At first I was afraid to touch bone with it, but I find now that no harm comes of doing so. The surgeon need not hesitate to apply it even to thin and delicate structures. Those cases have done best in which it has been most thoroughly used. For wounded surfaces I generally use a solution of from thirty to forty grains to the ounce of water.

An objection has been raised to the use of the chloride that union by the first intention must be prevented, and that we are therefore throwing away a chance of obtaining the best and safest mode of cure after operation. Entire union by the first intention is, however, rarely, if ever, seen after operations of any magnitude. Parts of the wound may so heal; and were the objection valid at all, it would be simply on a question of more or less.

Supposing, moreover, that in one case in twenty entire union by the first intention did take place (I am still speaking, of course, of large operations), surely it would be well to forego the chances of that one, if we could ensure the safer and better healing of the remaining nineteen.

The objection, however, is really not valid. A wound will heal entirely in twenty-four hours when the solution has been freely used. It may do so in less time, possibly, but I have certainly found it healed at the end of that time. And, as a rule, I have found wounds heal more rapidly since I have used the application. After what I have seen of its effect, I should have no hesitation in using it in a plastic operation in which, of all cases, one most desires kind and early healing.

Surgeons have, as is well known, tried various applications in the hope of preventing or lessening suppuration, or of warding off the evils which produce the large proportion of deaths after operations, such as erysipelas, pyæmia, and osteo-myelitis. Of these applications, the principal are strong alcohol, either pure, or holding in solution astringent or aromatic substances, or iodine. Alcohol and some of the aromatic tinctures I have tried; but though they act very beneficially in unhealthy wounds, I have not found that they protect recent ones for any length of time from unhealthy action; nor do they seem to lessen inflammation or promote union. Aloes enjoyed a very high reputation for many centuries for its healing qualities. When applied externally, it was said to hasten cicatrization and repress hæmorrhage. I have often used it in the form of the compound tincture both to recent wounds and to old ulcers. In the latter, and especially in bad sores, it often does great good; but I never found any special benefit from its use in recent wounds. So long as it remains in contact with the wound, it prevents decomposition—as the spirit itself would—and it perhaps has a locally tonic effect on the tissues, but it requires frequent renewal, which could not be managed in a wound which it is desired to heal rapidly. The application of tincture of iodine in recent wounds was recently proposed by M. Pétrequin.[1] Finding that suppuration was never present when this fluid had been injected into cavities or into the tissues, he recommended and practised its application to wounds made in the removal of tumours, especially when situated in the face or neck, where it is very desirable to prevent the formation of scars. Whether it has been tried by others I do not know; but for many years I have been in the habit of adopting a treatment which was suggested to me by Mr. Hoffman, late of Margate, the introduction of small tents covered with the iodide of starch into strumous glands in the neck, even before suppuration has occurred in them, the effect has been very satisfactory; but consi-

[1] Gaz. Méd. de Paris., Dec. 10, 1864.

derable pain and inflammatory action have followed the treatment. Mr. Higginbottom has long recommended the application of a solution of nitrate of silver in recent wounds as well as in erysipelas, as a means of checking any tendency to suppuration. I have not tried it, nor do I know whether it has been generally useful. I doubt, from what I have seen of its action in slight wounds, whether nitrate of silver could be applied as freely as the chloride of zinc; or whether, if so used, it would preserve the parts in a healthy state for so long a time.

We find, too, that Mr. Butcher advises the use of the actual cautery in some operations. He writes: "I have found its application in this way"—quickly brushed over the surface—"most serviceable after excision of the upper jaw, and other severe measures, in arresting unhealthy forms of inflammation, diffuse or erysipelatous, and arousing at once a reparative inflammation."

I have met with nothing which acts in so unmistakably beneficial a manner as the chloride of zinc.

But there is nothing new under the sun. My colleague, Mr. Shaw, was speaking recently to Dr. Brown, of Sloane-street, on the subject of the use at the Middlesex of the chloride lotion, who told him that the subject was not new to him, as he had in early life seen the use of Burnett's fluid in recent wounds. Dr. Brown has kindly favoured me with the following account:

"When I was a young disciple of that most worthy chief of the medical department of Portsmouth Dockyard, James Henderson, a tank was erected in the ropery for saturating yarns, &c., in Sir William Burnett's fluid. Now, it was the duty of every man in the whole establishment to come and report at the surgery the slightest hurt he might receive in the course of his work; but soon after this tank was set up we found on several occasions that men in the ropery meeting with slight wounds and abrasions did not so report themselves, and upon being remonstrated with, they said they had gone directly to the tank and applied some 'solution' to the part, and it always got well very quickly. Practically, the men employed at the tank found if they had cuts about their fingers that they healed surely and speedily through dabbling in the solution, and the fact soon spread amongst their fellows. This presently so attracted the attention of my excellent and observant friend, that we had Sir W. Burnett's solution brought into the surgery to be used in surgical treatment, and I am almost sure that at some time between 1842 and 1844 it was specially referred to in a Report to the Director-General of the Navy."

The Director-General of the Navy was so good as to have the reports of these years inspected; but he informs me that no mention of the external use of Burnett's fluid is to be found in them.

PART FOURTH.

Chronicle of Medical Science

(CHIEFLY FOREIGN AND CONTEMPORARY).

HALF-YEARLY REPORT ON MICROLOGY.

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PART I.—PHYSIOLOGICAL MICROLOGY.

Cell Membrane.—Professor Henle, in considering some recent works on this subject, says Reichert adopts the cell membrane despite Schultze and Bruecke, but he appears to me to go too far in the use of analogy when he requires that what has been made certain in a few cases, or even only in a single one, should be promoted, as a rule, for similar bodies. I rather believe that we must first, in investigating special cases, describe how far the homogeneity extends, and inasmuch as the presence of a cell membrane is to be shown certainly according to certain principles, the negative observations likewise are justifiable. They show that the membrane is not of the essential attributes of the cell, which again must not be understood, as it was explained in Schultze's treatise on muscle corpuscles, as if the membrane, where it existed, were an indifferent or even impeding component part indicating decrepidity of the cell. Schultze now modifies this decision by saying: "The form of the thesis which I put forth unmistakeably as one of which the contents do not absolutely and necessarily follow from what precedes it, but perhaps might only be defended as by a caprice, ought to have restrained my opponent from announcing it as exactly the point of my reformatory efforts, and from rallying against it the whole force of the prevailing views." But we are so much the more to be excused if we took that thesis seriously and as the point of the reformatory effort, because just in it lay the novelty of Schultze's view. For it has been long known that beside vesiculiform cells with liquid contents there are some consisting of little lumps without envelopes, of firm, soft substance, enclosing a nucleus, and that cells of the latter kind may amalgamate without having ever been separated from each other by a membrane; long ago the cell membrane had come to be generally considered as a condensation of the outer layer. Whether we would call this hardened layer cell membrane, primordial sac, or skin layer of the protoplasma, is a dispute about words. The essential difference between the Schleiden-Schwann and the present cell theory is that, according to the former, the cell membrane was primitive and definitive for the cell contents, that it was deposited around a nucleus, and by the substances which entered was gradually extended, whilst we now consider the cell contents as the primitive, the membrane as a secondary condensation, produced by separation of the cell substance (of the protoplasma) into firmer and liquid substances. If this be a reform, then we must own that it was quietly accomplished more than twenty years ago. We have guarded ourselves, however, against exaggeration in extending the idea of the cell to the connecting, granular, or fluid contents of the interstices of the fibrous tissues, because here and there in these interstices a nucleus is embedded.

As incontrovertible proofs of the existence of a separate cell membrane Reichert had brought forward the behaviour of the blood corpuscles in diluted nitric acid, and the duplicatures on the surface of the furrowing globules of the egg of the frog, before described by him. M. Schultze and von Vintschgau offer another explanation. As the division of the cells, so, according to Schultze, the cleavings of the egg also generally depend on a contractibility of the protoplasma, in this case of the yolk. The contraction proceeds from a determinate spot of the surface of the yolk, and thence gradually (not suddenly, as Remak stated) extends, splitting the yolk into two halves. However, inasmuch as the substance of the exterior and of the nucleus of the yolk are of different consistence, so also the degree of contractibility might be different on the periphery and in the interior, and hence we cannot be surprised that on the contraction of the whole mass the surface is disposed in folds. Von Vintschgau considers this folding of the outer protoplasma layer to be simply a consequence of the formation of furrows; in order to sink down into the furrow (which, however, according to von Vintschgau, takes place almost instantaneously) the so-called layer must lay itself out in folds, which again disappear when the furrow becomes deeper and the protoplasma adapts itself to the increased surface. The ovo-cellular membrane on the inner surface of the yolk membrane, as well as the membrane of the furrowing globules described by Remak, the author considers as a product of the coagulation of the outer protoplasma layer by the hardening mixture (of alcohol, wood-vinegar, and sulphate of copper in water) of which Remak made use. As further arguments against the membrane of the furrowing globules, von Vintschgau says that indentations, which, by the electric current, he has produced in them, coalesce when they come in contact, that diluted sulphuric acid and acetic acid gradually destroy the formations without leaving any membrane, or bursting, and so forth.

According to Bruch, the cells of the germinal membrane of the bird's egg are unenveloped before incubation, by which the cell membrane becomes distinct. To the yolk element the author certainly ascribes a membrane-like envelope, but says they are not to be taken for cells in the sense of the Schwann theory, but as belonging to the category of the drop-like albumen formations and enveloping globules analogous to the formations which in fishes and batrachia assume a crystalline form.—*Zeitschrift für rationelle Medicin*, 3rd Series, vol. xxii. pp. 6–8.

Laminar Membrane of the Placenta.—M. Joulin denies that the membrane on the fœtal aspect of the placenta, after having removed the amnion, and in the thickness of which the chief divisions of the cord pass, is the chorion. It differs from the latter histologically as well as in its situation in the placenta. The histological elements of the laminar membrane are altogether distinct from those of the chorion. They are composed of bundles of laminar fibres in parallel layers, sometimes crossed, of amorphous matter and some fatty granules. In the membrane are found neither the nuclei nor the molecular granules, the foundation of the chorion tissue. The laminar membrane may be separated into two distinct layers, between which the vessels leaving the cord subdivide. The superficial layer is always extremely thin; it adheres but little to the vessels, and amalgamates with the deep layer at the root of the cord and at the circumference of the placenta. The deep layer is a little thicker and less tough, and at certain limited parts, especially between the larger divisions of the knots, it has a thickness of one centimetre. It is very adherent to the vessels, and accompanying them forms sheaths in the thickness of the organ. This layer at the circumference of the placenta gives off lamellar tracts, which pass between the villosities. The laminar membrane is altogether devoid of proper vessels. The author also makes new observations on the distribution of the placental vessels, on the mode of insertion and direction of the villosities, and on their connexions with the maternal circulation.—*Archives Générales de Médicine*, July, 1865, pp. 22 and 105.

Lachrymal Canal.—Professor Henle compares its lining membrane to the cavernous structure of the urethra and vagina, thus keeping "the canal closed with gentle force," generally sufficient to keep air from passing by it from the nose into the lachrymal sac. He says: "In the wall of the membranous lachrymal canal there is found a venous network, which, in reference to the number and size of the venous branches, and the relative delicacy of the intermediate little branches, may undoubtedly be likened to the tissue of the cavernous bodies of the genitals and of the urinary apparatus. It is a continuation of the cavernous tissue of the inferior turbinated bone; it decreases from the mouth upwards in importance, and generally has completely disappeared below the transition of the lachrymal canal into the lachrymal sac. In the lower part of the lachrymal canal the so-called fibrous layer is 0·5—1·5 mm. thick; of this only a thin layer nearest to the bony wall is shown, the composition peculiar to the periosteum, out of fine connective tissue bundles parallel with the axis of the canal, in the intervals of which are branching out in a similar direction numerous likewise very fine elastic fibres. As for the rest, venous plexuses with longitudinally prolonged meshes, the lacunæ of which are occupied by isolated arteries, by proportionably numerous and stout nerve trunks, and moreover by a connective tissue profusely intermixed with elastic fibres, form the principal part of the membrane. The diameter of the (uninjected) veins amounts to 0·6, the diameter of the stouter arterial trunks to 0·1 mm.—*Zeitschrift für rationelle Medicin*, vol. xxiii. 1865, p. 264.

Normal Bone Marrow.—M. Charles Robin remarks that the structure of marrow consists simply of a juxtaposition of anatomical elements of the form of cells, with interposition of a small quantity of amorphous matter, of which the proportion varies in the different kinds of marrow. Capillary vessels are also met with in this tissue. Their meshes are about two or three times the diameter of the capillary vessels around them, as happens in those which are called tissues relatively rich in vessels; they are about of equal dimension in all senses. They are polygonal, with rounded angles, whilst in other tissues, in which the meshes are polygonal, we almost always find distinct angles, acute or obtuse, which give an appearance altogether peculiar to the capillary arrangement—quite different to the meshwork, of which the angles are rounded as in marrow. These meshes are not more numerous in contact with the bone than at the other parts of the tissue; but when an injection of marrow, especially of the fatty marrow, is studied, in order to find a meshwork easy to observe and not hidden by fat, it is almost always necessary to place the marrow in a current of water, and then the capillary vessels become flattened and superposed against the bony tissue. Therefore it has been said that the meshes were much more numerous in contact with the bone than elsewhere. The finest capillaries that are found in marrow show also this peculiarity, that they are larger than the ultimate capillaries of the periosteal and osseous network. Especially in the spongy tissue, and even in contact with the bone, they are not exactly cylindrical like those of the periosteum, and seem like sinuses moulded upon the neighbouring parts to which their walls are applied; this gives to injections of them a peculiar appearance as compared with the other tissues.

The marrow is in immediate contact with the osseous substance; and the pretended internal periosteum, which has been made to play so active a part in the nutrition of the bones, does not exist at all, even in the long bones. But there is an arrangement of fine laminated fibres crossing each other in all directions. These fibres are connected with the adventitious tunic of the vessels, and sometimes with the inner aspect of the bony trabeculæ.

In this arrangement of fine laminated fibres the fibres are generally alone, crossed or not. About the bony trabeculæ which traverse some parts of the medullary canal of the long bones, these fibres are closer together than elsewhere, but without forming proper layers or fibres. But even in the marrow, here and there, these fibres are arranged in wavy bundles, and are little compacted together. From these bundles radiate, in various and very elegant arrangements, fibres, isolated, fine, wavy, crossed in some parts; and among them are the medullary cells, amorphous matter, which in some parts exists alone, capillaries, and adipose vesicles.—*Gazette Médicale de Paris*, Feb. 4th, 1865, p. 68.

The fœtal or red marrow owes its colour to the fact of its being composed in great part of medullary cells, with a small quantity of amorphous matter. The medullary cells, which form about eight-tenths of the whole of the tissue, with the vessels, as well as can be calculated, are such as are described in the typical condition—that is, without the addition of little fatty drops in their thickness. The accumulation of these medullary cells with vessels and a small quantity of amorphous matter, goes to make up a reddish mass. The marrow, thus constituted, is little by little, through phases of development, replaced by a greyish, semi-transparent, gelatiniform marrow. This change of colour, this passage of the first into the second variety, is owing to an increase, in certain conditions, of the amorphous substance between the medullary cells in a manner disproportionate to the medullary cells; so that in the gelatiniform marrow one finds medullary cells separated from each other by a great quantity of a homogeneous, semi-transparent substance, of a gelatinous appearance.

This gelatiniform marrow has sometimes a greyish and at others a yellowish, almost semi-transparent colour. When greyish, it is from the absence of fat cells between the other elements; and if yellowish, it is that the separated medullary cells are accompanied by fat vesicles.

We have seen above, indeed, that it is by passing into the state of fat vesicles of the laminar fibres yet remaining in the state of fusiform or stellar fibro-plastic bodies in the fibrillary framework of the marrow, that the latter assumes the so-called fatty state. In this case the amorphous matter disappears, and the fibrils of the framework as well as the medullary cells are repelled and compressed between the adipose vesicles, which makes them hardly to be seen. But in the cases of the marrow, as a consequence of emaciation, passing into the gelatiniform state; or when, by inflammation, or from the presence of a tumour of the bone, or in the medullary canal, it returns to the red condition, the amorphous matter reappears, and the medullary cells again become visible, or even become more numerous than they were. There is a great difference between the texture of the marrow, thus become rich in fatty vesicles, and the adipose tissue. In the former the cells are simply placed together with medullary cells interposed, and in some places with amorphous matter still remaining. But the adipose cells are not here disposed in lobules separated by partitions of laminar fibres, as in the adipose tissues. Besides, the size and form of the capillary meshwork are unlike. The clammy consistence, the delicacy of tissue, and the greater facility with which the fatty vesicles may be broken, are quite unlike adipose tissue. The change to the adipose state of the fibro-plastic bodies of the fibrillary framework of the marrow takes place, especially in some bones, by preference. It is noteworthy, also, that the vascularity is relatively less in the fatty marrow than in the gelatiniform coloured marrow, or in that of a red colour.—*Ibid.*, Feb. 18th, 1865, p. 105.

Primitive Kidneys of Man and Mammals.—Professor Dursy of Tübingen, as part of a larger subject of investigation, communicates a few preliminary observations made by him on the kidney of a bovine fœtus. It was situated in the hindermost part of the abdominal cavity, covered by the Wolffian body, and attached to the lower end of the posterior lip of the primitive kidney. With a magnifying power the blind ends, like globular swellings, of the little urinary channels, could be seen shining through the envelope like little white knobs. After treatment, for several hours, with weak hydrochloric acid, he observed the ureter with the adjoining parenchymatous tissue of the kidney, and isolated the very short and comparatively thick canaliculi. The structure of the kidney was very apparent. The little trunks of the urinary canaliculi going off from the ureter furnish short, straight, or of greater length, winding, lateral, and terminal branches, which end in considerable ball or pear-shaped swellings. The latter, solid formations consisting of cells, evidently represent at once the foundation of the glomerule and of the capsule. In several of these knobs also had already begun the process of differentiation of bloodvessels and capsule, already distinguishable by the appearance of a reddish yellow colour in the interior of the knob, and caused by the blood. In some places these end knobs were placed, without a neck, immediately sideways or in the angle of division of a stouter canaliculus; here first, also, originated from the remoter winding urinary canaliculi, the solid first foundation of the capsule and of the glomerule. Hitherto, in regard to the mammalian embryo, he has not been able to convince himself of a subsequent inversion of the blind end of a urinary canaliculus by a glomerulus coming from another side. More precise statements, also, on the glomeruli of birds, the investigation of which now occupies him, he will publish later. He has made these preliminary observations only to show that in mammalian embryonal kidneys of early degrees of development, the connexion of the capsule-bearing urinary canaliculi with the chief trunks opening into the ureter may be certainly shown, and indeed in uninterrupted connexion.

With regard to the distribution of the bloodvessels of the Wolffian body, the hilus serves as an entrance of the numerous arterial trunks, which, dividing shrub-like, form as it were the nucleus of the primitive kidney, embraced semicircularly by the loop canaliculi, as well as the glomeruli situated in the front lip. In connexion therewith is a vascular plexus surrounding the blind little tubes; finally, larger vessels, which might be called vasa recta, running parallel, develop themselves out of it, which always pass regularly between two superficial little tubes towards the issue-passage. There they sink into a considerable vein, which in the whole length of the Wolffian body moves down upon the issue-passage close beside the Müller thread, simulating an issue-passage. The latter, however, lies under it, is with difficulty to be rendered visible, and even in cross sections is not always positively to be recognised, inasmuch as its delicate transparent coats collapse.

Besides this important venous trunk, which, having been injected, goes beyond the lower as well as the upper end of the primitive kidney, and represents the former vena cardinalis, there are still found in the gland envelope numerous large arborescent venous branches, which, however, do not open into the vena cardinalis, but diverge in an opposite direction median-wards, and unite into larger trunks passing down into the inferior cava. Whether there are also deeper venous trunks, which then would take their way through the hilus, he has not yet been able to discover.

On the nature of the upper end of the Wolffian body, which, in the male sex, goes to form the head of the epididymis, his observations are incomplete. But he rather supposes that the Wolffian body has two kinds of canaliculi—viz., urinary canaliculi, with capsuled ends, enclosing a glomerulus ; and seminal canaliculi, with simple ends, joining the uppermost embryonal gland end, which perhaps only appear later with the first appearance of the embryonal gland.

By comparison of the Wolffian bodies of both sexes in the stage of the retroformation, as well as by examination of the fully developed parovarium, he is able to explain the parovarium as the remains of the urine-excreting section of the Wolffian body, furnished with glomerule and capsule ; hence it is not the analogue of the epididymis which in the female, perhaps, was not even existing in the primitive formation ; and it is, perhaps, more correct to compare it with the organ of Giraldès. He reserves a more detailed representation.

He would observe, in conclusion, that Müller's thread has nothing at all to do at its first development with the Wolffian passage ; it is a string-shaped formation arising in the peritoneal envelope of the primitive kidney, like the embryonal gland, which also at first appears only as a string forming itself within the peritoneal envelope. Moreover, both appear simultaneously. On the other hand, the Wolffian passage does not, as is commonly assumed, lie at all within the envelope but beneath it ; it belongs to the gland tissue proper. Yet it is true that beside the Müller thread in the envelope there is a considerable canal, but this is the above-mentioned cardinal vein, which covers the Wolffian passage. This vein has the same bearing to the envelope as a sinus ; hence it stands open in cross section, and thus is easily confounded with the issue-passage of the primitive kidney, which is visible, but with difficulty, and for the most part collapsed.

Also, the generally assumed statement, that the Müller thread suddenly appeared *in its entire length*, is completely erroneous. It first appears on the top of the Wolffian body, and here indeed in connexion with the upper string-like end of the embryonal gland.—*Zeitschrift für rationelle Medicin*, vol. xxiii. p. 257.

Organs unprovided with Nerves, Lymphatics, or Capillaries.—Dr. Simpson, of Edinburgh, has a paper on this peculiarity of the human (mammalian) umbilical cord and placenta. He infers that—1. The volume of the umbilical cord and fœtal portion of the placenta is formed of nucleated cellular tissue, traversed by the tubes of the umbilical arteries and veins, and their numerous placental subdivisions; and the cord and fœtal surface of the placenta are covered by a sheath of serous or seroïd membrane. 2. In the composition of these parts no capillaries, vasa vasorum, lymphatics, nor nerves are found to enter.—*Medical Times and Gazette*, Oct. 29th, 1864, pp. 459, 460.

Reproduction of Parts of Animals.—M. Gross says—"If one cuts off the foot of a salamander, the next day the wound is covered with epidermis cells; two days after the amputation, an amorphous substance is poured out between the epidermic layer and the solution of continuity, and in five or six days this amorphous substance, semi-transparent, has pushed off the epidermis, and formed a kind of cap at the end of the member." The author says that the phases of the new foot resemble those through which the member removed had gone in its growth. "In the semi-transparent substance which is found between the epidermis and the solution of continuity, is instantly recognised an amorphous substance and granular matter, besides embryo-plastic nuclei; these nuclei, much larger than in the human subject, always precede the reproduction, as they precede formation of tissues in the embryo.

"From the fifth day after the amputation nuclei are found, which change into fusiform bodies, and, soon after, laminar fibres appear. Between the tenth and the fifteenth days the nuclei of cartilage are to be seen near the bone which has been divided. About the twentieth, a cartilaginous cone, continuous with the old bone, is found; the end of the bone is inserted in the cartilage; the reproduction of the cartilage proceeds gradually to the extremity of the member, of which it seems to guide the development; in fact, it precedes the formation of the muscles and nerves. The cartilaginous skeleton of the part is at first of a single piece, though the forms are already pretty plainly seen; the articulations appear little by little, beginning where the reproduction has begun. In a month and a half the articulations are formed, and ossification commences.

"The development of the muscles is easily followed; nuclei in rows produce the new myolemma, which is perfectly continuous with the old, and is plainly visible by about the fifteenth day; after a month, fresh nuclei appear within the myolemma to produce the muscular fibres.

"The nerves are thus developed: in the early days following the amputation the ends of the divided nerves undergo fatty degeneration, but not to any great extent; after ten days rather lengthened nuclei are seen close together at the sides and in front of the stump; these nuclei, surrounded by an amorphous yellowish substance, form a kind of fusiform body, whose slender extremities reunite to form the fibres of Remak, which soon become nerve-tubes."

The author, further on, says that "the conditions necessary for animal reproduction are contrary to the union by first intention, and *vice versâ*."—*Gazette Médicale de Paris,* Sept. 24th, 1864, pp. 598, 599.

Part II.—Pathological Micrology.

Fat in the Muscles.—MM. Ch. Robin and Reynal have been examining various kinds of butcher's meat, and in all fat meat, they say, the fattening resulted from the multiplication of the adipose vesicles in the partitions of the cellular or laminar tissue placed between the secondary bundles; vesicles disposed either in rows or prisms placed according to the muscular bundles, or in yellowish layers surrounding these latter, and in some sort causing the disappearance of the laminar tissue in the presence of the fatty tissue. One never finds adipose vesicles among the striated or primitive bundles, but only in the partitions separating the secondary bundles which form these primitive bundles.

The vesicles, newly created, of which fattening results, are otherwise like, taken individually, to those which are found in small quantity in the same partitions in animals not fattened. But, considered altogether as adipose tissue, the tissue of fattening which they form differs from the natural subcutaneous or interposed adipose tissue in that they are not, as in the latter, disposed in polyhedral or lenticular lobules, separated and surrounded by proper laminar tissue and capillaries. They are accumulated in a homogeneous way, without subdivision of layers or fatty rows into these above-mentioned lobules.—*Gazette Médicale de Paris,* April 1st, 1865, p. 202.

Reunion of Divided Nerves.—That this may be immediate, by the aid of a suture, has been instanced by M. Langin; but that it is so, and that the function of the peripheral end of the nerve is restored, are denied by Drs. Eulenburg and Landois, who have made twenty experiments for this purpose on animals. "The minute examination of the peripheral end showed, as time progressed, all the alterations accompanying the degeneration of a nerve separated from its centre—viz., coagulation of the marrow, and gradual fatty change, whilst the fibres of the central end offered no anomaly. The cylinder axis was often found to participate in a characteristic manner in the general degeneration, whilst that of the central end showed none of these changes. The substance between the two ends was made up of cellular tissue more or less mixed with a granular mass (of detritus), and of remains of extravasations. The isolated ends of the nerve fibres were sometimes swollen."—*Gazette Médicale de Paris,* March, 1865, pp. 170–1.

Pus Cell.—Dr. Collis says: "Pus affords another example of the low vitality of the lymph cell. Pus cells are only lymph cells which have perished, and which, in parting with most of their nitrogenous element, exhibit a superabundance of oily granules in their interior. This is the true reading of the multiple nucleus of the pus cells; it is not, as Virchow and Lionel Beale would have it, a spontaneous and vital division of the nucleus, preparatory to a multiplication of the cell by division; it is only a step towards the disintegration of the cell, and an evidence of the loss of its life in its very centre and most vital part."—*On Cancer and Analogous Tumours: Edinburgh Medical Journal*, March, 1865, p. 831.

Contractile Cells in the Pus of Primary Syphilitic Pustules.—Dr. Szabadfoldy, of Pesth, punctured two of these pustules which had formed within a few hours on the glans penis; they were small, transparent, itching, and surrounded by a bright areola. The contents of each, with a power of 300—350 diameters, were found to be composed, to about a third part, of cells rounded or with various prolongations. The latter were seen to change form; the rounded became oval, and the appendages appeared and disappeared. In some they became so numerous that one might have thought them ciliated. Some resembled the cells of cancerous tumours, but of these some were larger and their contours paler. A molecular movement was observed in the contents of many of the cells. In three or four minutes the movements observed in the different elements slackened.—*Archives Générales de Médecine*, November, 1864, p. 611.

Molluscum Contagiosum.—A case has lately occurred in Professor Ebert's clinique, in which a little girl had 107 of these tumours about her face. Their contents were epidermis cells, covered by a layer of large cells devoid of nuclei. Professor Virchow considers that these tumours were certainly contagious, because another child in the next bed in the hospital had several of them afterwards developed. He says that their name is by many, and chiefly by English authors, used indiscriminately for all sorts of little tumours, more especially for small encysted atheromata with excessive formation of connective tissue. The apertures communicated with a number of long canals, which were filled with a mass consisting of an exterior layer of epidermoïd cells, and an inner portion which looked like fat. In the same way we notice in cancroïd tumour of the lower lip, after ulceration has commenced, deep fissures and holes which lead into canals and tunnels. But there appeared this difference between the two, that the molluscum nodes proceeded directly from the follicles, and there was only production from the surface, but the parenchyma was nowhere affected; while, on the other hand, cancroïd tumour is a parenchymatous formation which can only secondarily proceed outwards, after the skin has been broken, and ulceration commenced. The mass which filled the centres of these molluscum canals, consisted chiefly of round, small, shining corpuscles, which lay closely together, and looked like heaps of cells filled with fat. But when they were more carefully examined, it appeared that they did not consist of fat, but of albumen. These corpuscles are formations which also occur in cancroïd tumour, which is notoriously infecting. By reason of their very small size they are peculiarly suited to penetrate into the smallest openings of the skin, and to be smeared, as it were, into the hair-follicles, where these are somewhat open. Professor Virchow has, therefore, endeavoured to find out whether phenomena of motion may be observed in them. They have the habitus of cells, which in other places are found to be contractile; but he has never as yet observed anything else but imbibition of liquids, and consequent change of shape in the former, but no spontaneous motion.—*Medical Times and Gazette*, March 25th, 1865, p. 317.

Encysted Epithelial Tumours. By Dr. A. Lücke, of Berlin.—In this essay the author considers some cases of which it may be doubted how they should be classified. The following case is first given: A woman, aged forty, had many years observed two small tumours under the skin—one in the neck, left of the vertebral column, the other under the right angle of the jaw; both had increased gradually in size, had become very hard, and then remained stationary. B. Langenbeck shelled them out of the subcutaneous areolar tissue; they were moveable, and hard as bone, and he considered them to be calcified atheromata. They had not reappeared. Being sawn through, we distinctly perceived two different substances—a yellow, brilliant trabecular tissue, alternating with spaces more or less isolated, which contained a white, partly friable mass. The larger tumour presented in its middle a larger space, filled with white mass; in the other the white mass was predominant, and by this its somewhat brittle consistence was explained. The white mass contained in the larger space of the former consisted of a quantity of epithelial cells, some provided with distinct nuclei, some chalky, some fatty, free chalk-granules, and a few cholesterine tablets. Fine slices of the tumour showed a connected trabecular system of true bone-substance, with developed bone-corpuscles, Haversian canals, and sparse marrow spaces. The bone-tissue formed alveolæ, in which a dull, yellow-transparent mass was embedded. If the latter was treated for decalcifying with dilute hydrochloric acid it was seen to consist of masses of epithelial cells with large nuclei, which as flattened epithelial cells were deposited close together, or if isolated, presented an hexagonal or other polygonal form. The epithelial cells were very dull, which partly depended upon an impregnation with lime-salts; however, they could not be completely cleared up either by treatment with hydrochloric acid or with ether or chloroform. The cyst-membrane consisted of solid fibrous connective tissue, was poor in bloodvessels, and was easily detached from the proper tumour. The tumour had no shell of bone—that is, its exterior rough surface consisted partly of osseous substance, partly of epithelial spaces.

The author believes it depended on a development of epithelial centres in the subcutaneous connective tissue; by this the alveolar structure would be explained; then, in a tolerably advanced stage, the epithelial cells are said to become chalky, the trabecular connective tissue to ossify, and the whole tumour to be separated from its capsule.

In a second series the author relates the following case, which much resembles the first described:

In a woman forty years old a tumour had, within a few years developed slowly and painfully on the posterior boundary of the right deltoïd. It had attained the size of a hen's-egg. It was situated under unchanged skin, was moveable, and gave a deceptive feeling of fluctuation. It was taken to be an atheromatous cyst, and it could be easily shelled out. The wound soon healed, and the author had heard of no return of the growth. The tumour was enclosed in a tolerably solid connective-tissue sac; on section, no atheroma was seen, but tolerably consistent contents, in which two distinct kinds of tissue could at once be distinguished; a white friable mass appearing in many small centres imbedded in a bluish white, jelly-like stroma. The white masses consisted of chalky and fatty nucleated epithelial cells, which here and there contained microscopic epidermic beads. The stroma was a soft young connective tissue, which in mass outweighed the epithelial-cells, and contained a tolerable number of nuclei and more developed spindle-cells, but, towards the capsule, passing into a solid fibrous tissue.—*Schmidt's Jahrbücher*, vol. cxxv. 1865, p. 167.

Cancer.—Concerning the diagnosis of cancer, Dr. Collis concludes that the nearer the constituent cells of a tumour approach to the healthy lymph-cell in form and power of development, the more clinically benign is the tumour; the farther they are removed in these two particulars from the healthy type, the more destructive or malignant is the growth. To this he would add the further observation, that tumours of rapid growth, and with a tendency to recur, have round or oval cells, which are rapidly reproduced, and have small powers of development in the direction of fibres, while the more chronic tumours, as a rule, are composed of cells which have more or less tendency to form fibre.—*Dublin Quarterly Journal of Medical Science*, February, 1865, p. 110.

Mr. Collis also says that the cancer-cell is a large, soft, weak, and delicate cell, and probably lighter—in other words, that there is the same amount of material in the larger cancer-cell as in the smaller lymph-cell, and that hence encephaloïd cancers are light as compared with fibrinous tumours.—*Ibid.*, August, 1865, p. 200.

HALF-YEARLY REPORT ON MATERIA MEDICA AND THERAPEUTICS.

By Robert Hunter Semple, M.D.

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On the Use of Sulphate of Aniline in Chorea.—In 1862 Dr. Fraser treated five cases of chorea in the London Hospital with the sulphate of aniline, but without success. Since that time he has employed the same agent in two more cases; and although in one of them there were two relapses, a cure seems to have followed on the third occasion of treatment. The first patient referred to was in the hospital for a month in each of the two previous attacks, and had been discharged cured, and he appeared for the third time in January, 1863. He was ordered to take as a draught five grains of sulphate of aniline with ten minims of dilute sulphuric acid and an ounce of water three times a day. The effects of the aniline could be observed about half an hour after its administration by the mauve colour of the mucous membrane of the mouth, and the nails showed a similar effect at the same time. The medicine caused some headache, giddiness, and vomiting, and its use was occasionally discontinued and then resumed, but after treatment for about a month the choreic movements ceased, and the patient was again discharged cured. The amount of aniline taken was 290 grains. In the second case the alkaline treatment had been tried unsuccessfully, and the use of aniline was resorted to, the dose being the same as in the last case. After about a month's treatment this patient also recovered, the quantity of aniline taken being 315 grains. The medicine produced no bad effects except a slight headache, which became gradually less in severity until it disappeared entirely. In this case also the mauve colour appeared on the mucous membrane.—*Medical Times and Gazette*, Aug. 5th, 1865.

On the Treatment of Articular Rheumatism and other Affections by the Sub-cutaneous Injection of Sulphate of Quinia.—The question which M. Dodenil proposed to himself in making some investigations upon the hypodermic use of quinia, resolved itself into two points—namely, whether it is possible to introduce this substance beneath the skin without inconvenience, in certain cases where the absorption by the digestive canal is insufficient or mischievous, and what relation can be established between the doses usually employed internally and those which ought to be injected, in order to obtain almost identical physiological results? To these propositions he replies, that although an acid solution such as that of quinine cannot be introduced beneath the skin with impunity, yet the inconvenience is trifling, and notwithstanding the employment of large doses, the local disturbance is slight and infrequent; and with regard to the dose, he administers the sulphate in the proportion of half a gramme (about 7½ grains) to two grammes in twenty-four hours. The instrument he employs is a small, graduated glass syringe, adapted to a perforated needle; and the seat of the injections he has varied without any unfavourable effects, the punctures being generally made on the sides of the vertebral column, and sometimes on the thighs and arms. M. Dodenil has observed, that in the cases where the medicine has been administered beneath the skin, the cure has been at least as rapid as when it was given in the usual manner; but it must be remarked in favour of the injections, that almost all the patients subjected to their employment were in the worst conditions. They all presented disturbance of the digestive system, which would have been increased by the administration of the sulphate by the mouth; several presented phenomena of intolerance, and vomited the drug; and some had had rheumatic complications of the greatest intensity. After describing the particulars of eight cases of rheumatism treated in the abovementioned manner, M. Dodenil remarks, that rheumatic patients are not the only subjects who derive benefit from this mode of treatment. He has successfully employed the same plan in cases of gastralgia complicated with periodical febrile or nervous symptoms, and also in the symptomatic fever of tubercular consumption.

When the sulphate is given after the commencement of feverish excitement the lowering of the pulse is produced with a certain degree of regularity three quarters of an hour after the injection; and this appears to be the time necessary for the introduction of the medicine in sufficient quantity into the circulation.

In the treatment of rheumatism it is necessary to employ rather large doses, but in intermittent fever the doses may be somewhat less. The injection of a large dose has a double advantage, for in the first place the physiological and therapeutical effects are definite; and, in the second, absorption is more rapid, and the economy is sooner saturated.

From the researches he has instituted on this subject M. Dodenil draws the following general conclusions: 1. In articular rheumatism, as well as in other diseases in which its efficacy is established, the sulphate of quinia may be administered by the hypodermic method without serious inconvenience, and with some advantages that may be readily appreciated. 2. Those who have hitherto employed this drug in subcutaneous injection appear to M. Dodenil to have employed insufficient doses, which circumstance explains the absence of physiological phenomena in some of the published details. 3. It is necessary to introduce beneath the skin a dose above the half, and nearly equal to two-thirds of that which is given by the mouth, in order to obtain almost identical effects. 4. Absorption is more rapid, and elimination is more prolonged where a large dose is employed; and 5. The greatest advantages of the hypodermic method so applied are the rapidity and certainty of its action, and the immunity secured for the digestive canal. This last result is important, for the healthy condition of the stomach allows the patients to take food early, and shortens the period of convalescence.—*Bulletin Général de Thérapeutique*, August 15th, 1865.

On the Employment of Oil of Petroleum in Prurigo.—As physician to a charitable medical institution in one of the most unhealthy parts of Rouen, Dr. Bellencontre has frequently observed cases of prurigo, in which disease he has often met with success from the use of oil of petroleum after other means had failed. He relates the case of a woman about forty years old, who was suffering from prurigo in a very aggravated form, and for whom he prescribed lotions containing oil of petroleum, and baths. In three days the pruritus had diminished, but the application of the oil of petroleum, which at first had only caused a slight smarting, had become painful, and he therefore ordered the petroleum in combination with oil of sweet almonds and a little laudanum, under the use of which the patient completely recovered. In another case, where general prurigo had existed for more than a year, and had resisted the local action of ointments containing calomel, tar, oil of cade, and cyanide of potassium, the oil of petroleum was recommended in the form of lotion, and in ten days effected a cure. Dr. Bellencontre states that the petroleum appears to him to succeed best in the prurigo attended with moderate itching (*prurigo mitis*): in the *prurigo senilis*, where there are also frequently pediculi on the skin, he has employed the remedy once only, but with complete success. The first application of this oil occasionally produces some redness of the skin and an increase of the itching, but it must not, therefore, be discontinued; the applications should then be made at more distant intervals, or the oil should be combined with oil of sweet almonds or olive-oil and laudanum. The oil of petroleum should never be rubbed into the skin, as it might cause inflammation, and thus retard the cure; but it should be employed in the form of lotion or ointment.—*Bulletin Général de Thérapeutique*, July 15, 1865.

On the Employment of Blisters in Rheumatic Fever.—Dr. Herbert Davies, who last year proposed to treat acute rheumatism by the application of blisters, and brought forward a few illustrative cases in support of the plan, now adduces the leading features of nearly fifty cases, the great majority of which have been under his own personal superintendence. He advocates this method of treatment on various grounds, and in the first place he states that, if adopted early and before any physical signs of endo-pericarditis are developed, it undoubtedly, in a large majority of cases, saves the heart from inflammatory mischief. In the fifty cases described, the results of the blister treatment were most remarkable and satisfactory, for as many as twenty-five of them, when discharged, were totally free from any endo- or pericardial mischief, and two recent cases of cardiac inflammation were apparently cured by the alteration effected in the blood by the free withdrawal of the serum from the poisoned joints. But to obtain such results it was necessary to treat the cases early, and to blister simultaneously every affected joint, or rather its proximity. Dr. Davies also states that by this treatment relief of the local pains is rendered rapid and permanent, the diminution of pain almost always commencing as soon as the blisters begin to draw; that the temperature of the body falls, the pulse is reduced in rapidity, and the reaction of the urine is altered; that convalescence is soon established; and that the application of the blisters is not remarkably painful. Dr. Davies, while advocating the blister-treatment, does not think it incompatible with the treatment by alkalies, and he does not doubt that the latter would be a useful adjunct. As a practical point, he remarks that good acetum lyttæ, *such as can be fully depended upon*, well painted in zones round the joints, and close to the inflamed parts, produces much less pain than is caused by the usual mode of blistering by strips of the emplastrum lyttæ.—*Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital*, vol. ii., 1865.

On the Application of Caustics in Phlegmonous Erysipelas.—Although cauterisation has formerly been employed in cellulo-cutaneous erysipelas, the method adopted by M. Long, a surgeon-in-chief of the French navy, presents some novel features. Three cases out of several have been recorded in illustration of this mode of practice. The first case was that of a brewer, whose right arm was swollen, painful, of a dusky red, and covered with livid vesications, the redness and swelling extending from the fingers to the axilla. There was deep-seated fluctuation, with fever, dilatation of the pupils, and a soft and regular pulse. Beef-tea was given with a mixture containing acetate of ammonia. Vienna paste was applied over thirty spots about half an inch in diameter in four longitudinal rows, and the limb was wrapped in cotton. The next day, as the patient was worse, fifty other applications of the escharotic were made over the whole surface of the arm, and more beef-tea was given. After this the delirium and fever ceased, and suppuration was fully established, the appetite was quite restored, and solid food was allowed. But the skin was extensively detached, and flakes of mortified areolar tissue were removed behind the elbow. Gradually, however, the condition of the arm was improved, and finally the wounds healed, and motion of the limb was restored. The two other cases presented an analogous condition of the skin and subjacent tissue, and were treated in the same manner with success. M. Long's object in this treatment is to arrest the mischief, prevent detachment of the skin, and give exit to the pus. Sixty or eighty cauterisations over a limb in the earlier stage of the disease, produce powerful counter-irritation, and in most instances the general condition of the patient becomes improved in twenty-four hours, the swelling diminishes, and the suppuration becomes limited. This plan may be adopted in traumatic erysipelas of the head. It is said that only two out of sixty cases thus treated terminated fatally. The application of the Vienna paste leaves indelible scars, but this disadvantage is considered unimportant considering the formidable nature of the disease.—*Journal de Médecine et de Chirurgie Pratiques*, August, 1865.

On the Efficacy of Lemon-Juice in Diphtheria.—Lemon-juice has for some time been recommended in the treatment of diphtheria. M.

Guersant was in the habit of prescribing to his patients slices of lemon to be kept in the mouth and frequently renewed; but this was only an auxiliary measure, being used together with cauterisations and astringent injections. But in a paper presented to the Academy of Medicine last June, Dr. Révillout contends that lemon-juice is one of the most efficacious medicines which can be applied to diphtheria, and he relates that when he was a dresser, his own life was saved by its timely application. He employed three dozen lemons and gargled his throat with the juice, swallowing a little at the same time, in order to act on the more deep-seated parts. The results were that the false membranes were detached, the glandular enlargement decreased, and recovery soon followed. M. Révillout has noted eleven cases of entire success attained by this method of treatment. In one of his cases, which may probably be regarded as a type of the others, he prescribed as a gargle the juice of four lemons every hour, and the patient was directed to swallow a portion so as to modify the condition of the pharynx and œsophagus. The effects were to detach the diphtheritic exudations and to reduce the glandular enlargements, and the swelling of the face; and in twenty-four hours after the commencement of the treatment all appearance of diphtheria was removed.—*Journal de Médecine et de Chirurgie Pratiques*, June, 1865.

On the Use of the Bromides of Potassium, Cadmium, and Ammonium in the Treatment of Insanity.—The experiments related in this paper were instituted by Dr. Belgrave, of the Lincolnshire County Asylum, chiefly to ascertain the therapeutic value of the bromides in the treatment of general paralysis, and they seem to show the possibility of controlling some of the more serious occasional manifestations of that disease, as also similar symptoms in other forms of mental disorder. Fourteen cases of general paralysis were treated with the bromides of potassium and ammonium with variable results; but the general effect of the bromides appeared to be to tranquillize the system. Eleven cases of epilepsy were also subjected to treatment by the bromides, the patients being selected in consequence of their extreme irritability and proneness to violence. The results did not prove that these drugs have the power to diminish permanently the number or the severity of the fits, but they afforded evidence of their influence in allaying some of the most violent manifestations of the disease. The bromide of cadmium was tried in eleven cases of mania, with a view to relieve severe temporary excitement, and in doses of one grain it was found to exert a very rapid and powerful effect, causing abundant vomiting and some purging, diminution in the force of the pulse, and temporary mental quietude, bordering on depression. Dr. Belgrave concludes, from his observations, that the bromide of potassium is antiphlogistic, and a sedative to the cerebro-spinal functions; that it subdues the force of the pulse and induces loss of flesh and debility, and that it exercises a powerful temporary effect over the number of fits in epileptic cases. The bromide of ammonium resembles the bromide of potassium in its action, but is less powerful, and does not induce emaciation or general depression. The bromide of cadmium is probably an irritant to the mucous membrane of the alimentary canal, its brief but marked calmative effect being principally the depression following the action of a powerful emetic and purgative. The action of this drug resembles that of tartar-emetic or sulphate of zinc, but it has treble the power of the former and twelve times that of the latter (*sic*). It is said by Dr. Belgrave to be exceedingly useful in severe exacerbations of mania.—*Journal of Mental Science*, October, 1865.

On a Case of Hystero-Epilepsy in a little Girl, treated successfully by the Bromide of Potassium.—A little girl, aged ten, had been subject for three years to attacks of a serious but anomalous character, consisting in sudden starting from sleep during the night, with a violent shriek and inexplicable terror, but without the other usual symptoms of epilepsy. After the attack was over, the child remained dull and bewildered, not being able to answer questions, and soon sinking into a deep sleep, but without snoring. These attacks subsequently appeared in the day as well as the night, and were accompanied by strange and wayward conduct, the digestive functions languished, the appetite was lost, and the general strength was very much reduced. The administration of iron and quinine, and of valerian in infusion, produced some temporary alleviation, but the fits afterwards increased in frequency and intensity, and at last hardly a fortnight elapsed without an attack; and in addition to the other phenomena, a dangerous one was added, consisting in a propensity on the part of the child to precipitate herself from any open outlet, and which nearly cost her her life on one occasion when she was near an open window. She became a patient under the care of M. Blache, and having been kept without treatment for a few days in order to ascertain the reality of the alleged attacks, she was ordered to take a mixture containing 10 grammes (about ʒijss) of bromide of potassium in 100 grammes of water; two teaspoonfuls the first day, two dessert-spoonfuls the second, and two table-spoonfuls the third, half an hour after food. Under this treatment the attacks entirely ceased, and the child left the hospital; but the treatment, which had been discontinued for a week, was to be resumed and continued in order to guard against future attacks.—*Bulletin Général de Thérapeutique*, Dec. 30th, 1864.

On the Successful Employment of Syrup of Belladonna, Opium, and Chloroform in a Case of Traumatic Tetanus.—In the case referred to it is admitted that no distinct conclusion can be drawn as to the relative efficacy of the different medicines employed, although it is suggested that the result was chiefly due to the combination of opium with chloroform. The patient was a young man of good constitution, who had accidentally received a contused wound in his left hand by the discharge of a pistol loaded only with powder and wadding. The wound was treated by ordinary remedies, and was healing, when the patient caught cold and was seized with trismus, preceded by a little stiffness in the posterior cervical muscles: there was a very slight space between the jaws at the incisors. He was recommended to rest in bed, and to take syrup of belladonna in successive doses of a teaspoonful every two hours. After this treatment had been pursued for two or three days, it was found that the trismus was not increased, but there was stiffness in the dorsal and abdominal muscles, and chloroform was administered twice in the same day so as to produce complete anæsthesia. The next day there was a marked improvement, and the separation of the jaws was more easily effected. The chloroform was administered three times a day, each period of anæsthesia being followed by abundant sweating. The stiffness of the dorsal and abdominal muscles continued to be very well marked, and there was also so much constriction of the pharynx that the patient had great difficulty in swallowing some soup. Under the continued use of chloroform the jaws became almost completely relaxed, but the state of the back, the abdomen, and the pharynx remained the same. An opium pill (containing a very minute quantity of opium) was given every two hours, without, however, discontinuing the administration of the chloroform, which was repeated two or three times a day. After some days a dose of quinine was added to the opium, and under this treatment all the symptoms were relieved except the stiffness of the abdominal muscles, which, however, was at last also removed.—*Journal de Médecine de Poitiers*, 1864.

On the Physiological Operation of the Preparations of Iron.—Dr. A. Sasse, after making some remarks on the astringent properties of the preparations of iron, used both externally and internally, proceeds to discuss their chemical operation on the body. He considers them to be, like the blood-corpuscles, carriers of ozone, and therefore capable of replacing the corpuscles. These bodies change the oxygen taken into the blood by respiration into ozone, and the common oxygen of the atmosphere is just as useful for breathing as so much nitrogen; it must first be decomposed into the negative electric oxygen or ozone, and the positive electric oxygen or antozone, and the former operates as an immediately oxidizing agent. The ferruginous preparations only share with the blood-corpuscles this property of changing common oxygen into ozone, of giving off this ozone to substances inclined to oxidation, and of again taking up oxygen, until all are decomposed or eliminated from the body. The iodide of potassium paper is coloured blue when brought in contact with a solution of a persalt of iron, or with blood mixed with water. From this similarity in reaction, Schönbein has concluded that peroxide of iron is composed of protoxide of iron and ozone. Ferruginous preparations resemble the blood-corpuscles also in this respect—that they change the positive electric antozone into ozone. Since the preparations of iron act altogether like the blood-corpuscles in relation to oxygen, ozone, and antozone, it will readily be understood that the former, when carried into the blood, may replace the loss of the corpuscles. It matters little whether the iron is taken into the blood in substance, or as an oxide, or as a proto- or persalt; for when it has once entered the circulation, it continually takes up and gives off ozone, and thus becomes alternately a proto- and a persalt united, until it is expelled from the body.

It may perhaps be asked whether in the living body the ferruginous preparations act as oxidizing agents. Such is probably the case, and Botkin has shown that iron, even in healthy persons, raises the temperature above the normal standard. This elevation of temperature often occurs rapidly, and at the same time more urea is excreted, these phenomena all showing an increased oxidation. The same conclusion may be drawn from the similarity of the appearances observed (by Demarquay and Leconte) from the breathing of oxygen and from the operation of iron. Peroxide of hydrogen also, which contains antozone and has lately been recommended for the promotion of tissue-metamorphosis, may be compared with the preparations of iron, since antozone can be easily converted into ozone. Since also iron increases the temperature of the blood, it stimulates the action of the heart.

Thus, concludes Dr. Sasse, we have in iron a means of improving the tone of the system and promoting the process of oxidation. As this metal, therefore, favours the nutrition and the activity of the body, it is contra-indicated when the process of oxidation is already too energetic, as in inflammatory diseases, or when an increased flow of blood is apprehended. In tuberculosis of the lungs, according to the author, iron ought to be avoided, notwithstanding some recent recommendations to the contrary.—*Ned. Tijdschr. v. Geneesk.*, Dec. 1864; *and Schmidt's Jahrbücher*, May, 1865.

On Two New Specific Remedies for Gonorrhœa.—After adverting to the circumstance that the two specifics at present employed in gonorrhœa are often unsuccessful, Dr. Henderson proceeds to notice two others which he thinks are likely to prove serviceable in the treatment of the disease. The first is the oil of yellow sandal-wood, obtained by distillation from the wood of the *Sirium Myrtifolium*, of the order of Santalaceæ, a tree growing in the East Indies. In Dr. Henderson's experience with this oil he has found it perfectly innocuous even in large doses. The formula he employs is twenty to forty minims of the oil three times a day in three parts of rectified spirits, and flavoured with oil of cassia or oil of cinnamon. In cases of gonorrhœa in the first, second, or third stage, in susceptible persons, he has seen the most marked suppression of the discharge within forty-eight hours. The oil has the great advantage of being pleasant to the taste, and not liable to cause sickness. Dr. Henderson has frequently succeeded with it when balsam of copaiba and cubebs have been fairly tried and have failed. Its action on the urethra is generally observed, in susceptible cases, within a few days after beginning its use. The oil is commonly kept for perfumery purposes by the druggists, and is therefore easily to be procured.

The other remedy is the Gurjun or Gurgina balsam, or wood-oil. It is the product of the *Dipterocarpus turbinatus*, a large tree growing in different parts of India, and one tree is said to yield about forty gallons in a season. When distilled with water it yields 35 per cent. of volatile oil. The Gurgina balsam is a liquid of the consistence of olive-oil, of a dark reddish colour, and slight odour. Dr. Henderson was induced to employ this medicine from seeing a notice of it in Mr. Waring's book on Therapeutics, the latter author stating that Dr. O'Shaughnessy employed it in numerous cases of gonorrhœa and gleet, and found it nearly equally efficacious with copaiba. Dr. Henderson has tried it for several years, and has used it only in cases where copaiba had been fully tried and failed. In every case it was successful within a week, and no symptoms of inconvenience were in any instance produced. He gave it in what may be termed large doses—namely, a teaspoonful two or three times a day, and uncombined. Dr. Henderson was unable to investigate its action very fully, as his supply became exhausted, and it was not easy to procure it in this country. He is thoroughly convinced that it is an excellent medicine, and that it is possible to obtain it abundantly at a moderate price, if the dealers would undertake the importation. Several specimens were exhibited at the International Exhibition in London in 1862.—*Glasgow Medical Journal*, April, 1865.

On the Use of Sternutatories in the Treatment of some kinds of Epilepsy, Dementia, and other Chronic Head Affections.—Although sternutatories have not lately been used in the treatment of epilepsy, they were formerly employed in several chronic affections of the head, and the *pulvis asari compositus* of a former Pharmacopœia was the last of various formulæ which were formerly used for these diseases in ancient and mediæval medicine. Some years ago Dr. Laycock, when reflecting on the probable cause of epilepsy, and more particularly of the sensorial and mental symptoms as distinguished from the convulsive and motor, was led to the conclusion that stimulants to the nostrils would be found useful in certain cases by warding off the attack, by shortening the duration of the comatose condition, and by diminishing the susceptibility to returns. He had the opportunity of trying such stimulants on some insane epileptics in a private lunatic asylum near Edinburgh, and he afterwards extended the trial to some cases of headache, loss of memory, and hallucinations, in which he had reasons to think that the circulation and state of nutrition in the encephalon were disordered.

Before relating the particulars of the cases in which the above plan was adopted, Dr. Laycock offers some general remarks on the pathology of epilepsy, and he is led to conclude that the proximate cause of an epileptic paroxysm is a sudden cessation of the circulation through the encephalon, or some portion of it, and of the proper metamorphosis of the cerebral or encephalic tissue, in so far as it is the seat of consciousness. If these conditions could be obviated, the fit would be arrested or limited. Dr. Laycock also believes that there is some regulating centre for the circulation and nutrition of the encephalon, and that in epileptic paroxysms this regulating centre is disturbed in its functions, so that if it is desired to prevent a fit of epilepsy it is necessary to act on this centre, which Dr. Laycock believes to be the cerebellum. But the last-named part is itself acted upon by the medulla oblongata from below and by the cerebrum from above, and by influencing the medulla oblongata, the cerebellum is also affected. Now the best plan of arousing a person from deep sleep, syncope or dreaming, is by acting on the medulla oblongata by means which excite that part of the nervous system through the afferent nerves of the respiratory system, and more particularly the nasal branches of the fifth pair. Dr. Laycock, in short, was led to the conclusion that irritants applied to the nostrils would be the best means of exciting the activity of the medulla oblongata, and through it of the cerebellum, in cases of epilepsy. He, therefore, first experimented with strong liquid ammonia and snuff.

The results were not uniform in all the cases, ordinary snuff producing no effect in some, but ammonia seeming to be generally more beneficial. Dr. Laycock then suggested that the snuff should be made more irritating by drugs, and given regularly. The powder of the root of white hellebore and cayenne pepper were accordingly used for the purpose, and the results on the whole were satisfactory, the snuff appearing to keep off the fit, while the strong liquid ammonia brought the patients out of it. Other substances were subsequently used as snuffs—namely, asarum, hellebore, and arnica, but they generally failed to produce sneezing, and the good effects were not so well marked as when sneezing resulted, but still they were "sufficiently hopeful."

Since the irritation of the nostrils by irritant drugs is curative, mechanical irritation may be substituted for them, and Dr. Laycock has accordingly, in some slight cases of epilepsy, tried irritation of the nostrils with a feather or quill. A young lady had an attack of the *petit mal* when seated at table, and Dr. Laycock instantly irritated the nostrils mechanically by scratching them, and in a few seconds she was restored. On a previous occasion a similar attack had passed into convulsions. A young gentleman had an attack similar to that of the young lady, and the fit was ended by Dr. Laycock in the same manner. That the irritation is the effective element in the treatment is further shown by the fact that the *throat* may be irritated with advantage in other paroxysmal affections, as, for instance, hysteria, in which Dr. Laycock has removed the fit by tickling the throat with a feather so as to excite retching.—*Medical Times and Gazette*, May 6th, 1865.

On the Comparative Merits of Ether and Chloroform as Anæsthetics.—Dr. Lente, who is a warm advocate for the use of ether as an anæsthetic, commences his paper by inquiring whether it is justifiable to use a remedy which has undeniably killed its hundreds, when we have an equally efficient one which, with ordinary care, is perfectly safe? As regards the proofs of the efficacy of ether, he refers to hospital experience in America, and also in Italy, where no other anæsthetic is employed, and where immunity from pain is as perfect as elsewhere; and he especially calls attention to the statistics obtained in the Mill Creek Military Hospital, Fortress Munroe, where the results were so successful as to convince some of the ablest civil and military surgeons and physicians in the United States. He regards this fact as settled—viz., that a patient may be brought under the influence of ether as quickly as he can safely be under that of chloroform, and with a quantity costing less, and weighing but little more. Dr. Lente argues, that although due care may be used, no human skill can enable us to foresee or prevent the dangers of chloroform; and he mentions the case of a young colonel of one of the American regiments, who died recently of chloroform, administered for the performance of some trifling operation. The editor of the 'New York Medical Journal' joins in the opinions of Dr. Lente, believing that his views are tenable, and that their correctness will be fully substantiated by future experience. He goes on to state that he has reason to believe that death from chloroform has occurred in several instances in the American military hospitals, and in surgical operations on the field of battle, where every precaution that skill and care could suggest was employed to avert the catastrophe. He believes the truth to be, that chloroform is a dangerous agent, and will cause death in a certain proportion of cases in which it is employed in spite of all precautions. He leans to Dr. Lente's opinion, that ether is perfectly safe—a view which is entertained by most of the American hospital surgeons.—*New York Medical Journal*, April, 1865.

On Anthracite as a Remedial Agent.—Dr. A. Dyes discovered the therapeutical properties of anthracite by accident. He remarked that pigs greedily devoured some anthracite lying in their styes, and especially after they had eaten a full meal. Experiments and observations subsequently made convinced him that this kind of coal, like common salt, but in a much greater degree, promoted digestion and fattening, so that it was especially serviceable in catarrh of the stomach and intestines, and in colic. He afterwards employed anthracite in the human subject with beneficial results in the following complaints:—1. In intestinal worms, but generally combined with other anthelmintics. 2. In spasm of the stomach, caused by catarrh of that organ, or by affections of the liver or spleen. 3. In chlorosis, in which it even surpasses iron, although in cases of retention of the menses he recommends moderate abstraction of blood from the inside of the thigh. 4. In tumours of the spleen, during and after intermittent fever. 5. In scurvy. In this disease, Dr. Dyes found iron, anthracite and other remedies, efficacious only after the teeth of the patients had been thoroughly cleaned from the adhering salts of lime which often cause bleeding of the gums, and other anæmic and scorbutic conditions. Sailors often mix gunpowder with their food in order to protect themselves from scurvy, and Dr. Dyes thinks that it might be useful to mix anthracite daily with the food on a sea voyage. 6. Rachitis. In this disease anthracite is more beneficial, and oppresses the stomach much less than ferruginous preparations. 7. In scrofulous complaints, in which Dr. Dyes places anthracite by the side of iron, sulphur, and rhubarb. Generally speaking, anthracite is beneficial in all cases in which sulphurous waters, springing from anthracite earths, and having the same constituents, are useful, bnt the action of the latter is disproportionately weaker.—*Schmidt's Jahrbücher*, February, 1865.

On the Physiological Action of Iodide of Potassium and its Use in the Treatment of Syphilis. By DR. JOUBIN.—In addition to the sense of heat and dryness in the mouth and throat produced by the administration of iodide of potassium, this drug often produces an eruption of acne on the skin, and Professor Küss has called attention to the fact that it causes a limited infiltration of the sub-cutaneous areolar tissue in almost all the regions of the body. Another sign, and one which indicates the saturation of the body with the iodide, is a greyish coating of the tongue. It appears to be a stimulating medicine exercising a special action on the areolar tissue in the affections over which it exercises control, that is to say, if the doses are increased so as to produce the phenomena in question. Dr. Joubin, adopting the views of Professor Küss in reference to the treatment of syphilis, admits two kinds of this disease founded upon its manifestation in two different tissues. Syphilis, whatever may be its intensity, always affects either the epithelium or the connective areolar tissue, and for each of these forms of disease there is a corresponding medicine; which acts, not on the syphilitic virus, but on its manifestation, whether in the areolar tissue or in the epithelium. Of the two medicines employed in syphilis, iodide of potassium acts especially upon the areolar tissue, and thus, according to Dr. Joubin, its efficacy is very extensive in this disease, and is not confined to the treatment of tertiary symptoms. The writer also considers that the indurated chancre does not differ in any respect from the other manifestations of syphilis, and that it ought to be amenable to the action of iodide of potassium. Whether, however, the iodide has the power of controlling the ulterior course of syphilis, as well as of checking the progress of the primary sore, he has not yet satisfactorily determined, but, on theoretical grounds, he thinks that such would be the result.—*Bulletin Général de Thérapeutique*, September 30, 1865.

HALF-YEARLY REPORT ON PATHOLOGY AND PRINCIPLES AND PRACTICE OF MEDICINE.

By Francis C. Webb, M.D., F.L.S.,

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The Animalcular Theory of Malignant Pustule.—The following is an abstract of M. Davaine's paper "On Malignant Pustule," lately read before the Academy of Sciences:

"The relations of malignant pustule in man with carbuncular affections in animals have been now recognised for some time. We know that this pustule has for its determining cause the introduction under the epidermis of the blood of an animal affected with carbuncle.

"But if carbuncle has for its essential element those filiform infusoria which the author has named *bactéridies*, these infusoria ought also to constitute an element of malignant pustule. The absence, according to the author, of *bactéridies* in the carbuncular pustule in man, would be the disproof of the effect attributed to these corpuscles in the production of carbuncle, whilst their presence would be its confirmation. In a communication made to the Academy in the month of September, 1864, M. Raimbert and the author reported a fact confirmative of this relation between malignant pustule and carbuncle. The pustule examined enclosed a large number of *bactéridies*, in every respect resembling those which are found in the blood of carbuncular animals. The author now brings forward other facts confirmatory of his theory. He is indebted to M. Mauvezin of Bray-sur-Seine for an opportunity of examining two cases of malignant pustule treated by ablation of the tumour, and subsequent cauterization. The pustules submitted to examination had been extirpated on the second and third day of their development, and placed immediately afterwards in a solution of chromic acid. The specimens were thus hardened and conserved. By slightly cutting and the employment of caustic potash the elements of the skin were disintegrated and dissolved, and the *bactéridies* were exposed. They occupied the centre of the pustule, and were seated in the *rete mucosum* beneath the superficial layer of epidermis; they were not uniformly distributed, but formed groups or islets separated by groups of normal epithelial cells. In each group of *bactéridies* these little bodies existed by thousands, constituting a very compact layer. In the centre of these groups no other element could be distinguished, but towards their circumference the *bactéridies* were more or less mixed and interspersed with epithelial cells—or, rather, they formed between the cells trains which connected them with neighbouring groups of *bactéridies*. No other pathological element existed in the pustules. In the deep layers of the dermis, the fat cells all contained crystals of margarine, but the same fact was observed in other cases. To sum up, in the malignant pustule, on the third day of its development, the *bactéridies* formed the essential and unique element of the tumour."

The author continues: "We know that malignant pustule is at first a local affection, whose progress we can arrest by ablation or cauterization, but that after it has existed two or three days it becomes general, and is then beyond the resources of medicine. But the anatomical constitution of the pustule thoroughly explains the succession of these phenomena. In reality, we see that the *bactéridies* are developed in the epidermic layers of the skin structures which do not contain vessels. Whilst thus isolated from the rest of the economy, their destruction may prevent their ulterior propagation. But if their development be not arrested at this point they soon meet the superficial layers of the dermis, which are abundantly provided with blood-vessels and lymphatics; they introduce themselves into these vessels, and, carried along in the circulating fluid, they infect the rest of the economy. He then gives the particulars of a fatal case of malignant pustule which occurred at the Hôtel Dieu. The man died on June 6th, and the autopsy was made on the 8th. In blood taken from the heart and examined microscopically by M. Lancereaux, a large number of *bactéridies* were discovered. A drop of the blood, sent to the author some hours after the autopsy, contained a large number. The author inoculated a healthy guinea-pig with a small drop of this blood by four punctures. The animal died two days afterwards, and its blood exhibited the *bactéridies* in considerable numbers."

We subjoin the objection urged by M. Jules Guérin to M. Davaine's observations. It appears, from the experiments of MM. Leplat and Jaillard, communicated to the Academy of Sciences by M. Pasteur, that they could produce *bactéridies* at will by the putrefaction of blood or flesh, and that these *bactéridies* do not exercise a septic action unless they are accompanied by a certain quantity of putrefied liquid. The observations of M. Davaine do not absolutely weaken the objection which results from these experiments—that is to say, that the presence of *bactéridies* in malignant pustule is a circumstance occurring after the development of the malady. This touches the great question of spontaneous generation, of which the *bactéridies* of malignant pustule may be only a particular instance. It is not, then, *on the third day* from the development of the malady that the fluid furnished by malignant pustule should be examined, but the fluid of the central vesicle at its first appearance should be made the subject of examination, it being first proved that this fluid is capable of reproducing the malady.—*Researches on the Nature and Anatomical Constitution of the Malignant Pustule by M. C. Davaine, read before the Académie des Sciences: Gazette Médicale de Paris,* July 1st, 1865.)

The Recent Epidemic in St. Petersburg.—Dr. J. Millar, who visited St. Petersburg during the recent epidemic, attributes it to the following causes:—Over-crowding, from the influx of the agricultural population who annually repair to St. Petersburg in the winter for the purpose of obtaining work; last winter the number so doing was much increased in consequence of the recent ukase liberating the serfs; a severe Russian winter, resulting in scarcity of food, which was also made more than usually dear by a recent regulation of Government ordering all cattle to be slaughtered at a distance from the city; the long fast of Lent, kept scrupulously by the Greek church; and the admixture of a considerable proportion of spurred rye in the black bread ordinarily in use. Three forms of fever were prevalent—typhus, typhoïd, and relapsing. Whichever form of fever was ultimately manifested, rapid prostration was an early symptom. The patient often succumbed in the very first stage of the disease. Dr. Millar apparently holds the doctrine of the specific identity of the different forms of fever. He writes: "In those who managed to pass through the first stage, and escaped death from the extreme prostration, the disease manifested itself either as typhus, typhoïd, or relapsing fever. In the typhus the symptoms were almost purely cerebral, and after death a peculiar condition of the dura mater was found. This condition has been described by Dr. Kremiansky. "He found the internal surface of the dura mater, both at the top and sides of the hemispheres of the brain, covered with a thin layer of yellowish deposit, over which were spread light reddish spots, more or less closely grouped together, varying in size from a poppy seed to that of a millet seed, pea, or even larger. When the surface of the dura mater was washed, there remained on its under surface, swimming in the water, a sort of fringe, which consisted of tender, villous, dendroid, reddish excrescences, evidently capillaries produced on the under or vascular layer of the dura mater. Sometimes it was found that also the internal surface of the dura mater presented a turbidness and succulence which gave, on section, a thin, turbid, serous fluid. This fluid was found to contain blood-corpuscles and epithelial cells that had undergone fatty degeneration. It was also observed that considerable hæmorrhage often arose from the ruptured vessels of these tender newly-formed textures, sometimes in considerable quantity, so that the internal surface of the dura mater was lined with a thick layer of coagulated blood, together with the other newly-formed textures. But it was found that when no hæmorrhage occurred, a thin serous exudation took its place, which produced similar compression of the brain." Dr. Kremiansky attributes these lesions to the commencement or acute form of hæmorrhagic inflammation of the dura mater. He found it entirely confined to the cases of typhus and relapsing fever that came under his notice, but believes that it was connected with the common abuse of ardent spirits amongst the population. The symptoms accompanying this condition were pyrexia, severe pain in the head, principally towards the occipital region, restlessness, absence of sleep, followed in from one to three days by convulsions and subdued delirium, which passed gradually into coma, in which the patient died. In connexion with this hæmorrhagic inflammation of the dura mater many cases of cerebro-spinal meningitis were observed. The condition of prostration was succeeded by paroxysms of tetanic convulsions, which increased in severity until the patient became comatose and sank. Many died on the second and third days.

In reference to the cases of typhoïd, Dr. Millar states that they exactly resembled in symptoms and pathological appearances those occurring at home. Some spurious cases, however, were observed, which seem to form a link between fever and cholera, the symptoms of the two diseases being mixed. These cases were always fatal. Buboes, boils, and typhoïd pneumonia were noticed in the more aggravated cases of typhoïd. The symptoms presented by the cases of relapsing fever resembled those described as occurring during the epidemics in Scotland and Ireland. Jaundice was a prominent symptom of the more severe cases.—*Dr. John Millar on the Recent Epidemic in St. Petersburg: Edinb. Med. Journal*, Sept., 1865.

Cholera.—It would be impossible in the space allotted to this report to give notices of all the memoirs and observations on the subject of cholera with which foreign journals teem. This is the less to be regretted as they add but little to our knowledge of the subject. We select, however, the following: 'Cholerine considered as the Period of Incubation of Cholera.'—Under this title M. Jules Guerin has republished, with some additions, a memoir, which first appeared in 1837. The following are the conclusions at which he arrives: 1. That cholera, such as is described by most authors, is constantly preceded by a period of incubation, which he was the first to describe, and to which he gave the name of cholerine. 2. That this period, which lasts from two to eight days, consists of a slight diarrhœa, with a feeling of general malaise, with a tendency to cold perspiration and fainting. 3. That this group of symptoms is due to the epidemic cause, and is the first degree of true cholera. 4. That this first degree, left to itself in places where epidemic cholera reigns, is almost always susceptible of conversion into grave cholera. 5. That the means, *par excellence*, to prevent this conversion are the complete suspension of every species of alimentation from the appearance of the first symptoms of cholerine, and where this precaution has not been sufficiently observed, the use of an emetic of ipecacuanha.—*Gazette Médicale de Paris*, Nov. 4.

Experimental Researches and Observations on Epidemic Cholera.—In a memoir presented to the Academy of Sciences, M. A. Baudrimont asserts the following conclusions: That in cholera the albumen of the blood is transformed into diastase, and is found as diastase in the dejections; that the presence of diastase, and also of a matter analogous to yeast, is remarkable as representing the two products successively formed, at the expense of albuminoid matter, during germination and fermentation; that, as there is a great resemblance between the alvine dejections in cholera and the pancreatic juice, may it not indicate that cholera is due in great part to a hypersecretion of this fluid, and that it is principally by the canal of Wirsung that the choleraic fluids and the matters they hold in solution pass into the intestine?—*Gazette Médicale*, Nov. 25.

MM. Guyon and Cloquet on the Nature of Cholera—In a communication addressed to the Academy of Sciences by M. Guyon, he advocates the existence of a relation between cholera and the sweating sickness, a principal difference being that in the sweat the body is rapidly deprived of fluid by the external surface, in cholera by the internal. In the discussion which followed, M. Cloquet, whilst allowing that the diarrhœa is a copious colliquative intestinal sweat, traced the original action of the poison to the nervous system. His conclusions were, that cholera poison, whatever it be, exerts its influence primitively on the nervous system; 2, that the functional disorders observed in cholera depend on the modifications that the nervous system, under the influence of the morbid principle, impresses on the functions of all the organs dependent on it; 3, that we may hope to combat cholera by therapeutic agents which act on the nervous system in an opposite manner to that of the poison, and which may therefore neutralize its action and annul its consequences.—*Gazette Médicale*, Nov. 18.

Progressive Locomotor Ataxy, following Angina Diphtheritica.—A man, aged thirty-one, who had previously had good health, came under treatment for a deep diphtheritic ulcer of the right tonsil, which caused pain in swallowing, troublesome accumulation of mucus in the throat, and was accompanied with cardialgia, and suddenly occurring paroxysms of suffocation, coming on especially at night. Three weeks after the diphtheritic affection commenced he had a fall from giddiness, and during the next eight days he became very feeble, and impairment of memory, heaviness of the legs, and a dragging, staggering gait came on. He walked better quickly than slowly, and worst in the dusk, as he required to see his feet in order to direct the movements of them. Then sight began to get dim, and after fourteen days everything appeared as if shrouded in a thick mist; but there was neither strabismus nor double vision, nor any drooping of the upper eyelids; the pupils were normal, and the iris freely mobile. Deglutition ceased to be painful, but became difficult, food returning by the nose. The soft palate hung loose, and flapped on deep inspiration. The ulcer in the still swollen tonsil was deep, but clean. Some days later taste was lost for all but sweet things, a solution of quinine not tasting at all bitter. Peculiar pricking and shooting pains affected the middle of the hard palate and the right corner of the mouth. The nape of the neck was sensitive, and the head could not be held up straight, the muscles of the nape having lost power. The giddiness increased, and he became very drowsy. Later the pricking pains affected the ulnar border of the right hand, and the tips of the fourth and fifth fingers, and the sensibility of those parts was diminished. The voice became highly nasal. He could no longer see his feet, and his gait became extremely uncertain and staggering. There was obstinate constipation, and though laxatives were taken daily the bowels were relieved only once in four or even six days.

On the 22nd of September sight returned quite suddenly, after having been lost for a month. Some days later memory was restored, and the giddiness, the drowsiness, and the pricking pains in the gums and corner of the mouth all ceased. But as the cerebral symptoms disappeared, affection of the spinal cord rapidly increased. Pain fixed itself in the upper vertebræ. The sensations of formication, cold, and deadness in the hands and feet changed in fourteen days into complete anæsthesia. Everything felt to the patient as if his hands were covered with woollen gloves. He could not take up small objects, nor hold fast larger things when given him; he could neither write nor convey food to his mouth; but he could distinguish between heat and cold, and the latter seemed to rather increase the sensibility. In the feet and legs up to the knees he felt an icy coldness, and he could only just feel the ground under his feet; he could neither stand nor walk alone, for he felt as if rocking constantly to and fro. Peculiar convulsive movements of the fingers and toes now came on, some being extended, others flexed or abducted, the movements being altogether beyond his control. He could still taste sweet things, but nothing else. To this was added complete insensibility inside the mouth; difficulty of deglutition continued. If in the dusk he folded his hands together, he could never separate them till light was brought so that he could see the positions of the fingers. In the beginning of October the delusive rocking motion ceased, and the patient could no longer, when he put down his feet, feel the ground; he was obliged always to sit or lie; he could not rise up without aid, and when lifted up, his legs would not support him; but while lying he could stretch out the legs with tolerable force; he could give a fair squeeze of the hand, and could swing the arms backwards and forwards, but was not able to lift them up. At this time the dysphagia suddenly quite disappeared, so that the patient could again satisfy his imperious appetite. The impulse to eat came on suddenly, and if he could not quickly satisfy it he grew faint. The swelling and ulceration in the tonsil had disappeared, but the voice remained nasal. In the middle of October the icy coldness of the legs ceased, giving place to an agreeable sensation of warmth. In the latter half of the month, however, the paralysis of the lower half of the body reached its highest degree, and the pain in the back was felt lower down towards the loins. For some days the patient could not feel that he sat, and had no sensation in the genitals. The government of the lower extremities was entirely lost, and when the patient was held up on both sides he dragged his legs after him as if quite inanimate; yet he could, when lying on a sofa with his legs up, stretch them forcibly out. He sat always bowed extremely forwards, and could only raise himself straight for a moment. Œdema of the feet came on; the urine was clear and frothy, but contained neither sugar nor albumen. In the middle of November the above-described symptoms began to gradually disappear, so that in December all the functions were again normal; only some difficulty in writing remained. By the end of the year, however, this had ceased; he was perfectly well, and had gained flesh and strength.

The treatment "did not differ from that usually adopted in such cases." It may be mentioned, however, that the patient through a long period took strychnine—$\frac{1}{20}$th of a grain, cautiously increased to $\frac{1}{12}$th, morning and evening, every twelve days discontinuing its use for four days.—*Dr. M. Jensen: Hospitals Tiende*, 11, 1865; *Schmidt's Jahrb.*, cxxvii., pp. 164–6, 1865.

Tabes Dorsalis and Progressive General Paralysis—In Band xx. of the 'Allg. Ztschr. f. Psychiat.,' Dr. C. Urstphal published three cases in which the symptoms of advanced general paralysis were united to those of tabes dorsalis, in two of which after death he had recognised, by the microscope, grey degeneration of the posterior columns of the spinal cord. Since then he has had the opportunity of examining three cases after death, and has collected several analogous cases of other observers, so that he now reports on ten cases—three of his own, four of Hoffmann's, and one each by Joffe, Frerichs, and Meyer. Of these only two were not examined after death; the remainder presented—partly to the naked eye, partly to the microscope—the characteristic alterations in the spinal cord. In seven cases brain affection appeared early under the form of mental exaltation, even with epileptiform convulsions; in three, on the contrary, as intellectual weakness, coming on gradually, and increasing into the deepest apathetic imbecility; but in the former also imbecility appeared finally.

The disorder of motor power in this combined form presents at a certain stage a close resemblance to the complex symptoms of the so-called general paralysis. It might be very possible, therefore, that, in some cases at least, where the intellectual preceded the motor disorder, the grey degeneration of the posterior columns was secondary to a primary cerebral affection. But pathological anatomy has as yet afforded this theory no support, the opinion of Joffe and Erlenmeyer, who alone have at present declared themselves in favour of a secondary affection of the spinal cord, not being supported by post-mortem examination. On the contrary, that most important symptom in tabes—namely, the dependence in standing and walking on sight—is wanting in the usual picture of general progressive paralysis. It is, therefore, highly probable that the peculiar motor disorder of progressive paralysis does not arise, as in tabes, from disordered or abolished conduction in the nerves of sensibility; the often present dulling of feeling admitting very well of the explanation that the mental dulness of the patient prevents the perception of sensitive impressions; but they could, nevertheless, exercise a regulating influence over the usual order and sequence of movements, and thus make sight superfluous. The true cause of the motor disorder is, however, doubtful.

In seven cases symptoms of paralysis of the tongue were wanting; in two they were present, but not distinctly.—*Allg. Ztschr. f. Psychiatr.* xxi. p. 361; *Schmidt's Jahr.*, B. cxxvi. p. 167, 1865.

Tubercle in the Brain.—Professor Duchek has published, as the first of a series of "Studies of Diseases of the Brain," three cases of tubercle in that organ—

1. *Tubercle in the Pons.*—Besides tuberculosis of various other organs, there was found in a young man a tubercle an inch in diameter in the left half of the pons. The surrounding brain was softened, and there was exudation on the membranes. The symptoms corresponded to the well-known type of the disease. At first, gradually increasing paralysis of the right leg, later of the right arm and left facial nerve, and perhaps of the palpebral branches of the oculo-motor nerve. Sensibility was diminished in the left half of the face, and mastication interfered with through paresis of the masseter muscle. Electrical contractility was weaker in the affected muscles than in the corresponding ones of the opposite side. Pains, spasms, and stiffness, preceded the paralysis in the extremities, paresthesia accompanied it. Consciousness remained untroubled throughout the course of the illness; death followed in a year from its probable commencement.

2. *Tubercle in the Corpus Striatum.*—A child, six years of age, was affected with chorea-like movements, at first in the right half of the face, and these soon spread to the neck, shoulder, arm, and leg of the same side. By degrees the affected parts became weaker, and at length wholly paralysed. Contraction occurred only in the muscles of the nape; sensibility of the skin, and reflex-sensibility, were increased. The perceptive faculties were normal. Micturition very difficult in the daytime, involuntary at night. The intellect was latterly disturbed and clouded. The illness lasted six months. A tubercle, the size of a hazel-nut, was found in the left corpus striatum; and one, the size of a bean, in the upper wall of the fourth ventricle, and many smaller ones in the cortical part of the brain.

Duchek attributes the contraction of the muscles of the nape to the tubercle of the fourth ventricle.

3. *Tubercle in the Cerebral Hemisphere.*—A man, eighteen years of age, who worked very laboriously with the right arm, was suddenly seized with clonic convulsions of it, shortly followed by a like affection of the face and loss of consciousness. At first the convulsions recurred in the same way every fourteen days, later irregularly, even many times a day, but without loss of consciousness. Soon tonic spasms alternated with them. For some months all symptoms of illness disappeared; but then they broke out anew, and seized also on the right foot, appearing for the most part only in one extremity; they also once transiently affected the left foot. Burning pain in the forehead preceded the attacks. Coming on again after a second pause of some months, marked paresis of the right half of the face and of the right extremities was noted. The paroxysms lasted till a short time before death, about two years after the first attack, the patient dying, tuberculous, of marasmus. On post-mortem examination there was found in the left cerebral hemisphere a wedge-shaped tubercle, which, from its broad base at the cortex and attached membranes, extended an inch and a half downwards in the substance of the brain towards the optic thalamus; it was enclosed by a richly vascular membrane; the cerebral substance round it was of a pulpy softness. Elsewhere the

brain was healthy.—*Wien. Ztsch.* (*Med. Jahrb.*), xxii. p. 99; *Schmidt's Jahrb.*, B. cxxvi. p. 284, 1865.

Paralysis of the Palate in Facial Palsy.—Dr. W. R. Sanders observes, that on the subject of unilateral paralysis of the velum palati in connexion with paralysis of the portio dura, the statements of authors are curiously contradictory. Some deny the existence of paralysis of the palate, or believe it to be an accidental coincidence independent of lesion of the facial nerve. The majority, however, recognise the affection, and state that the hemiplegic palate and uvula are, like the features of the face, drawn over to the sound side, whilst others describe no displacement of the palate, and assert that the uvula is not drawn from, but to, the paralysed side. Dr. Sanders finds that the real or principal deviation of the soft palate in palsy of the portio dura is a vertical lowering of the arch on the paralysed side. Although this fact has been noticed by one author, the mechanism of the distortion has never been explained. He relates several cases in which the vertical distortion has been observed, and draws the following conclusions:—1. That in paralysis of the palate due to lesion of the portio dura, the levator palati and azygos uvulæ are the only muscles affected; the other muscles of the palate—viz., the circumflexi, the palati glossi, and palati pharyngei—are not paralysed nor impaired in their actions. 2. That consequently the true form of hemiplegia of the palate, in lesion of the portio dura, is partial, and consists of a vertical relaxation or lowering of the corresponding half of the velum palati, with diminished height and curvature of the posterior palatine arch on the paralysed side. This condition is due to paralysis of the levator palati. 3. That there is reason to doubt the accuracy of the description usually received of the horizontal displacement of the palate and uvula to the sound side when affected in facial palsy. Such deviation would imply paralysis of the circumflexus palati (and probably also of the palato-pharyngeus), which does not receive its nervous supply from the portio dura, and cannot be palsied by its lesion. 4. That the lateral distortion of the palate due to paralysis of the portio dura, which may be expected sometimes to occur, would take place, not transversely, but in the diagonal direction upwards, and to the sound side—i.e., in the line of action of the sound levator palati—and would be accompanied by the lowering of the palatine arch on the paralysed side, as above described. 5. It is probable that the absence of this oblique lateral distortion, both in a state of rest and when the levator palati was in action, in the cases recorded by the author, was due to the minor degree of the paralysis, the balanced action of the unaffected muscles (circumflexi and palato-pharyngei) being sufficient to maintain the raphé of the velum in the mesial line, in spite of the disturbance of equilibrium produced by the weakness (paresis) of one of the levators. 6. The existence of this diagonal distortion upwards to the sound side, in certain cases, may have given rise to the common description of lateral deviation of the palate in facial palsy, although the phenomena presented do not correspond at all accurately with that description. 7. That the position of the uvula varies frequently, both in the natural and the hemiplegic palate, being twisted sometimes to the right, sometimes to the left, the point directed sometimes to the paralysed, sometimes to the sound side. Curvature of the uvula, taken by itself, is, therefore, an uncertain sign, and does not possess the diagnostic importance which has been ascribed to it. 8. If the partial hemiplegia above described, due to paralysis of the levator, be looked for, instead of the lateral displacement from paralysis of all the muscles of one side of the palate, which the common description leads us to expect, but which the author believes never occurs from paralysis of the portio dura alone, it will be found that unilateral paralysis of the velum is by no means so rare as has been generally supposed. 9. That the prognosis is not necessarily rendered more unfavourable in facial palsy when the palate is implicated.—*Edinburgh Medical Journal*, August, 1865.

In the September number of the same journal, Dr. Sanders describes an instance of vertical hemiplegia of the palate which occurred in a patient labouring under diabetes mellitus, and in whom there was no facial palsy or other kind of paralysis. This condition Dr. Sanders proved was not due to malformation, but to hemiplegia. The occurrence in a case of the kind of paralysis of one muscle supplied by a twig of the portio dura, calls to mind the experiments of Bernard on the medulla oblongata, and their bearing on glycogenesis. The occurrence of vertical hemiplegia of the palate in cases of diabetes mellitus, is a subject well worth investigation.

Case of Acute Yellow Atrophy of the Liver.—The patient, aged thirty-five, was in the sixth month of her fifth pregnancy, and was under the influence of depressing circumstances. Her illness was of a fortnight's duration. Her symptoms were vomiting of yellow matter, and afterwards of blood in large quantities; jaundice, drowsiness, and, finally, delirium and coma. Labour commenced shortly before death, and she was delivered of twins when *in articulo mortis*. The following are the chief points of interest presented by the post-mortem examination: The muscles were dry, the blood dark and fluid; all the internal organs were jaundiced; there were patches of extravasation under the visceral layer of the pericardium. "The liver was reduced to one-half its natural size; but except being somewhat flattened, it retained its ordinary form. It weighed 1 lb. 7½ oz. Its surface was not shrivelled. Beneath its capsule and throughout its substance there were numerous ecchymoses, and small patches of an ochre-yellow colour, but the mass of the organ was of a dark reddish-brown hue. The outlines of the lobules were not recognisable. The gall-bladder was contracted, and contained a little grey inspissated mucus. On microscopic examination of a scraping from a cut surface of the liver, a large quantity of débris and fatty matter, with cells in different stages of alteration, some full of oil-globules, and containing a few bright ochre-yellow granules, and others full of dense granular matter, not fatty, were found. All the cells were considerably enlarged, and denser than natural. No natural cell was observed. The amount of bile-pigment found both in the cells and débris was less than is usual in such cases. On examining sections, it was found that the cells in the outer part of the lobules were almost completely destroyed, while those towards the centre were larger, denser, and more opaque than natural, and the amount of oil was greater towards the margins, where the destruction of cells was taking place. The system of vessels and the fibrous stroma of the organ were not destroyed. It was easy to make sections, and the sections were easily washed without their giving way. In the sections a considerable amount of bile pigment was seen scattered about." Neither crystals nor balls of tyrosin or leucine were observed in the liver or in its blood. The spleen was enlarged, soft, and pulpy, and its substance exhibited some points of extravasation. The supra-renal bodies were enlarged and partially disorganized. The kidneys weighed together $10\frac{1}{2}$ oz.; the cortical substance was dense and pale, the capsule stripped off easily. The tubules, both straight and convoluted, were filled with dark matter, which was found to be exudation into and between the cells. In some the epithelium was swelled and thickened, and here and there loaded with fat granules. In some parts the outlines of the renal cells could not be made out; the tubules were full of a dense, homogeneous, granular matter, containing numerous oil globules. The mucous coat of the stomach presented numerous catarrhal ulcers; extravasation of blood existed about the base of some of the ulcers. The Peyerian patches and solitary glands of the small intestine were swelled and prominent. The bladder contained 16 oz. of dark amber-coloured urine, which was analysed by Dr. A. Gamgee. The following were the principal results of the examination—1. The presence of a large amount of abnormal ingredients, but the total solids were considerably below the natural quantity. 2. Leucine and tyrosin were the chief abnormal ingredients. 3. A little albumen was present. 4. Notwithstanding the extreme jaundice no bile acids were found. 5. That urea was probably much diminished in quantity, and only traces of uric acid were found. 6. That the chlorides and earthy phosphates were entirely absent, and only the faintest trace of sulphates was found.

The facts of the case have led Dr. Grainger Stewart to the conclusion that acute yellow atrophy of the liver is not a local affection, but a blood disease:—"The following considerations seem to me to point distinctly to this conclusion: 1. The blood was dark and fluid, and the muscles were dry, as they are in typhus fever and other blood-diseases. 2. The spleen was soft and pulpy, as it is in many blood-diseases. 3. The fact that the kidneys and the liver were affected by a peculiar and identical morbid process indicates that they were influenced by a common cause, that cause situated in the blood, and a form of fever poison. 4. The appearance, amount, and effects of the exudation being different from what we see in simple inflammation, either of the liver or kidneys, indicates that some peculiar matter was present in the system, altering the ordinary processes. 5. The fact that this disease occurs so often during pregnancy, and that it seems to be induced by depressing mental emotions, indicates that it is of a constitutional origin. From these considerations I think we cannot avoid concluding that this peculiar affection is a blood-disease, and that it leads to atrophy of the liver, by diffuse exudation into the hepatic cells, which is followed by a rapid fatty degeneration."—*On Acute Yellow Atrophy of the Liver, by T. Grainger Stewart, M.D., &c. &c.: Edinburgh Medical Journal*, Oct. 1865.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

On Pseudo-rheumatic Ostitis and Arthritis in the Young. By Prof. Roser.—By this appellation Dr. Roser indicates the affection which has been described by different clinical observers under the names of periostitis rheumatica, diffusa, or acutissima, ostitis acutissima, osteomyelitis diffusa, osteophlebitis, osteite epiphysaire des adolescents, typhus des os, &c. Attacking boys and young persons in otherwise good health, and apparently not the subjects of any dyscrasis, it manifests itself as a severe periostitis and osteomyelitis with suppurative inflammation of the joints, accompanied frequently with febrile action resembling typhus. Dr. Roser lays down the following propositions:—1. This affection may be conveniently designated as *pseudo-rheumatic ostitis*, &c., as distinguishing it from rheumatism, with which it has been confounded. 2. Its cause is unknown, for suppurating osteomyelitis cannot be attributed to slight concussions of the limb and similar occurrences. Indeed, in only two or three out of more than 100 cases, has Dr. Roser been able to trace its occurrence to a fall; and in none of the numerous examples of fracture which he has treated in the young, has he ever met with osteomyelitis. That there must be peculiarity in the cause is evident from the fact of the disease being almost confined to youths, infants being well nigh exempt from it. 3. It is most frequently found localized in the large diaphyses, especially the tibia and lower end of the femur. The joints are rarely primarily affected, and phlegmonous abscesses are of still seldomer occurrence. The greater activity of growth, with its accompanying vascularity, is the probable predisposing cause of the localization of the disease. The inflammation does not generally extend far, but several bones or joints may be simultaneously or successively attacked. 4. This pseudo-rheumatic disease may exhibit different degrees of acuteness. In very bad cases, the patient may die before any localization has taken place, death occurring in some from acute septicæmia, and in more chronic cases from hectic septicæmia with profuse discharges, or from disease of the kidney. In all these instances the true nature of the affection is liable to be overlooked. 5. Pseudo-rheumatic fever is usually accompanied by miliary sweat, and diarrhœa; but these do not seem to be in direct connexion with the primary diseased process. The diarrhœa, not generally met with at the commencement, almost always occurs after the establishment of suppuration. 6. Most of the cases formerly designated as periostitis, would be more correctly named osteomyelitis. 7. In osteomyelitis there is compression of the medullary cavity, and the fluid fatty matter exuding through the canaliculæ accumulates behind the periosteum, and separation and inflammation of the periosteum, secondary to myelitis, may be induced by the irritation caused by the presence of this fat. 8. Osteomyelitis is often arrested at the point of junction of the epiphyses; and sometimes an inflammatory separation of the epiphyses takes place, and that not only at the heads of bones, but at the processes for the attachment of muscles. This separation of epiphyses is not of such frequent occurrence as it is sometimes represented to be, fractures, or pseudo-fractures having been several times in osteomyelitis mistaken for it. 9. The epiphyses may be the seat of primary or secondary inflammation and necrosis. 10. Osteomyelitis in the vicinity of the epiphyses may give rise to hypertrophic elongation of the articular ligaments, producing loose-joint, sub-luxation, or spontaneous luxation. The true cause of this occurrence is the too active growth of the ligaments from the increased supply of blood and nutrition in the vicinity of the necrosis. 11. In several cases the osteomyelitis leads not only to necrosis, but to inflammatory osteoporosis and hyperostosis. 12. Abscess of bone is only met with in pseudo-rheumatic osteomyelitis as a secondary occurrence, most of the abscesses occurring in the bones first attacked by the osteomyelitis. The fact that no acute abscesses occur in the medullary cavity in these cases, which would seem to offer more favourable conditions for their production than the spongy ends of the bone, has much puzzled clinical observers. 13. In many cases, besides the osteomyelitis, there is cotemporary inflammation of one or even of several joints, which may be regarded as a pseudo-rheumatic arthritis localized in the synovial membrane. This, in some cases, indeed, appears without any osteomyelitis being present. In some rare cases it may terminate in complete resolution, but anchylosis is the usual consequence. It may also be followed by spontaneous luxation, abscess, or necrosis. It not unfrequently happens that a pseudo-rheumatic articular abscess may form and be evacuated without anchylosis supervening. 14. Pseudo-rheumatic phlegmon and myositis are of rare occurrence, ostitis or periostitis having usually preceded. 15. Although there are instances of inflammation of the pericardium, pleura, lungs, or kidneys, met with in the course of pseudo-rheumatic osteomyelitis, a portion of these must be regarded only as secondary affections due to septicæmia or pyæmia. 16. There is no affection which has given rise to more errors of diagnosis. It has been especially confounded with typhus, rheumatism, erysipelas, and pyæmia. 17. Pseudo-rheumatic osteomyelitis is accompanied from the beginning with inflammatory œdema around the bone. This is not dependent upon periostitis, and must not be confounded with periostitic abscess. This deep-seated œdema is of high utility in establishing the reality of the existence of osteomyelitis. 18. The osteomyelitis, after amputation, is always of a pyæmic nature, and must be distinguished from pseudo-rheumatic osteomyelitis. 19. Pseudo-rheumatic coxitis presents especial difficulties in its diagnosis. The apparent elongation of the limb at the commencement is often wanting, and the apparent thickening of the bone towards the cavity of the pelvis is remarkable. This apparent thickening of the ileo-pectineal region resembles that of the condyles of the femur in gonitis, and of the trochanters in coxitis. 20. Incision of the periosteum is of no great utility. 21. Incision into the bony structure should be avoided as much as possible, expansion by means of a forceps being a preferable procedure after dividing the superficial tissues. The muscles which are in the way should be divided transversely, not longitudinally. 22. Explorative "necrotomy" must frequently be resorted to in osteomyelitis; and operations for necrosis from pseudo-rheumatic inflammation of bone, are attended with little danger. 23. Osteomyelitic fractures (pseudo-fractures) do not call for amputation. 24. Amputations, disarticulations, or excisions after pseudo-rheumatic ostitis or arthritis have usually a favourable issue.—*Archiv der Heilkunde,* 1865, Nos. 2—6.

On the Performance of Circumcision in Accidental Phimosis. By M. Tillaux.—Accidental phimosis may arise from various causes, but in this communication M. Tillaux confines his attention to that form of it connected with soft chancre of the glans or prepuce, and inquires whether circumcision should be performed for its relief. Several French surgeons, with M. Nélaton at their head, prohibit its performance in the presence of unhealed chancre, as the pus of the latter, coming in contact with the edges of the incised surface, may give rise to new chancres, delay cicatrization, and even induce a phagedænic character. Although, in some instances this position may hold good, it must not be too absolutely affirmed. A patient having a corona of soft chancres around the preputial orifice accompanied with phimosis, it may seem an easy matter to rid him of both grievances by an operation performed on the healthy portion of the prepuce. Such an operation does not, however, succeed, for rarely, if ever, can immediate union be obtained, although the incision has passed through only healthy parts, and no pus from chancrous surfaces has come into contact with it. The duration of the treatment is never abridged, and the surgeon may thus only substitute a large for a small sore. A case of rarer occurrence, but of greater importance, may, however, present itself. Chancres having formed on the inner surface of the prepuce or corona glandis, may, in certain patients, become complicated with accidental phimosis. The pus only imperfectly escaping produces additional sores, and cleansing can only be performed with difficulty. This state of things may remain apparently stationary for weeks or months, the pain not being great, and the patient paying little attention to it. The glans cannot be exposed, and the prepuce is thickened, infiltrated, and sometimes indurated, an incessant and abundant discharge pouring from the orifice. Observation teaches us, that in such cases more or less extensive and deep ulceration may exist, which may be attended with great destruction and do irreparable mischief. The precept of waiting for the performance of circumcision until the chancre has cicatrized, is of impossible execution, for no cicatrization will take place while the chancre remains unexposed. It must be resorted to at once even if the incision has to pass through the midst of the chancrous surface. This practice, possessing many advantages, has no inconveniences. Even if the wound become chancrous, we have a visible in place of a concealed sore to deal with, and should phagedæna arise it is far more easily dealt with than is a hidden and destructive chancre. Not only by an operation is a cure rendered probable which was before problematical, or at all events only procurable after more or less destruction of the organ, but considerable time is gained. Thus, a patient who has suffered from soft chancre, with phimosis, during three months, will require five or six months or more for his cure, imperfect as it is; but after an operation this may almost always be accomplished within a month.

"In fine, circumcision should not be performed when the phimosis dates only a few days, when the chancres are sufficiently exposed to admit of their being dressed, and if there is no reason to fear the existence of erosion of the glans. But it should be resorted to without delay if the phimosis is of old date, and if an incessant and abundant discharge from the preputial orifice testifies to the existence of hidden ulcerations, the number and extent of which can only be discovered by this means."—*Bulletin de Thérapeutique*, June 15.

On the Increase of Syphilitic Affections of the Mucous Membrane of the Mouth. By Prof. SIGMUND.—Not only is syphilis yearly on the increase, Prof. Sigmund observes, but of late he has met with a more than proportionate increase of the forms of it which affect the mucous membrane of the lips, cheeks, tongue, and palate. The papular infiltrated form is that which usually prevails, being often accompanied by more or less well-defined excoriations or chaps, sores accompanied by induration being much seldomer met with. In fact, the forms accompanying the earlier stages of syphilis have been of much more frequent occurrence than those of a later epoch, although in some instances there has been seen the dry, inelastic condition of the mucous membrane, with disposition to crack and peel off, characterizing old syphilides.

In one set of the cases a careful examination readily detects signs of syphilis in other parts of the body; but in others the existence of such cannot be demonstrated, and there seems every probability, or even certainty, that the buccal mucous membrane is the original seat of the disease. The first group of cases presents no great difficulties, as, other signs being detected, this affection of the mouth resolves itself into one of the symptoms of constitutional syphilis. Some of these cases, however, present more difficulties, especially in children, in whom a decision is often of great importance as to whether the syphilitic contagion has been conveyed by the nurse to the child, or the reverse, a probability only being attainable where the condition of the mother cannot be examined into. Some of the adults attributed their sore mouth to smoking poisoned cigars; but even the patients themselves became convinced of the groundlessness of their belief by the exhibition of the signs of syphilis which existed on other parts of the body. Some years since a similar charge was brought against the cigars of a particular factory, and was disproved not only in the same way, but by the examination of several hundred women engaged in the factory, not one of whom, strange to say, manifested any signs of syphilis.

In the second group are comprised those patients in whom the buccal mucous membrane alone exhibits the signs of syphilis usually met with in the sexual organs, the anus, and its vicinity, but which in these cases manifest no traces of disease. It is to be observed, however, that for the most part they rapidly disappear, sometimes leaving no marks of their existence. In some of these cases the mode of communication may be ascertained, this being sometimes immediate from the diseased to the healthy individual by smoking, kissing, &c., and sometimes mediate through the use of various drinking or eating utensils, tobacco-pipes, &c.; and although some of the statements to this effect are to be received with incredulity, yet repeated observation has shown that the employing in common blowing instruments, &c., by barometer-makers and musicians, may become the means of conveying syphilitic poison to the mucous membrane of the mouth.—*Wien Med. Wochenschrift*, 1864, No. 50.

Summary.

Air-passages.—Guersant on Foreign Bodies in the Air-passages of Children. (Bull. de Thérapeutique, Sept. 15th.)

Amputation.—Spence on Successful Amputation at the Hip-joint. (Edin. Med. Journal, November.)—Roux on Tibio-tarsal Amputation. (Archives de Méd. Navale, Sept. 1st. With Photographs.)

Aneurysm.—Kennedy on Femoral Aneurysm Successfully Treated by Digital Compression. (Edin. Med. Journ., Nov.)

Bladder.—Seydel on the Sacculated Bladder. (Archiv der Heilkunde, 1865, No. 5. A posthumous essay by a distinguished surgeon.)

Bone.—Contusion and Contused Wounds of Bone. (American Journ. of Med. Science, July.)—Textor on Removal of an Exostosis from the Orbit. (Würzburg Med. Zeitsch., Band vi. H. 5.)

Cancer.—Popper on Development of Cancer in Transversely-striped Muscle. (Med. Jahrb. d. Wien. Aerzte 1865, H. 4. Besides a minute description of several cases, there is an elaborate bibliographical critique.)

Cataract.—Jacobson on Occurrences during Healing after Operation for Cataract. (Graefe's Archiv, Band xi. Abth. 2.)

Club-foot.—Adelmann on Application of Pirogoff's Operation to the Treatment of Club-foot. (Petersburgh Med. Zeitsch., Band ix. No. 1.)

Dislocation.—Dolbeau on Apparatus for Reducing Dislocation of the Elbow. (Gazette des Hôp., No. 138.)—Guersant on Traumatic

Dislocation of the Femur in Children. (Bull. de Thérap., July 30th. Two cases related of dislocation on the ilium, occurring in children twelve and thirteen years of age.)—Parisot on Unilateral Dislocation of the fifth on the sixth Cervical Vertebra, treated successfully thirty-six hours after. (Gazette Hebdom., No. 47.)

Ear.—Roger on Meningitis consecutive on Otitis. (L'Union Méd., Nos. 118 and 122.)—Triquet on Differential Diagnosis in Changed Conditions of the Membrana Tympani. (Gaz. des Hôp., Nos. 120 and 122.)—Braconnot on Catheterism of the Eustachian Tube by means of the Otoscope. (Gaz. des Hôp., No. 135.)—Gruber on Treatment of Catarrh of the Middle Ear. (Deutsche Klinik, Nos. 38 and 39. This is effected by injection of the Eustachian tube, which the author states he has performed 20,000 times without any ill effect whatever; or by injections thrown into the nares by a glass syringe, which find their way into the tympanum.)

Elephantiasis.—Fayrer on Elephantiasis of the Leg treated by Ligature of the Femoral Artery. (Edin. Med. Journ., Nov.)

Emphysema.—Gallez on Primary Traumatic Emphysema. (Bull. de l'Académie de Belgique, June 24th.)

Epistaxis.—Reiner's Simple Means of Arresting Epistaxis. (Allg. Wien. Med. Zeit., Nos. 40 and 41. Reiner states that profuse epistaxis may be easily arrested by the application of external compression in place of plugging, either by means of the finger or of a compressor, of which he gives a figure.)

Excision.—Mursick's Successful Case of Excision of the Head of the Femur after Gun-shot Fracture. (New York Journal of Med., vol. i. No. 6.)—Sarazin's Case of Excision of the Hip-joint. (Gaz. des Hôp., No. 112. Successfully performed on a lad nine years of age, the report coming down to a few weeks afterwards.)—Bergmann on Two Fatal Cases of Excision of the Hip-joint. (Petersburgh Med. Zeitsch., Band viii. No. 3.)—Fauvel's New Excision Saw. (Gaz. des. Hôp., No. 123. A combination of saw and forceps, to which the name of *priollabe* is given.)

Eye.—Guersant on Cancer of the Eye in Children. (Bull. de Thérap., Sept. 30th.)—Hildreth on Gonorrhœal and Purulent Ophthalmia. (New York Journ. of Med., vol. i. No. 5. Insists on the great advantage derivable from compression of the globe, instituted from the earliest period.)—Warlomont on the Operation for Entropion. (Presse Méd. Belge, No. 49. Jäsche and Arlt's operation as modified by Von Graefe.)—Jäsche on his new Operation for Trichiasis and Entropion. (Petersburg. Med. Zeit., Band viii. No. 4.)—Dyer on Asthenopia unconnected with Hypermetropia. (New York Journ. of Med., vol. ii. No. 1. With a long discussion at the American Ophthalmological Society.)—Carter's Diagnosis of Diseases of the Eye. (Dublin Journ., Nov.)—Debrou on Malignant or Carbuncular Œdema of the Eyelids. (Archives Générales, Oct.)

Foot.—Sédillot on Perforating Ulcer of the Foot. (Gaz. des. Hôp., No. 125.)

Fracture.—Wahl's Prognosis and Treatment of Compound Fracture.

(Prag. Vierteljahrschrift, 1865, B. iv. Especially referring to purulent œdema and infectious complications of severe injuries, and deprecating too hasty amputation.)—Rizet, Employment of "*Massage*" in the Diagnosis of Fracture. (Gazette Méd., No. 29. He states that the prolonged employment of methodical frictions is an excellent means of distinguishing sprains from fractures, when great effusion is present. It also aids in dispersing the effusion when fracture exists.)—Zaufal on Fissure of the Petrous Bone. (Wien. Wochensch., Nos. 63 and 64. Minute anatomical description of two cases of fissure of the temporal bone, implicating the tympanum.)—Berenger-Ferand on Immobilization in Fractures of the Lower Jaw. (Bull. de Thérap., Oct. 30. A case in which numerous fragments were kept in contact by means of ligatures.)—Rosenberg on Congenital Fracture of the Neck of both Femora. (Virchow's Archiv, B. xxxiv. H. 1. With illustrations.)

Gunshot Wounds.—Sourier's Cases of Gunshot wound of the Hand. (Recueil de Med. Militaire, July. Related in proof of the value of continuous cold irrigations.)

Hæmatocele.—Fano's Case of enormous Hæmatocele successfully treated by incision and suture. (L'Union Méd., No. 139.)

Hare-Lip.—Giraldès on the Operation for Hare-lip. (L'Union Méd., No. 138.)

Hysterometer.—Avrard on a Dilating Hysterometer. (Gaz. Hebd., Nos. 34 and 38.)

Knee-joint.—Verneuil on Penetrating Wounds of the Knee-joint. (Gaz. Hebd., No. 36; Gaz. des Hôp., Nos. 112 and 118. With a Discussion at the Paris Surgical Society.)

Lithotomy.—Civiale on the Breaking-up of Large Calculi. (Bull. de l'Acad. de Méd., Oct. 10. With figures of the instruments employed.)

Lithotrity.—Ségalas on a Case of Successful Lithotrity in a Patient Seventy-three Years of Age. (L'Union Méd., No. 137.)

Molluscum.—Pick on Molluscum. (Wien. Med. Woch, No. 49.)

Nerve.—Szymanowski on Neureotomy and Suture of Nerve. (Prag. Viertalj., 1865, No. 4. Believes that Eulenberg and Landois' experiments throw much doubt on the results of nerve-suture observed by Nélaton and Laugier.)

Nose.—Adelmann on Osteotomy of the Perpendicular Plate of the Ethmoid Bone and the Vomer. (Petersburg Med. Zeit., B. ix. No. 1. Performed for the remedy of a deformity of the nose.)

Ovariotomy.—Butcher's Two Cases of Successful Ovariotomy. (Dublin Journal, Nov.)—Peaslee on Two Cases of Ovariotomy. (American Journ. Med. Science, July.)—Elliot on the Removal of a Multilocular Exogenous Ovarian Tumour. (New York Journ. of Med., vol. i. No. 6. A fatal case, very minutely recorded.)—Labbé's Case of Ovariotomy. (L'Union Méd., No. 142. This, we believe, is the first successful operation performed in Paris.)

Polypus.—Borelli's Cases of Naso-pharyngeal Polypus and Retropalatine Tumour. (Gaz. Med. di Torino, Nos. 39–41.)

Pyæmia.—Krackowizer on Pyæmia. (New York Journ. of Med., vol. i., Nos. 5 and 6. With a discussion at the New York Academy of Medicine.)

Rhinoscope.—Voltolini's Application of Galvano-Caustic to the Pharynx by means of the Rhinoscope. (Allg. Wien. Zeit., No. 33. A figure of the instrument given.)

Spine.—Auerbach on the Production, Prevention, and Treatment of Scoliosis. (Deutsche Klinik, Nos. 30–37.)

Staphyloma.—Schiess-Gemuseus on the Pathological Anatomy of Anterior Sclerotic Staphyloma. (Graefe's Archiv, B. xi. Ab. 2.)

Strabismus.—Von Graefe on Binocular Vision in Strabismus. (Von Graefe's Archiv, B. xi. Ab. 2.)

Syphilis.—Sigmund on Incubation of Syphilis. (Wien. Med. Woch., Nos. 77–80.)—Günts on Syphilitic Fever. (Küchenmeister's Zeitsch., 1865, No. 6. The author traces analogies between the febrile action at the onset of constitutional syphilis, and the exanthemata.)

Thoracocentesis.—Prolonged Discussion of the Académie de Méd. de Paris on the Best Mode of performing Thoracocentesis. (Bulletin, July 25–Oct. 10.)

Thyroid Body.—Hamilton on Extirpation of the Thyroid Body. (Dublin Journal, Nov.)

Urethrotomy.—Schiltz on Cases of Stricture treated by Urethrotomy. (Deutsche Klinik, Nos. 31, 32, 35. Twelve cases given, with the autopsies in two of them.)

Vesico-vaginal Fistula.—Courty on Six Successful Cases of Vesico-vaginal Fistula treated by the American method. (Bulletin de Thérap., Oct. 15.)—Heppner's Cases of Vesico-vaginal Fistula. (Petersb. Med. Zeits. B. ix. H. 2.)

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D.,

Fellow and Examiner, Royal College of Physicians; Obstetric Physician to St. Thomas's Hospital, &c.

I. The Non-pregnant State.

A Case of Double Uterus and Vagina. By Dr. Rabe.—A healthy woman, aged twenty, was admitted in May, 1865, into the Town Hospital of Dresden, for blennorrhœa and excoriations of the vulva. She had menstruated since sixteen. The external genitals were normal, but the hymen was wanting. The vagina was double, the lower end of each half being provided with a hymen-like fold of mucous membrane. In the summit of each vagina was a small firm vaginal portion of uterus, each possessing a small transverse os. The uterine sound passed freely into the left os uteri, but only slightly into the right, so that it remained doubtful whether the body of the uterus had two distinct cavities.—*Monatsschrift für Geburtskunde*, Oct. 1865.

A Case in which the Menses were apparently substituted by Hæmor-

rhage from the Skin.—The subject of Mr. d'Andrade's case was a stout healthy Parsee lady, aged eighteen. She had menstruated regularly from thirteen to fifteen and a half, when catamenia became first irregular, then ceased, being replaced by bleeding at the gums and nose, and vomiting of blood. Menstruation returned; no pregnancy. Mr. d'Andrade observed blood to ooze from the healthy skin of the left breast and of the right fore-arm. The blood exuded showed red and white globules under the microscope. The skin-hæmorrhage recurred every month or two. Subsequently blood oozed from the forehead.—*Trans. of Med. and Phys. Soc., Bombay,* 1862.

Dropsy of the Fallopian Tubes. (*Reports of the Dublin Obst. Society.*) By Dr. BENNETT. *Dublin Quarterly Journal of Medical Science,* Aug. 1865.

Operations for Ovariotomy, performed since June, 1864, *to June,* 1865. By Professor KŒBERLÉ, of Strasbourg.—Professor Kœberlé continues the relation of his ovariotomy cases. The present series contains six cases. Of these, three were cases of double ovarian tumours. There were four recoveries and two deaths. (*L'Union Médicale,* Sept. 1865.)

On the Use of Intra-Uterine Pessaries. By Dr. H. HILDEBRANDT. (*Mon. f. Geb.,* Aug. 1865.)

On the Emmenagogue and Galactagogue Properties of Faradisation. By Dr. FORDYCE BARKER, New York. (*Medical Journal,* May, 1865.)

Case of Traumatic Occlusion of the Os and Cervix Uteri. By R. T. TRACY, M.D. (*Australian Medical Journal,* June, 1865.)

II. PREGNANCY.

Extra-uterine Gravidity. By Dr. KAMMERER.—Dr. Kammerer related a case and presented the specimen to the New York Obstetrical Society, of extra-uterine gestation. A woman, aged thirty, had been under treatment for chronic metritis. Seven or eight years previously she had a child. She became again pregnant, and a little time subsequently was taken suddenly ill, with symptoms of internal hæmorrhage and peritonitis, and in the course of a few hours died. Several quarts of blood were found in the peritoneal cavity, and on the left ovary a rent revealing the source of the hæmorrhage. On opening the ovary an embryo was discovered about four weeks old.—*New York Medical Journal,* May, 1865.

[The above history is very brief, but appears to be precise. If the facts are correct, there can remain no doubt of the possibility of ovarian gestation, which has been strenuously denied.—R. B.]

Case of Fibrinous Concretion in the Pulmonary Artery (in a Pregnant Woman) causing Sudden Death. By G. H. PHILIPSON, M.D. Cantab. (*Lancet,* Sept. 1865.)

Prolapsus of the Uterus in Advanced Pregnancy; Incision practised in the unyielding Cervix; Perforation of the Child. By Professor Breslau. (*Monatsschr. f. Geburtsk.*, 1865.)

A Contribution to the Microscopical Anatomy of the Mature Human Ovum Investments. By Professor Dohrn, of Marburg. (*Ibid.*, Aug. 1865.)

III. Labour.

Forty Cases of Artificial Premature Labour.—Dr. Simon Thomas, of Leyden, relates 40 cases in which labour was artificially induced. The indications were chiefly contractions of the pelvis; and these were determined less by the histories of previous labours than by accurate measurements expressly made. Thus, in 5 cases, the patients were primiparæ. The first method employed was to place a bougie for a short time a few inches between the uterus and membranes, changing it every day for a larger one. Labour only came on in ten days, and the forceps was used. In another case, Kiwisch's douche was used. Labour followed in five days. The mother died of pyæmia. In other cases the bougie was used, or the douche; generally days elapsed before labour. Afterwards Krause's method, the leaving an elastic catheter in the uterus, was used. The time expended was from six to ninety-two hours, the majority taking from twenty-four to forty-eight hours. Of the 32 children born after Krause's method, 25 lived; of the 32 mothers, 25 had a quite natural puerperal history; 4 died of pyæmia or endometritis.

[The method of Kiwisch is so uncertain as to time, and so many fatal results are now recorded as the consequence of the operation, that it ought henceforth to be abandoned. The method of Krause, of leaving a catheter in the uterus carefully slipped up between the membrane and the uterine wall, is, upon the whole, the most safe and certain of those in ordinary use; but there seems no good reason why the operation of inducing labour should be permitted to drag on for forty-eight or ninety-two hours. And, in most cases, the reasons against uncertainty and protraction are very strong. The method of direct artificial dilatation of the cervix practised by the Reporter, has now been amply proved to be quite safe and practicable, and by its aid it has become altogether unnecessary to keep a woman in suspense, and perhaps in danger, more than twelve hours, under any circumstances.—R. B.]

Contributions from the Obstetric Clinique of Königsberg. By Dr. Seydel.—*On Eclampsia Puerperalis.*—Dr. Seydel relates 5 cases of eclampsia. In all albumen was found in the urine before, during, or shortly after labour, and disappeared wholly or in part during childbed. A commentary is appended in which he discusses the various theories of the etiology of the disease. Traube's view, that eclampsia puerperalis is produced by increased pressure upon the aorta, inducing œdema of the brain, with secondary anæmia, will, he thinks, account for many, but not for all cases. The theory which (forgetful of British predecessors) he attributes to Hecker, that the cause is the transport of excrementitious matters into the blood in consequence of acute nephritis, he opposes, saying its supports vanish more and more with the advance of clinical observation; for, he observes, the albumen in the urine does not appear by itself without labour, but increases during the act of labour, there having been no albumen during pregnancy; and dissection of persons dead of eclampsia always proves the inadequateness of the kidney affection to account for the acute uræmia. He recalls attention to the view followed by the older obstetricians, and recently favoured again, that the most powerful, perhaps the primary, affection is that of the nervous system. In support of this, Seydel adduces, after Spiegelberg, a group of symptoms manifesting excitation of the sympathetic nervous system; for instance, the dilated sluggish pupils, the spasm of the vessels of the skin which—not indeed without the help of the spasm of the respiratory muscles—causes lividity of the skin; the contraction of the muscular coat of the vessels seen in the paleness of the face, and after the attacks, the compensating turgor; and, lastly, the remarkable atony of the uterus revealed by hæmorrhages. He thinks the alteration of the nervous system starts from the uterus, and especially its contractile function.—*Monatsschrift für Geburtskunde*, Oct. 1865.

Case of Labour with Fatal Result; Steatomatous Degeneration of the Uterus. By J. K. Ostertag.—The following is an outline of Dr. Ostertag's case of steatomatous degeneration of the uterus:

A woman, aged forty-four, had borne twelve children; on two occasions there had been adhesion of the placenta and some flooding. In the thirteenth pregnancy copious hæmorrhage occurred; labour set in with profuse flooding; uterus very large, visibly divided into two halves, suggesting twins; child delivered alive; uterus maintained the same size as before. Flooding persisting, the hand was introduced, and felt a tumour in the posterior wall of the uterus, giving the sensation of an incarcerated placenta. The cord ran past this tumour to the fundus, but the placenta could not be reached. Its expulsion was abandoned to nature. The discharges became very offensive, and on the sixth day the patient died. On dissection, the whole uterus, from the left border of the os to beyond the fundus, and over to a third of the right side downwards, was in a state of steatomatous degeneration. At the fundus it was three inches thick, and there were three or four excrescences on the surface the size of a fist; upon these the form of the uterus depended. The right side must by itself have formed the ovular sac. The uterus weighed ten pounds.—*Zeitschr. f. Wund. u. Geb.*, 1865.

Two Cases of Rupture of the Uterus. By J. Llewellin, Esq.—Mr. Llewellin relates two cases of rupture of the uterus. The first is of unusual interest, from its ending in recovery. The patient was in her third labour. Pains had ceased two hours after a sudden gush of blood, but there was no alarming symptom at first. On examination, no child, but a mass of intestines, was felt. Under chloroform, delivery was effected by turning. The head was in the left iliac fossa; the uterus filled with intestine. The rupture was in the middle of the posterior wall from fundus to cervix. The child was dead. Placenta followed, when a small bunch of intestines was seen on the sheet. These were returned; the uterus contracted. She took small doses of opium, took no aperients, and was at the wash-tub in three weeks.

The second case ended fatally in thirty-one hours. The patient was in her fourth labour. The head was found in pelvis, a large quantity of blood having passed; prostration; delivery by turning; child restored. Rupture not known at time, until fruitless search was made for the placenta. The hand passed through the womb amongst the intestines. Collapse set in, and death. The placenta was found lying flat against the middle lobe of the liver. The uterus was ruptured in the middle line from fundus through cervix, vagina, and rectum, to within half an inch of the anus.—*Australian Medical Journal*, Jan. 1864.

Cæsarian Section on account of a Large Fibroïd Tumour of the Uterus.—Professor Breslau's case is of peculiar interest. A woman suffered retention of urine and constipation. A large tumour filled the pelvis, pushing the cervix uteri near to the outlet and forwards. The tumour was uniformly smooth, very hard, and immovable. It appeared impracticable to extirpate it; it rose above the symphysis. Prof. Breslau endeavoured to bring about degeneration by transfixing it with needles. This did no harm and no good. She became pregnant. It was resolved, on consultation, that Cæsarian section offered the only means of safety to mother or child. The cord came down and could not be replaced; the left foot could be reached with great difficulty high up. For a moment the bleeding from the incision through the uterus was profuse. Two sub-peritoneal tumours presented themselves on the uterine surface; another, sub-mucous, showed itself in the wound. The child extracted alive. The uterus being emptied, did not contract regularly; it did not sink towards the pelvis, nor did the sides of the wound close. It remained large, and showed a disposition to invert itself. It was necessary to unite the uterine wound. Death followed twenty-two hours after the operation.

Autopsy.—Only two fingers could be squeezed in the conjugate diameter. The entire uterus and tumour was removed; it weighed five pounds and three-quarters.—*Mon. f. Geburts.*, 1865.

Statistics: Report of the Cologne Lying-in Institution, 1860–3.—The Reports of several lying-in hospitals of Germany have been published by the physicians. The statements are chiefly statistical, and it would be tedious to analyse them at any length. The Cologne Report, however, records one feature which is extremely important as an aid in forming a correct estimate of the mortality of lying-in hospitals. During the four years there were 1274 labours; of these no less than 788 were *primiparæ*, 1102 were *unmarried*, and 32 were *widows*. The great majority, therefore, were subjects especially prone to severe labours and to puerperal fever: 2·86 per cent. of the children were dead before birth; 2·78 per cent. more died during labour. The proportion of stillborn births therefore amounted to 5·64 per cent.—Dr. F. H. G. BIRNBAUM: *Mon. f. Geb.*, 1865.

Laceration of the Uterus, Vagina, and Bladder during Labour: Examination as to Manslaughter through Malapraxis. By Dr. HOFFMANN. (*Mon. f. Geb.*, 1865.)

On Dilatation of the Os Externum Uteri during Labour by Incisions. By H. HILDEBRAND. (*Königsb. Med. Jährb.*, Band iv. Heft 1.)

On the Facilitation of the First Stage of Labour. By ANDREW INGLIS, M.D. (*Edinburgh Medical Journal*, July, 1865.)

Double-headed Monster; Obstructed Labour; Decapitation. By Professor BRESLAU. (*Mon. f. Geb.*, 1865.)

A Case of Cæsarian Section. By WALTER HARDIN. (*Lancet*, Sept. 1865.)

IV. PUERPERAL STATE.

Rupture of a Varix in the Vagina after Labour, causing Death.—The case of Dr. Helfer is a remarkable example of a rare form of post-partum hæmorrhage. A woman who had borne two children was pregnant for the third time; in the early months varicosities, especially along the vena saphena, crural vein, and pudendal veins of the left side appeared; these increased so that in the seventh month the pudendal varices were as large as grapes and goose-eggs, and walking was difficult and painful. Labour occurred at term, easily. The puerperal state was normally passed. On the fourteenth day she went to household work. She exerted herself in drawing water from a running stream; this brought on pain, and flooding followed. It was arrested by plugging. She recovered for a time. Three days later a second very profuse flooding occurred, in consequence of which she sank. Autopsy showed no mark of peritonitis, no extravasation in the peritoneum. Varicosities on both ovaries, the size of crow and goose-quills; the tubes also varicose; the whole cellular tissue of the pelvis filled with blood-clots running along the posterior wall of the vagina, and proceeding from a burst varix.—Dr. F. W. HELFER: *Mon. f. Geb.*, 1865.

Transfusion in a Puerperal Woman exhausted by Flooding.—A woman, aged forty, had flooding from placenta prævia. The colpeurynter was used, and she was delivered on the following day. More blood was lost during and after labour. She was extremely exhausted and anæmic; had frequent faintings. Simon Thomas injected two ounces of blood drawn from the husband, when the rest began to congeal. Two hours later the feet got warm, the pulse returned. She rallied more and more, and gradually recovered completely. The apparatus used was that of Martin.—SIMON THOMAS: *Nederlandsch. Tijdschr. von Geneeskunde*, 1865.

Lectures on Puerperal Fever.—1. The Prevalence and Causes of Puerperal Fever. 2. The Constitution of the Pregnant Woman; the Constitution of the Puerperal Woman. 3. Uræmic Excretory Puerperal Fever; Cholæmic Excretory Puerperal Fever. 4. Thrombotic Puerperal Fever. By ROBERT BARNES, M.D.—*Lancet*, vols. i. and ii., 1865.

Practical Remarks on Puerperal Insanity. (*Reports of the Dublin Obstetrical Society.*) By Dr. LALOR.—The paper of Dr. Lalor gives a statistical account of 39 cases of puerperal insanity admitted into the Richmond District Lunatic Asylum.—*Dublin Quarterly Journal of Medical Science*, Aug. 1865.

V. THE FŒTUS AND NEW-BORN INFANT.

A Case of Lithopædion. By Dr. CONANT.—This gentleman exhibited to the New York Obstetrical Society an interesting specimen of lithopædion. The specimen was the result of the woman's first pregnancy. So far as was known, gestation was normal, and when labour came on Dr. Prescott, of Maine, was called. Labour pains subsided. Subsequently she had a very offensive perspiration. She recovered, and a hard tumour could be felt in her side. She subsequently gave birth to three children in successive pregnancies. In June, 1863, thirty-five years after the first pregnancy, she died. A calcified extra-uterine fœtus was found, and connected with it another hard mass, which was considered by those who examined it to be the placenta. The whole fœtus was covered by a calcified membrane.—*New York Medical Journal*, May, 1865.

On the Apparent Death of New-born Children.—Dr. J. Poppel, assistant-physician to the Obstetric Policlinic, at Munich, discusses the various doctrines referring to still-birth, and asphyxia at birth; and gives an elaborate account of the practical observations made by himself at the Munich Institution. Amongst other points, he distinguishes between death from asphyxia the result of interruption of the placental circulation, and death from compression of the brain. He details the histories of eight cases of death during labour from the use of the forceps, or under other conditions causing pressure upon the child's head. He remarks, that in none of these cases were there found clear marks of suffocation, such as sub-pleural or pericardial ecchymosis, or foreign matters in the trachea or bronchi. On the other hand, lesions of the central nervous organs were always more frequent than in the chest. These lesions commonly were, dislocation of the cranial bones, partial separation of the periosteum and dura mater by interposed layers of half-coagulated blood; considerable meningeal hæmorrhages on the upper and lower surfaces of the brain and cerebellum; hyperæmia of the cerebral substance. To illustrate the influence of pressure on the brain, he made experiments on pups by injecting water or blood through an opening made in the dura mater. The results were, that a very sudden and intense pressure increased for a very short time the respiratory function, the first effect being that the respirations were doubled in frequency; and the second, speedy paralysis and death; that a very gradually increasing pressure always retarded the respiratory movements.

He then gives an interesting statistical abstract of the experience of the Munich institution. Of 6183 children born, 5569 = 90·2 per cent. were born alive and vigorous; 309 = 4·9 per cent. were born asphyxiated and recovered; 58 = 0·9 per cent. asphyxiated and not recovered; 149 = 2·4 per cent. were dead-born; and 98 = 1·6 per cent. dead and putrid. Of the 5467 living children, 1911 = 34·9 were of primiparæ, and 3556, or 65·1, of multiparæ.

Of 362 asphyxiated children, 129 were of primiparæ and 233 of pluriparæ.

Of 149 dead children, 57 were of primiparæ, and 92 of pluriparæ.

For every 100 living girls there were . . 106 living boys.
,, 100 asphyxiated girls there were 136 asphyxiated boys.
,, 100 dead girls there were . . 161 dead boys.

Of all the vigorously alive and asphyxiated children that were recovered = 5878, 189 or 3·2 per cent. died within eight days.

Of all the healthy children = 5569, there died within the first eight days 137, or 2·45 per cent.

Of all the asphyxiated and recovered children = 309, there died in the first eight days 52, or 16·8 per cent. Of the children dying within the first eight days = 189, 116, or 61·3 per cent., were boys, and 73, or 38·7 per cent., girls.

The following table supplies useful information:

	Asphyxiated.			Born vigorous.	Dead-born.
	Total.	Not recovered.	Recovered.		
Accouchement forcé	77·7	11·1	66·6	—	22·3
Placenta prævia	63·3	20·	43·3	13·3	23·3
Turning	60·5	14·1	46·4	16·9	22·5
Prolapsed cord	49·9	9·4	40·5	21·6	28·4
Contracted pelvis	44·7	13·1	31·6	39·5	15·8
Breech presentation	44·5	9·1	35·4	44·6	10·9
Forceps	43·1	6·4	36·7	44·6	12·2
Protracted second stage	37·8	5·6	32·3	51·5	10·6
Secale	21·	1·7	19·3	77·3	1·7
Premature escape of liq. am.	20·7	4·1	16·6	73·1	6·1
Premature labour	17·5	2·7	14·8	75·4	7·2
Face-presentation	13·7	—	13·7	82·3	3·9
Cord-strangulation	10·8	1·5	9·3	86·5	2·6

Dr. Poppel sums up in the following propositions:

1. Anatomical examination shows, in the largest number of cases of dead and apparently dead-born children, unmistakable marks of death by suffocation.

2. In rare cases the marks of suffocation are absent or so slight, whilst the changes in the cranium are so conspicuous, that these latter

must be regarded, if not as the sole, at least as the auxiliary causes of death.

3. When, together with meningeal hæmorrhage, undoubted marks of suffocation are found, this does not prove that life had persisted notwithstanding the meningeal hæmorrhage, and that death had followed by suffocation. It may be affirmed with equal right that the pressure of the extravasations had excited premature respiration movements, and that the disturbances in the circulation in the chest were not the effect of respiration movements excited by carbonic acid in the blood, but of irritation of the medulla oblongata caused by pressure.

4. The observations of meningeal hæmorrhage in adults are, on account of their widely different etiology, unavailable in the explanation of similar hæmorrhages in the new-born.

5. Experiments on animals prove that only a pressure very sudden or very strong from fluid effusions in the cranium can destroy life, which in nature hardly can happen.

6. From these experiments the conclusion appears that the meningeal bleedings, which arise during birth, never cause the child's death by direct pressure upon the medulla oblongata. They point, however, to the opinion that the establishment of the regular respiration is delayed, and a more or less prolonged condition of sopor is brought about.

7. Clinical observation suggests that in all cases of death, and apparent death, there is interruption of the placental circulation, and also suffocation.

8. Children of primiparæ are more frequently born asphyxiated and dead, than are children of multiparæ.

9. More boys than girls are born dead, and apparently dead.

10. Within the first eight days after birth nearly seven times more children, recovered from asphyxia, die than vigorously born children.

11. The more children born under any given complication occasioning asphyxia and death that are born asphyxiated, the greater will be the number irrecoverable from this asphyxia, and the greater will be the number born dead.

12. Not only in general, but also in every child-imperilling complication, more boys than girls will be born dead, or apparently dead.

13. The mortality within the first eight days is in direct relation to the duration and depth of the asphyxia.—*Monatsschrift für Geburtskunde*, 1865.

On the Diagnosis of Twisting of the Cord round the Child's Neck.—By Dr. Haake. Dr. Haake, referring to the frequency with which the child's life is threatened from the coiling of the cord around its neck, thinks it desirable that the existence of this complication should be verified before the head is born. This may be done, he says, by examining with the finger in the rectum. The finger can be easily carried above the head, so as to feel the umbilical cord and its pulsation. This gives a valuable guide to the life-condition of the child, and tells when to accelerate the birth of the head is necessary.—*Zeitschr. f. Med. Chir. u. Geburtsk.*, 1865.

CONTRIBUTIONS TO MEDICAL LITERARY HISTORY.

ADVERSARIA MEDICO-PHILOLOGICA.

BY W. A. GREENHILL, M.D. OXON.

(*Continued from* vol. xxxvi. p. 560.)

[THE persons who may consult this collection of words are requested to bear in mind that it does not pretend to completeness in any way. It is very far from being a complete collection of Greek medical terms, for all botanical and chemical words have been designedly omitted; neither is the treatment of each word to be considered complete, for no doubt various meanings are passed over, and probably better passages might frequently be quoted. It is simply a contribution (and that a very imperfect one) to Greek medical technology.]

ἀπολίνωσις, the name given by Paulus Ægineta[1] to the operation for fistula in ano by means of a ligature of raw thread, described in the Hippocratic treatise 'De Fistulis.'[2] The verb *ἀπολινόω* is used by Leo[3] to signify the application of a ligature to a tumour on the eyelid.

ἀπονευρόω, probably always used in the passive voice, and applied to a muscle, signifying to *become tendinous*.[4] In one passage we find the expression *μῦς ἀπονευρούμενος εἰς τένοντα*.[5] From this verb comes the word

ἀπονεύρωσις, which signifies the *tendinous extremity of a muscle*, whether it be flat or round.[6] It is probably always used in this sense, and though it is of course derived ultimately from *νεῦρον*, it has nothing to do with the *nerves*. It seems to have been a term recently introduced in the time of Galen, as in one place,[7] in speaking of tendons (*τένοντες*), he says, "The more recent authors call them *ἀπονευρώσεις μυῶν*, because (I suppose) they see the muscles terminating in them." He uses the word frequently,[8] as does also Oribasius, who transcribes him.[9] It is found also in Theophilus.[10]

ἀποπληκτικός, a word older than Hippocrates (being found in the 'Coacæ Prænotiones'), signifying *apoplectic, relating to apoplexy*. It

[1] Lib. vi. cap. 78, p. 222, l. 12, ed. Briau.
[2] § 4, tome vi. p. 450, ed. Littré.
[3] 'Consp. Medic.,' iii. 10, in Ermerins, 'Anecd. Med. Gr.,' p. 133.
[4] Galen, 'De Anat. Admin.,' i. 5, 11, tom. ii. p. 252, l. 8; p. 276, l. 2.
[5] Galen, *ibid.*, v. 8, p. 521, l. 5.
[6] See Galen, 'Comment. in Hippocr. "Epid. VI.,"' i. 2, tom. xvii. A. p. 804, l. 7; 'De Anat. Admin.,' i. 3, tom. ii. p. 133, l. 17.
[7] 'De Motu Muscul.,' i. 1, tom. iv. p. 368, l. 17.
[8] 'De Usu Part.' i. 16, tom. iii. p. 46, l. 8; 'De Musc. Dissect.,' c. 6, tom. xviii. B. p. 934, l. 11, and elsewhere.
[9] 'Coll. Medic.,' xxiv. xxv. tome iii. p. 298, l. 10; 350, 11, 13; 426, 1, ed. Daremberg, and elsewhere.
[10] 'De Corp. Hum. Fabr.,' v. 14, § 1, p. 205, l. 9, ed. Oxon.

is generally applied to the individual struck by apoplexy;[1] sometimes to the accidents or symptoms of the disease, so that τὰ ἀποπληκτικά[2] is nearly synonymous with ἀποπληξία. It is probably not found in any Latin writer earlier than the fourth century after Christ.[3] The application of the word—1, to remedies for apoplexy; 2, to the constitution predisposing to apoplexy; 3, to the state or condition of apoplexy; and 4, to the jugular veins, is mediæval or modern.

ἀπόπληκτος, a word also found in the Hippocratic Collection, and signifying a person struck by apoplexy,[4] synonymous nearly with βλητός. It is used in the same sense by Aretæus,[5] and also by Cælius Aurelianus, who uses it as a recognised Latin word.[6] The expression ἀπόπληκτον σκέλος, *an apoplectic* (or, as we should call it, a *paralytic*) *leg*, is quoted from Hippocrates by Aretæus[7] and also by Paulus Ægineta,[8] but it is probably not to be found in any treatise of the present Hippocratic Collection. In one passage of the 'Aphorisms,' where the word occurs,[9] it has been altered by Ermerins into ἀποπληξία, which is certainly a great improvement of the text, but seems to rest on no authority.

ἀποπληγία, if the reading in Galen is correct, is another form of the more common word ἀποπληξία.[10]

ἀποπληξία, in the ancient medical writers, probably always signifies *cerebral apoplexy*, as distinguished from the other applications of the term found in later authors. It differs from παραπληγία as affecting the whole body, whereas παραπληγία affects only a part,[11] in which it agrees very nearly with παράλυσις.[12] This distinction, however, is not observed in the Hippocratic Collection, in which we find ἀποπληξία applied to different parts of the body.[13] The principal passages in the old medical writers relating to apoplexy are mentioned in a note to Theophilus;[14] an analysis of their opinions on the nature of the disease and its treatment is given by Adams in his Commentary on Paulus Ægineta;[15] and there is a good note on the meaning of the word ἀποπληξία by Dr. Daremberg, in his 'Œuvres Choisies d'Hippocrate.'[16]

1 'Coac. Praen.,' §§ 467, 468, 469, tome v. p. 688, ed. Littré; Aretæus, 'De Cur. Morb. Acut.,' i. 5, p. 217, l. 5, ed. Kühn; Galen, 'Defin. Med.,' c. 245, tom. xix. pp. 415, 416.

2 Hippocr., 'Prorrhet.,' i. 82; 'Coac. Praen.,' §§ 466, 470, tome v. pp. 530, 688.

3 See Facciolati 'Lex. Lat.'

4 'De Cap. Vulner.,' c. 19, tome iii. p. 254, l. 9, ed. Littré; 'Aphor.' vi. 57, tome iv. p. 578; 'Coac. Praen.,' § 157, tome v. p. 618; 'De Morb.,' ii. 6, tome vii. p. 14, l. 13.

5 'De Cur. Morb. Acut.,' i. 4, p. 210, l. 13; p. 214, l. 14, ed. Kühn.

6 'Morb. Acut.,' i. 15, § 123, p. 48; iii. 5, §§ 54, 55, p. 201, ed. Amman.

7 'Sign. Morb. Chron.,' i. 7, p. 85, l. 6; and Wigan's Note, p. 744, in ed. Kühn.

8 Lib. iii. cap. 18, p. 31, l. 25, ed. Ald.

9 Sect. iii. § 16, tome iv. p. 492, ed. Littré; vol. i. p. 412, ed. Ermerins.

10 'Comment. in Hippocr. "Prorrhet. I."' ii. 84, tom. xvi. p. 672, l. penult.

11 Aretæus, 'De Caus. Morb. Chron.,' i. 7, p. 62, ed. Adams; Galen, 'Defin. Med.,' c. 245, tom. xix. p. 415, and elsewhere.

12 Galen, 'De Locis Affect.,' iii. 14, tom. viii. p. 208.

13 'Coac. Prænot.,' § 353, tome v. p. 658, ed. Littré.

14 'De Corp. Hum. Fabr.,' p. 185, l. 12, ed. Oxon.

15 Lib. iii. cap. 18, vol. i. p. 397, &c.

16 p. 258, 2nd edition.

The word had not been Latinized when Celsus wrote,[1] but seems to have been so in the time of Cælius Aurelianus, who uses it without observation.[2]

ἀπόπληξις τοῦ σώματος[3] is synonymous with ἀποπληξία τοῦ σώματος.

ἀποσιτικά (τά), a Hippocratic word, explained by Galen[4] to mean things *that excite aversion for food*, ἀποσιτίαν.

ἀπόσιτος, another Hippocratic word, meaning a person *having an aversion for food*, ἀποσιτίαν.[5]

ἀποσιτία, also found in the Hippocratic Collection,[6] and explained by Galen, to mean *an aversion for food*, as distinguished from ἀσιτία, which means merely *a want of appetite*.[7] The word appears to be used by Galen only in connexion with the writings of the Hippocratic Collection:[8] in one passage, where the present reading is ἀσιτία, we should probably read ἀποσιτία,[9] as in the Aphorism to which it relates.

ἀπόσκημμα, according to Galen,[10] signifies a collection of morbid humours, which has passed from one part of the body to another, in which sense it is used by him several times.[11] It is perhaps nearly synonymous with the French *dépôt* (the word suggested by Dr. Daremberg in his translation of Galen),[12] and is at the same time distinguished from ἀπόστημα, *abscess*.[13] In one passage,[14] it is said by some lexicographers to signify *the excrements*, but this interpretation is at least doubtful, as the sense given above seems to suit the context tolerably well, and in no other passage has it been supposed to have this latter signification.

ἀπόστημα is (etymologically) an affection in which parts previously in contact are separated (ἀφίσταται) from each other;[15] and the derivation of the Latin equivalent *abscessus* is similar. It is an old word, often met with, but used in a more extended sense than the modern *abscess*. Paulus Ægineta[16] (making use of some earlier writer, who is also transcribed by Oribasius)[17] says that an abscess is a corruption and change of the flesh or fleshy parts, such as muscles, veins, and

[1] He says, "ἀποπληξίαν hunc [morbum] Græci appellant," iii. 26, p. 117, ed. Daremberg.

[2] 'De Morb. Acut.,' iii. 5.

[3] Hippocrates, 'Aphor.,' vi. § 56, tome iv., p. 576, ed. Littré.

[4] 'Gloss. Hippocr.,' tom. xix. p. 84.

[5] Galen, 'Comment. in Hippocr. "Prorrhet. I."' i. 71, tom. xvi. p. 654, l. penult.; 'Comment. in Hippocr. "Aphor.,"' tom. xvii. B. p. 527, l. 8; tom. xviii. A. p. 11, ll. 14, 15.

[6] 'Aphor. vi. 3, tome iv. p. 564, ed. Littré, and elsewhere.

[7] 'Comment. in Hippocr. "Epid. I.,"' i. 30, tom. xvi. p. 74. See also Dietz, 'Schol. in Hippocr. et Gal.,' vol. ii. p. 395.

[8] 'Comment. in Hippocr. "Aphor.,"' ii. 32, tom. xvii. B. p. 527, l. 12.

[9] 'Comment. in Hippocr. 'Aphor.,"' iv. 17, tom. xvii. B. p. 677, l. 1.

[10] 'Ad Glauc. de Meth. Med.,' ii. 9, tom. xi. p. 116, l. 9, &c.

[11] 'De Anat. Admin.,' iii. 1; iv. 1; vii. 13, tom. ii. p. 340, l. 6; 417, 7; 634, 2.

[12] 'Œuvres,' &c., tome ii. p. 769.

[13] 'De Anat. Admin.,' i. 3, tom. ii. p. 228, ll. 15, 16.

[14] Galen, 'Comment in Hippocr. "Prognost.,"' ii. 13, tom. xviii. B. p. 133, l. 4.

[15] Galen, 'Ad Glauc. de Meth. Med.,' ii. 9, tom. xi. p. 115; transcribed by Oribasius, 'Coll. Med.,' xlv. 1, tome iv. p. l. ed. Daremberg.

[16] Lib. iv. cap. 18, vol. ii. p. 56, ed. Adams.

[17] 'Coll. Med.,' xliv. 6, tome iii. p. 564, ed. Daremberg.

arteries; that some are contained in a kyst (χιτών), as ἀθέρωμα, στεάτωμα and μελικηρίς, and others are formed without a kyst, which are called by the generic name ἀπόστημα. Celsus uses the word *abscessus*, adding "quæ ἀποστήματα Graeci nominant."[1] Cælius Aurelianus, however, uses *apostema*, "quod etiam *vomicam* dicere poterimus;"[2] though in another passage he says that the Greek synonym for *vomica* is ἐμπύημα.[3]

ἀποσχίς, a word found in the Hippocratic Collection,[4] signifying a *small branch* applied to a vein. It is generally found in the plural (but not always,[5]) and is applied by Galen to arteries and nerves.[6]

ἀποφλεγματίζω, *to excite the secretion or discharge of* φλέγμα, that is, the saliva, nasal mucus, &c. The word is generally applied to medicines, ἀποφλεγματίζοντα,[7] and includes both gargles and masticatories. Sometimes, however, it is applied to the physician, and signifies, *to give an apophlegmatic medicine.*[8]

ἀποφλεγματικὴ δύναμις, *the power or faculty of secreting or discharging* φλέγμα.[9]

ἀποφλεγματισμός, properly *the secretion or discharge of* φλέγμα,[10] but more commonly used to signify a medicine calculated to promote this secretion or discharge, whether used in the form of a gargle (or mouthwash) or a masticatory.[11] It is used as a Latin word by Cælius Aurelianus.[12] In a passage of Antyllus quoted by Oribasius,[13] and also in Paulus Ægineta,[14] the word is translated *masticatory* both by Daremberg and by Adams. It would, however, seem to be by inadvertence that they have thus restricted the meaning; for 1, Dr. Daremberg, in his useful note (p. 812) says himself, "Les apophlegmatismes étoient des masticatoires *ou des collutoires*," &c.; and 2, Paulus Ægineta directs the medicine which he describes to be used (not as a masticatory, but) as *a gargle.*[15]

[1] 'De Med.,' ii. 1, p. 28, l. 29, ed. Daremberg.

[2] 'Morb. Acut.,' ii. 14, § 94, p. 113, ed. Amman. [3] Ibid., ii. 17, p. 117.

[4] 'Epid. II.,' 4, § 1, tome v. p. 122, l. 7, ed. Littré; 'De Oss. Nat.,' §§ 6, 7, tome ix. p. 172, ll. 5, 20; p. 174, l. 1; § 10, p. 178, l. 14.

[5] See Galen, 'De Anat. Admin.,' vi. 12, tom. ii. p. 578, l. 10.

[6] 'De Usu Part.,' ix. 4, 15, tom. iii. p. 701, l. 1; p. 745, l. 1, quoted by Oribasius, 'Coll. Med.,' xxiv. 1, § 45; xxv. 58, § 27, tome iii. p. 288, l. 13; p. 489, l. 13, ed. Daremberg.

[7] Galen, 'De Simplic. Medic. Temper. ac Facult.,' v. 20, tom. xi. p. 769, l. 4; 'De Compos. Medic. sec. Loc.' ii. 2, tom. xii. p. 566, l. 3; p. 586, l. 14; p. 589, l. 5; Antyllus, quoted by Oribasius, 'Coll. Med.,' viii. 10, tome ii. p. 183, l. 10, ed. Daremberg.

[8] Pseudo-Galen, 'De Remed. Parab.,' iii. tom. xiv. p. 512, ll. 2, 11.

[9] Galen, 'De Simplic. Medic. Temper. ac Facult.,' v. 20, tom. xi. p. 769, l. 5.

[10] Galen, 'De Compos. Medic. sec. Loc.' ii. 2, tom. xii. p. 589, l. 3.

[11] See the Index to Kühn's Galen, and to H. Stephani, 'Med. Artis Princ.'

[12] 'Morb. Chron.,' ii. 4, § 2, p. 375, ed. Amman.

[13] 'Coll. Med.,' viii. 10, tome ii. p. 183, ed. Daremberg.

[14] Lib. iii. cap. 5, p. 27 B. l. 37, ed. Ald., vol. i. p. 357, ed. Adams.

[15] Will Dr. Daremberg excuse me for suggesting that the passage which he quotes in his note contains the words of *Galen*, not of *Archigenes*, as he supposes? The words quoted by Galen from Archigenes appear to end just one word before the passage quoted by Dr. Daremberg. Galen then goes on to speak in his own person, and he quotes the same passage from Archigenes again at tom. xii. p. 582, ed. Kühn.

ἀπόφυσις, literally *an offshoot*, was applied generally (as in modern anatomy,) to a bony protuberance; not, however, exclusively, as we meet with it applied to the *branches* of a *nerve*,[1] to the *cerebral nerves*,[2] to a *branch* of the *vena cava*,[3] to the male *urethra*, as being a *prolongation* of the bladder,[4] and (if the reading in Galen is correct,[5]) to the *labia* and *prepuce*. In one passage of Aretæus[6] it seems to mean simply *prominence*,[7] applied to induration of the peritoneum; and in one passage of Soranus[8] it is translated "*origine*" by Dr. Daremberg, and "*productio*" by Rasarius.

ἀπόφυσις is often apparently confounded with *ἐπίφυσις*; sometimes, no doubt, by the fault of the copyist, sometimes, perhaps, because the writer did not care to preserve the usual distinction between the two words, and sometimes because neither word seemed to be more appropriate than the other. The distinction between the two words is thus stated by Galen[9] "an *ἐπίφυσις* is the union of one bone with another, an *ἀπόφυσις* is a part of the whole bone." Sometimes *ἀπόφυσις* and *ἔκφυσις* appear to be used indiscriminately to signify a *process*.[10]

The following are the principal combinations in which the word is used by the old medical writers:—

ἀπόφ. ἀγκιστροειδής, *the hook-shaped offshoot*, i.e., probably the *coracoid process* of the scapula. See *ἀγκιστροειδής*.[11]

ἀπόφ. ἀγκυροειδής, *the anchor-shaped offshoot*, probably synonymous with the preceding term. See *ἀγκυροειδής*.

ἀπόφ. ἀνάντης τοῦ δευτέρου σπονδύλου, *the ascending offshoot of the second vertebra*,[12] means probably the *odontoid process* of the axis. The *ἀποφύσεις ἀνάντεις*[13] of the vertebræ in general appear to mean the *superior oblique* or *articulating processes*, opposed to the *ἀποφύσεις κατάντεις*.

ἀποφύσεις ἀραχνοειδεῖς, *cobweb-like offshoots*,[14] applied to nerves, signify nervous filaments. So also *ἀπόφυσις ἀραχνοειδεστάτη*.[15]

[1] Galen, 'De Anat. Admin.,' iii. 4, tom. ii. p. 368, ll. 7, 11, 13.

[2] Galen, 'De Usu Part.,' ix. 9, tom. iii. p. 722, l. 2, copied by Oribasius, 'Coll. Med.,' xxv. 58, § 4, tome iii. p. 483, l. 7.

[3] Theophilus, 'De Corp. Hum. Fabr.,' ii. 13, § 11, p. 83, l. 1, ed. Oxon.

[4] Galen, 'De Usu Part.,' xv. 3, tom. iv. p. 222, l. ult., copied by Theophilus, v. 24, § 3, p. 220, l. 8.

[5] Loco cit., p. 223, ll. 6, 9. Theophilus, in the corresponding passage, p. 220, l. 14, has *ἐπιφύσεις*.

[6] 'De Caus. et Sign. Morb. Chron.,' i. 13, p. 77, l. 17, ed. Adams.

[7] Adams (p. 320, l. 12) translates it "*process*," which hardly seems to make sense.

[8] 'De Morb. Mul.,' cap. 5, p. 13, l. 11, ed. Dietz; transcribed by Oribasius, 'Coll. Med.,' xxiv. 32, § 5, tome iii. p. 379, l. 13.

[9] 'De Oss.,' procem., tom. ii. p. 733, l. 16; transcribed by Oribasius ('Coll. Med.,' xxv. 2, § 7, tome iii. p. 394, l. 2), Meletius ('De Nat. Hom.,' p. 32, l. 20, ed. Cramer), &c.

[10] Hippocrates, 'De Artic.,' § 45, tome iv. p. 190, l. 13; p. 194, l. 10, ed. Littré; Galen, 'De Oss.,' capp. 1, 6, tom. ii. p. 745, l. 14; p. 755, l. 4, copied by Oribasius, 'Coll Med.,' xxv. 3, § 17; 7, § 2, tome iii. p. 398, l. 13; p. 402, l. 11.

[11] In the October No. 1864, where by an oversight it is rendered *anchor-shaped*.

[12] Galen, 'De Admin. Anat.,' iv. 8, tom. ii. p. 462, l. 1.

[13] Galen, 'De Oss.,' cap. 8, tom. ii. p. 758, l. 5; copied by Oribasius, 'Coll. Med.,' xxv. 9, § 12, tome iii. p. 407, l. 7.

[14] Galen, 'De Admin. Anat.,' iii. 10, tom. ii. p. 400, l. ult.

[15] Id., *ibid.*, iii. 4, tom. ii. p. 366, l. 17.

ἀποφύσεις ἄτακτοι, *irregular offshoots*,[1] applied to a vein, signify its extreme subdivisions.

ἀπόφ. βελονοειδής, *the needle-shaped offshoot*, i.e. *the styloid process* of the temporal bone, called also γραφιοειδής and στυλοειδής.[2]

ἀπόφ. γραφιοειδής, or perhaps γραφειοειδής, sometimes (but less accurately,) written γραφοειδής,[3] *the pencil-* (or *stile-*) *shaped offshoot*, so called because some persons compared it γραφείων πέρασι.[4] It signifies sometimes the *styloid process* of the temporal bone, sometimes the *styloid process* of the ulna. When applied to the temporal bone it is synonymous with βελονοειδής,[5] and στυλοειδής; and these processes are sometimes called γραφιοειδεῖς ἐκφύσεις.[6] The styloid process of the ulna is called indifferently ἀπόφ. γραφιοειδής and ἀπόφ. στυλοειδής.[7]

ἀποφύσεις ἐγκάρσιαι, *the cross offshoots*, synonymous with ἀποφύσεις πλάγιαι, signify *the transverse processes* of the vertebræ.[8]

ἀποφύσεις κατάντεις, *the descending offshoots*, applied to the vertebræ, signify the *inferior oblique or articulating processes*, opposed to the ἀποφύσεις ἀνάντεις.[9]

ἀπόφ. κορακοειδής, *the crow's-beak-shaped offshoot*, one of the processes of the scapula, probably that which is still called the *coracoid* process. It was also called ἀπόφ. ἀγκυροειδής;[10] though in one passage Galen uses the name as synonymous with ἀκρώμιον, and distinguished from the ἀπόφ. ἀγκυροειδής.[11]

ἀπόφ. μαστοειδής, *the udder-shaped offshoot*, signifies, 1. One of the processes of the temporal bone, which still retains the name of *mastoid*.[12] This is sometimes called ἔκφυσις μαστοειδής.[13]—2. The words are used by Leo[14] in a very different sense, and have been supposed by the editor to signify the *frontal sinus;* but this (as was pointed out to me by the late Dr. Adams) is no doubt a mistake. Leo says there are seven pairs of cerebral nerves " besides (χωρίς) the so-called *mastoid*

[1] Id., *ibid.*, iii. 12, tom. ii. p. 406, l. 12.

[2] Galen, 'De Oss.,' cap. i. tom. ii. p. 745, l. 11, quoted by Oribasius, 'Coll. Med.,' xxv. 3, § 17, tome iii. p. 398, l. 11; Galen, 'De Usu Part.,' vii. 19. tom. iii. p. 592, l. 4.

[3] In Galen it is written γραφοειδής, and this form alone appears in Liddell and Scott's Lexicon; in Oribasius it is written γραφιοειδής.

[4] Galen, 'De Usu Part.,' vii. 19, tom. iii. p. 592, l. 2.

[5] Galen, 'De Usu Part.,' vii. 19, tom. iii. p. 592, ll. 3, 4; Id. 'De Oss.,' cap. i. tom. ii. p. 745, l. 11, copied by Oribasius, 'Coll. Med.,' xxv. 3, § 17, tome iii. p. 398, l. 12.

[6] Oribasius, 'Coll. Med.,' xxv. 8, § 3, tome iii. p. 404, ll. 1, 5.

[7] Galen, 'De Anat. Admin.,' i. 5, 10, tom. ii. p. 252, l. 18; p. 271, l. 18.

[8] Galen, 'De Usu Part.,' xiii. 3. 9, tom. iv. p. 89, l. 2; p. 117, l. 8.

[9] Galen, 'De Oss.,' capp. 8, 11, tom. ii. p. 758, l. 5; p. 762, l. 1, copied by Oribasius, 'Coll. Med.,' xxv. 9, § 12; 10, § 1, tome iii. p. 407, ll. 7, 10.

[10] Galen, 'De Anat. Admin.,' i. 11, tom. ii. p. 275, l. 1; 'Comment. in Hippocr. "De Artic.," i. 2, tom. xviii. A. p. 306, l. 13; 'De Oss.,' c. 14, tom. ii. p. 766, l. ult., quoted by Oribasius, 'Coll. Med.,' xxv. 13, § 5, tome iii. p. 411, l. 5.)

[11] 'De Usu Part.,' xiii. 12, tom. iv. p. 132, l. 17. In this passage (l. 16), for ὑποφύσεων we should probably read ἀποφύσεων, though the word ὑπόφυσις is recognised by Liddell and Scott.

[12] Galen, 'De Oss.,' c. 6, tom. ii. p. 755, l. 4, copied by Oribasius, 'Coll. Med.," xxv. 7, § 2, tome iii. p. 402, l. 11.

[13] Galen, *ibid.*, p. 745, l. 14, copied by Oribasius, p. 398, l. 13.

[14] 'Consp. Medic.' iii. 1, in Ermerins, 'Anecd. Med. Gr.,' p. 127.

offshoots (μαστοειδῶν ἀποφύσεων)," and it is inconceivable that he should be referring here to the frontal sinus, even if the word ἀπόφυσις could be applied to such a cavity. The term probably signifies the *olfactory nerve*, which is not reckoned among the nerves by Galen, who calls it merely an ἀπόφυσις of the brain,[1] and who also enumerates only seven pairs of cerebral nerves.[2] Theophilus is probably the earliest extant writer who recognises the olfactory nerves as the first pair of cerebral nerves;[3] but later authors for many centuries call them "processus mammillares," and other similar names.[4]

ἀπόφ. ὀδοντοειδής, *the tooth-shaped offshoot*, i.e., the *processus dentatus* (or *odontoides*) of the second cervical vertebra, called sometimes simply ὀδούς,[5] and by the more modern writers in Galen's time ἀπόφυσις πυρηνοειδής.[6] A full description of this bone and its movements is given by Galen, *De Usu Part.* xii. 7, tom. iv. p. 23, &c.

ἀποφύσεις ὀστοειδεῖς, the *bone-like offshoots*, a name applied by the Author of the *Introductio* (printed among Galen's[7] works), to two parts of (apparently) the occipital bone, "not far apart, through which descend tendons and nerves, τένοντες καὶ νεῦρα:" a description too vague to enable us to determine what parts the writer intends to speak of, even if we give him credit for having himself a distinct idea of the processes which he is describing.

ἀποφύσεις πλάγιαι, *the side offshoots*, signify the *transverse processes* of the different vertebræ.[8] Sometimes they are called ἀποφύσεις εἰς τὸ πλάγιον,[9] or ἀπὸ τῶν πλαγίων,[10] or ἐκ τῶν πλαγίων.

ἀποφύσεις πτερυγωδεῖς or πτερυγοειδεῖς, *the wing-shaped offshoots*, two of the processes of the sphenoïd bone;[11] but the description of this very irregular bone is not sufficiently distinct to enable me to say with certainty whether the words mean the *pterygoïd processes* of modern writers, or (as Dr. Daremberg[12] explains them), *les grandes ailes*, or *temporal processes*. They are probably the same parts that are called also ἐκφύσεις πτερυγωδεῖς.[13]

[1] 'De Usu Part.,' viii. 6; ix. 8, tom. iii. p. 640, l. 4; p. 713, l. 11; 'De Odor. Instr.,' c. 2, tom. ii. p. 859, l. 6, copied by Oribasius, 'Coll. Med.,' xxiv. 5, tome iii. p. 304, line 9; and elsewhere.

[2] See Index to Kühn's edition of Galen.

[3] 'De Corp. Hum. Fabr.,' iv. 12, § 1, ed. Oxon.:—see Note to p. 151, l. 8.

[4] e.g., Vesalius, 'De Hum. Corp. Fabr.,' iv. 3. See Sprengel, 'Hist. de la Méd.,' tome iv. p. 69.

[5] Galen, 'Comment. in Hippocr. "Prorrhet. I.,"' ii. 89, tom. xvi. p. 681, l. 7; 'Comment. in Hippocr. "Epid. II.,"' ii. 29, tom. xvii. A., p. 374; Palladius, 'Comment. in Hippocr. "Epid. VI.,"' vii. 9, tom. ii. p. 192, l. 11, in Dietz, 'Schol. in Hipp. et Gal.'

[6] Galen, 'De Oss.,' c. 8, tom. ii. p. 756, ll. 13, 17; p. 757, l. 7, copied by Oribasius, 'Coll. Med.,' xxv. 9, §§ 7, 9, tome iii. p. 405, l. 11; p. 406, ll. 1, 7.

[7] Cap. 12, tom. xiv. p. 720, l. ult.

[8] Galen, 'De Oss.,' cap. 8, tom. ii. p. 758, l. 11. See also *ibid.*, cap. 13, p. 763, l. 17; p. 764, l. 4.

[9] Id., *ibid.*, p. 758, l. 4; p. 762, l. 3, copied by Oribasius, 'Coll. Med.,' xxv. 9, § 12; 10, § 1, tome iii. p, 407, ll. 6, 12.

[10] Id., *ibid.*, cap. 20, p. 772, l. 8, copied by Oribasius, xxv. 19, § 1, p. 416, l. 3.

[11] Galen. 'De Oss.,' c. 1, tom. ii. p. 743, l. 11.

[12] Oribasius, 'Coll. Med.,' xxv. 3, § 12, tome iii. p. 397, l. 6.

[13] Galen, 'De Anat. Admin.,' iv. 4, tom. ii. p. 439, l. 7; p. 441, l. 10; 'De Musc. Dissect.,' c. 7, tom. xviii. B., p. 935, l. 7, copied by Oribasius, 'Coll. Medic.,' xxv. 30, § 5, tome iii. p. 429, l. 13.

ἀπόφ. πυρηνοειδής, (or πυρινοειδής), *the fruit-stone-shaped offshoot*, a later name for the *processus dentatus* (or *odontoides*) of the second cervical vertebra, which by earlier writers was called ἀπόφ. ὀδοντοειδής, or simply ὀδούς.[1]

ἀπόφ. σιγμοειδής, *the sigma-shaped* (*or semi-circular*) *offshoot*,[2] is used by Galen as synonymous with ἀπόφ. ἀγκυροειδής, in a passage [3]where it certainly seems to signify the *coracoid process* of the scapula. In this same passage it is (apparently) distinguished from the ἀπόφ. κορακοειδής, which is elsewhere said to be synonymous with ἀπόφ. ἀγκυροειδής [4]; so that, if the reading in Galen is correct (and if he is not using the word in the sense given to it by other anatomists, rather than by himself,) there would seem to be some confusion in one of his descriptions. It may be added that in one passage the ἀπόφ. ἀγκυροειδής is described as being ἔξωθεν to the scapula[5]; in another it is said to be ἔσωθεν[6], which latter reading is probably the correct one, as it seems to agree better with the actual form of the scapula.

ἀπόφ. (or ἐπίφ.), σκωληκοειδής, *the worm-shaped offshoot* (or *ongrowth*), the name given to a part of the brain, possibly to that which is still called the *vermiform process*, or *appendix*. In the present editions of Galen it is sometimes called ἀπόφυσις,[7] sometimes ἐπίφυσις;[8] and as neither of the words is more suitable to the part intended than the other, it is at present scarcely possible to say which is the more correct. Caspar Hoffmann has discussed the point,[9] and decides in favour of ἀπόφυσις; but probably Dr. Daremberg is right in saying that the question must be determined (at least in a great degree) by the aid of Greek MSS.—The *appendices vermiformes* are sometimes said to be so called from their resemblance to *earth-worms*; but Galen compares them τῷ κατὰ τὰ ξύλα γεννωμένῳ, σκώληκι,[10] *to the worm that is engendered in timber*.

ἀπόφ. στυλοειδής, the *stile-* (or *pencil-*) *shaped offshoot*, signifies sometimes the *styloid process* of the temporal bone, sometimes the *styloid process* of the ulna. The spelling of the word is somewhat doubtful, as it is not quite certain whether it should be written στηλοειδής, or στιλοειδής, or στυλοειδής, all of which words were probably pronounced more alike by the ancients than by us. Rufus Ephesius[11] calls the

[1] Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 37, l. 13, ed. Clinch, ; and Theophilus, 'De Corp. Hum. Fabr.,' v. 4, § 4, p. 191, ed. Oxon. See also, ἀπόφ. ὀδοντοειδής, and the passages there referred to.

[2] It will be remembered that the form of the 'Sigma' intended is not Σ, nor σ, nor ς, but C.

[3] 'De Usu Part.,' xiii. 12, tom. iv. p. 133, l. 1.

[4] See these two words in these 'Adversaria.' [5] Galen, 'De Usu Part.' *loco cit.*

[6] Galen, 'De Oss.,' c. 14, tom. ii. p. 766, l. 17; copied by Oribasius, 'Coll. Med.,' xxv. 13, § 5, tome iii. p. 411, l. 4.

[7] 'De Anat. Admin.,' ix. 5, tom. ii. p. 730, l. 16; 'De Usu Part.,' viii. 14, tom. iii. p. 676, l. 10; 677.13; 'Introd.,' c. 11, tom. xiv. p. 711, l. 4.

[8] Tom. ii. p. 729, l. 17; 730.12; tom. iii. p. 678, ll. 11, 16; 680.15; 681.14; 711.10.

[9] 'Comment. in Gal. "De Usu Part.,"' p. 204, quoted by Daremberg, 'Œuvres Choisies de Galien,' tome i. p. 565, note.

[10] 'De Anat. Admin., ix. 5, tom. ii. p. 729, l. 16.

[11] 'De Appell. Part. Corp. Hum.' p. 35, l. 13, ed. Clinch.

styloid process of the temporal bone στηλοειδής, deriving the word (if the reading be correct), from στήλη, *a post.* Galen calls it στυλοειδής,[1] using the word as synonymous with βελονοειδής and γραφιοειδής. In one place he says that some persons compare it to the end of *a pencil* or *stilus* (γραφεῖον), and call it by the *barbarous* (βαρβαρίζοντες) name στυλοειδής.[2] Dr. Daremberg supposes,[3] with great probability, that the solecism which offended Galen was the joining together in one hybrid word the Latin *stilus* with the Greek εἶδος; and would write the word στιλοειδής, for which spelling there is some MS. authority, and which is perhaps somewhat confirmed by the Arabic mode of expressing the word—viz., سطيلويدس not سطولويدس. But Dr. Daremberg is not correct in supposing that the styloid process of the ulna (which is another meaning of ἀπόφυσις στυλοειδής),[4] is not compared to a stile or pencil, as Galen says it was called indifferently στυλοειδής and γραφιοειδής.[5]

ἀποφύσεις τριχοειδεῖς, *hair-like offshoots*, is an expression applied to the ramifications of the veins,[6] which has been translated and retained in the word *capillary.*

ἀποφύσεις χονδρωδεῖς, *cartilaginous offshoots,* the expression applied to the cartileges of the false ribs.[7]

ἀραχνοειδής, *like a cobweb,* is used in combination with several words. Sometimes it is applied to the veins, and signifies a greater degree of tenuity than τριχοειδής;[8] sometimes to the nerves, when the superlative (ἀραχνοειδέστατος), is also used;[9] sometimes to the pulse, when so small and unsteady that it was fancifully compared to a cobweb gently shaken by the wind;[10] sometimes to the urine, when fatty substances like cobwebs float on the surface.[11]

It is most frequently joined to χιτών, when it probably never signifies the arachnoid membrane of the *brain* (as in modern anatomical works), but one of the membranes of the *eye.* This term is as old as Herophilus,[12] and is sometimes used as synonymous with ἀμφιβληστροειδής and ὑαλοειδής, sometimes as meaning something different—viz., either the *retina* or the *capsule of the crystalline lens.*[13] But the

[1] 'De Oss.,' c. i. tom. ii. p. 745, l. 11, quoted by Oribasius, 'Coll. Med.,' xxv. 3, § 17, tome iii. p. 398, l. 11.

[2] 'De Usu Part.,' vii. 19, tom. iii. p. 592, l. 3.

[3] Note to 'Œuvres d'Oribase,' tome iii. p. 702, l. 34.

[4] Galen, 'De Oss.,' cc. 17, 18, tom. ii. p. 769, l. ult.; 770.14, 15; 771.5 (quoted by Oribasius, 'Coll. Med.' xxiv. 16, § 6; 17, § 2, tome iii. p. 414, ll. 1, 11); 'De Usu Part.,' ii. 18, tom. iii. p. 166, l. 6.

[5] 'De Anat. Admin.,' i. 5, 10, tom. ii. p. 252, l. 18; p. 271, l. 18.

[6] Galen, *ibid.*, iii. 5, tom. ii. p. 381, l. 10.

[7] Galen, 'De Musc. Dissect.,' tom. xviii. B. p. 965, l. 13, copied by Oribasius 'Coll. Med.,' xxv. 40, § 3, tome iii. p. 451, l. 6.

[8] Galen, 'De Ven. et Arter. Anat.,' cap. 8, tom. ii. p. 808, ll. 6, 12, 14.

[9] Galen, *ibid.*, iii. 4, 10, tom. ii. p. 366, l. 17; p. 400, l. ult.

[10] Galen, 'Defin. Med.,' cap. 227, tom. xix. p. 411, l. 4.

[11] Hippocrates, 'Progn.' § 12, tome ii. p. 142, l. 7; 'Praenot. Coac.,' § 571 (582), tome v. p. 716, l. 13, ed. Littré.

[12] Celsus, vii. 7, § 13, p. 392, ed Targa.

[13] See the note to the Oxford edition of Theophilus, 'De Corp. Hum. Fabr.,' p. 164, l. 7, and the references there given. See also the article ἀμφιβληστροειδής in these 'Adversaria.'

uncertainty of the nomenclature of the different parts of this delicate organ will frequently be noticed.

ἀρθριτικός, a Hippocratic word, signifying sometimes a person suffering from ἀρθρῖτις,[1] sometimes diseases like ἀρθρῖτις, *arthritic*.[2]

ἀρθρῖτις, also a Hippocratic word,[3] is said by Aretæus[4] to signify a general pain of all the joints, comprehending pain of the *feet*, ποδάγρα, that of the *hip-joint*, ἰσχιάς, and that of the *hands*, χειράγρα; to which species Cælius Aurelianus adds that of the *knee*, γονάγρα, and (though apparently with less propriety) that of the *tendons* or *nerves*, τενοντάγρα.[5] The word occurs frequently, and is probably always used to signify gout in its most comprehensive sense.[6]

ἄρθρον, sometimes used generally to signify any kind of *joint*,[7] sometimes more strictly to signify the natural juxtaposition[8] of *moveable* bones, as distinguished from σύμφυσις or ἁρμογή, the union of *immoveable* bones.[9] In this latter sense it comprehended the two great divisions of joints, διάρθρωσις and συνάρθρωσις, the third which is sometimes asssociated with them—viz., *amphiarthrosis*, being a modern term first used by Winslow.[10] Διάρθρωσις, which signified a joint having extensive movement, was divided into ἐνάρθρωσις, ἀρθρωδία, and γίγγλυμος, three words in Galen's time of recent origin;[11] συνάρθρωσις, which signified a joint almost immoveable, comprehended ῥαφή, γόμφωσις, and ἁρμονία. The writer of the book 'Introductio, seu Medicus,' found among Galen's works,[12] uses the words σύμφυσις and συνάρθρωσις differently from Galen, thereby affording another proof of the spuriousness of the treatise. In the Hippocratic Collection the word is sometimes used to signify, not the joint itself, but one of the two bones that form the joint. This is noticed by Erotianus,[13] and by Galen,[14] who says that the convex

1 'Epid.,' vi. 4, § 3, tome v. p. 306, l. 11. ed. Littré. Galen, 'De Comp. Medic. sec. Gen.' vii. 12, tom. xiii. p. 1010, l. ult., and elsewhere.

2 'De Nat. Hom.,' § 14, tome vi. p. 66, l. 7. Galen has ἀρθριτικὰ ἀλγήματα, 'Comment. in Hippocr. "Epid. VI."' iv. 3, tom. xvii. B. p. 125, l. ult., and elsewhere.

3 'Aphor.,' iii. 16, tome iv. p. 492, l. penult.

4 'De Sign. Morb. Chron.,' ii. 12, p. 118, ed. Adams.

5 'Morb. Chron.,' v. 2, § 28, p. 557, ed. Amman. In this passage is it more probable that τένων is used to signify a *nerve*, or *nervus* to signify a *tendon*?

6 Adams has given a reference to the writers on the disease in his Commentary on Paulus Ægineta, iii. 78, vol. i. p. 666.

7 In Galen, 'De Musc. Dissect.,' tom. xviii. B. p. 939, l. 1, the shoulder joint is called τὸ κατ' ὦμον ἄρθρον; and a few lines above (p. 938, l. 15) it is called more precisely ἡ κατ' ὦμον διάρθρωσις.

8 σύνταξις, σύνθεσις, or ὁμιλία ὀστῶν. See Galen, 'De Oss.,' procem., tom. ii. p. 734, l. 13; p. 735, l. 10.

9 Id. 'Defin. Med.,' cap. 470, tom. xix, p. 460, l. 7; 'De Oss.,' p. 734, l. 11, quoted by Oribasius, 'Coll. Med.,' xxv. 2, § 1, tome iii. p. 392, l. 8.

10 Nysten, 'Diction. de Méd.,' in "Amphiarthrose."

11 'De Oss.,' procem., tom. ii. p. 735, l. 12.

12 Tom. xiv. p. 720, l. 6.

13 'Lex. Voc. Hippocr.'

14 'De Oss.,' procem., tom. ii. p. 734, l. ult.; 'Comment. in Hippocr. "De Artic.' i. 1, tom. xviii. A. p. 304, l. ult.

surface is intended, whereas sometimes, at least, it is certainly the concave.[1]

ἀρθρώδης and *ἀρθρωδῶς*, two words of kindred meaning to *ἀρθρωδία*, but of considerably older origin.[2]

ἀρθρωδία, a recently adopted word in Galen's time, which has retained its place in medical terminology to the present day, signifying a species of *διάρθρωσις* formed by a slightly convex surface applied to one slightly concave,[3] and distinguished from *ἐνάρθρωσις* and *γίγγλυμος*.

ἁρμογή, used apparently as synonymous with *σύμφυσις*,[4] to signify the union of bones without motion, opposed to *ἄρθρον*.

ἁρμονία, one of the species of *συνάρθρωσις*, in which the union of the bones takes place by simple apposition of their surfaces.[5]

ἀρρευμάτιστος, applied to medicines that arrest hæmorrhage and other morbid discharges, *astringent*.[6]

ἀρτηρία, a word used in two distinct senses, inasmuch as the ancients spoke of two kinds of *ἀρτηρίαι*[7]—viz., 1, the *ἀρτηρίαι λεῖαι*, or *arteries*; and, 2, the *ἀρτηρίαι τραχεῖαι*, or *bronchia*.[8] If this distinction is borne in mind, much of the apparent confusion caused by the uncertainty of the signification of the word in any particular passage will disappear. The whole subject will, however, always be an obscure and intricate one, and sometimes it will seem doubtful whether the writer himself knew exactly which set of vessels he was writing about; for while the *ἀρτηρίαι* were supposed to convey air to all parts of the body, it was also perfectly well known to many anatomists (though the fact was disputed[9]) that blood issued from them when they were wounded. Much curious and interesting information on the subject will be found in Littré's "Hippocrates," tome i. p. 201, sq.; Daremberg's "Comment. de Galien sur le *Timée* de Platon," p. 43; and the passages indicated in the note to the Oxford edition of Theophilus, "De Corp. Hum. Fabr.," p. 296.

With respect to the derivation of the word, as the *ἀρτηρίαι* were supposed to contain air, (and correctly, when the bronchia were signified,) it is not perhaps very surprising that some persons should have considered it to be a corruption of *ἀεροτηρία*, compounded of *ἀήρ* and *τηρεῖν*. This derivation can be traced back to one of the earliest commentators on the Hippocratic Collection—viz., Baccheius;[10] it has been repeated by other ancient writers,[11] and pertinaciously keeps its ground even in

[1] Hippocr. 'De Locis in Hom.,' § 6, tom. vi. p. 288, l. 10. See Notes to Erotianus, p. 68, ed. Franz, where a reference is given to Foesii 'Œcon. Hippocr.,' which at this moment I am unable to consult.

[2] Galen, 'De Oss.,' prooem., tom. ii. p. 735, ll. 15. 17.

[3] Id., *ibid.*, tom. ii. p. 736, l. 5.

[4] Galen, 'Defin. Med.,' cap. 470, tom. xix. p. 460, l. 7, compared with 'De Oss.,' prooem., tom. ii. p. 734, l. 11.

[5] Galen, 'De Oss.,' prooem. tom. ii. p. 737, ll. 7, 16.

[6] Galen, 'De Compos Medic. sec. Loc.,' vii. 4, tom. xiii. p. 77, ll. 4, 13.

[7] Galen, 'De Anat. Admin.,' vii. 1, tom. ii. p. 590, l. 1 sq.

[8] Id., *ibid.*, and cap. 4, p. 598, ll. 4, 5.

[9] Hence Galen's treatise, entitled 'An in Arteriis Naturâ Sanguis contineatur,' tom. iv. p. 703, sq.

[10] Erotianus, 'Gloss. Hippocr.,' p. 68, ed. Franz.

[11] See Note to Theophilus, p. 296.

respectable books of the present day. As, however, the ancients were deficient in philological knowledge, it may safely be said that no competent scholar will be satisfied with this derivation; nor indeed should any one else accept it, unless he is prepared to follow Dionysius of Syracuse, and explain a *mystery*, μυστήριον, to mean (etymologically) a *mouse-trap*, ὅτι τοὺς μῦς τηρεῖ.[1] The derivation given in Liddell and Scott's Greek Lexicon is from αἴρω, *to raise* or *carry*, which will suit either of its meanings quite sufficiently well, as the lungs may be supposed to be carried or suspended by the trachea, or the heart by the aorta.

The following are some of the combinations in which ἀρτηρία is found:—

ἀρτηρία καρωτίς,[2] or more commonly in the plural ἀρτηρίαι καρωτίδες, or simply καρωτίδες, *the carotid arteries*, so called because the compression of them was supposed to produce stupor, κάρος. This opinion, however, was not universally held,[3] and Galen finds fault with the name, though he made use of it because it was generally adopted in his time.[4]

ἀρτηρία μεγάλη,[5] *the great artery*, or μεγίστη,[6] *greatest*, or ὀρθή,[7] *upright*, or παχεῖα,[8] *thick*, names applied to the *aorta;* the last is attributed to Praxagoras.[9]

ἀρτηρία πνευματική,[10] *the pneumatic artery* (probably used actively, *filling*, not *filled* with wind,) does not mean the *trachea*, (as we should at first sight be inclined to imagine,) but the *aorta*.

ἀρτηρία τραχεῖα,[11] *aspera arteria, the rough* ἀρτηρία, that is, the chief of the rough ἀρτηρίαι, which (as has been explained) were distinguished from the smooth ἀρτηρίαι. The name was applied to the βρόγχος,[12] is constantly used by the ancient writers,[13] and has continued in use (in the form *trachea*,) to the present day.

ἀρτηρία φλεβώδης,[14] *the venous* ἀρτηρία, the name given to the *pulmonary veins*, because, though they have a venous structure, they

[1] Athenaeus, 'Deipnos.,' iii. § 54, p. 98.

[2] Galen, 'De Nerv. Dissect.,' c. 5, tom. ii. p. 835, l. 10, quoted by Oribasius, 'Coll. Med.,' xxv. 58, § 14, tome iii. p. 485, l. 10.

[3] Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 42, l. penult., ed. Clinch.

[4] 'De Hipp. et Plat. Decr.,' i. 7, tom. v. p. 195, l. 6 sq.

[5] Galen, 'De Ven. et Arter. Dissect.,' cap. i. tom. ii. p. 780, l. 10: 'De Anat. Admin.,' v. 8, tom. ii. p. 523, l. 1, quoted by Oribasius, 'Coll. Med.,' xxiv. 24, § 3, tome iii. p. 356, l. 5. The μεγάλη ἀρτηρία ἡ ἐν τῷ τραχήλῳ (Galen, *ibid.*, p. 602, l. penult., quoted by Oribasius, p. 328, l. 10), means, of course, the *trachea;* as also does the *arteria major* mentioned by Cælius Aurelianus, 'Morb. Chron.,' v. 10, pp. 586, 587, ed. Amman.

[6] Galen, 'De Sem.,' i. 8, tom. iv. p. 541, l. 10.

[7] Galen, 'De Anat. Admin,' vii. 1, tom. ii. p. 5 sq.

[8] Aretæus, 'De Caus. Morb. Acut.,' ii. 2, p. 31, l. 4; 'De Cur. Morb. Acut.,' ii. 7, p. 277, l. 13, ed. Kühn.

[9] Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 42, l. antepen., ed. Clinch.

[10] Theophilus, 'De Corp. Hum. Fabr.,' iii. 7. § 2, p. 97, l. 3, ed. Oxon.

[11] Celsus, 'De Med.,' iv. 1, p. 168, ed. Targa.

[12] Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 28, l. 16.

[13] Aretæus, 'De Sign. Morb. Acut.,' ii. 2, p. 28, l. 14; p. 31, l. 9. See Note to the Oxford edition of Theophilus, p. 106, l. 14.

[14] Galen, 'De Anat. Admin.,' vii. 4, tom. ii. p. 600, l. 4.

convey arterial blood (or, as was formerly supposed, *air*). The author of the name is not known, but it may have been Herophilus, as he was the author of the corresponding name, φλὲψ ἀρτηριώδης.[1] The name is frequently met with, and there is a good note on the subject in Dr. Daremberg's translation of Galen, tome i. p. 407, on "De Usu Part.," vi. 10, tom. iii. p. 445.

ἀρτηριακός, of or belonging to an ἀρτηρία, not in the sense of *an artery*, but of an *air-vessel*.

ἀρτηριακὴ κοιλία τῆς καρδίας, called also ἀρτηριώδης and πνευματική, *the left ventricle of the heart*, which Diogenes of Apollonia considered to be the seat of the soul.[2]

ἀρτηριακή (ἀντίδοτος being understood), *a remedy for affections of the trachea and bronchi*, a name in frequent use, synonymous with ἐκλεικτόν.[3]

ἀρτηριακόν (πάθος), an affection of the *trachea*, synonymous with βράγχος,[4] which Cælius Aurelianus[5] renders *raucedo*, or *hoarseness*.

ἀρτηριοτομέω, *to open an artery*, by a surgical operation.[6]

ἀρτηριοτομία, *the opening of an artery*, in surgery, opposed to φλεβοτομία.[7] The operation is mentioned by Aretæus,[8] who, however, does not use the word.

There is a chapter on the subject in Aëtius (taken from Severus),[9] Paulus Ægineta,[10] and Joannes Actuarius.[11]

ἀρτηριώδης, like an ἀρτηρία, probably always in the sense of an *air-vessel*.

ἀρτηριώδης κοιλία τῆς καρδιας,[12] called also ἀρτηριακή and πνευματική, a name given to the left ventricle of the heart.

ἀρτηριώδης φλέψ, *the arterial vein*, the name given by Herophilus to the *pulmonary artery*, because it conveys venous blood, though it resembles an ἀρτηρία in structure.[13] It is in frequent use.

ἀρυταινοειδὴς χόνδρος, the *arytænoid cartilages* of the larynx, which Galen supposed to be but one, instead of two.[14] The name was given from their being compared to an ἀρύταινα, probably a *pitcher* or *ewer*.[15]

[1] Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 42, l. 3, ed. Clinch.

[2] Plutarch, (?) 'De Philos. Decr.,' iv. 5; Pseudo-Galen, 'De Philos. Hist.,' tom. xix. p. 315, l. 10. This was also the opinion of the work 'De Corde,' in the Hippocratic Collection, tome ix. p. 88, § 10, ed. Littré.

[3] Galen, 'De Compos. Medic. Sec. Loc.,' vii. 1, tom. xiii. p. 1 sq.

[4] Paulus Ægineta, iii. 28, p. 39, l. 41., ed. Ald.

[5] 'Morb. Chron.,' ii. 7, p. 380, ed. Amman.

[6] Antyllus, quoted by Oribasius, 'Coll. Med.,' vii. 14, § 2, tome ii. p. 55, l. 7.

[7] Galen, 'De Cur. Rat. per Ven. Sect.,' cap. 22, tom. xi. p. 312, l. 12; Oribasius, *loco cit.*

[8] 'De Cur. Morb. Chron.,' i. 2, p. 295, l. 1; i. 3, p. 303, l. 8.

[9] ii. 3, 90. [10] vi. 4. [11] iii. 2.

[12] Galen, 'De Differ. Puls.,' iv. 8, tom. viii. p. 737, l. ult.; p. 738, l. 4. See also Rufus Ephesius, 'De Appell. Part. Corp. Hum.,' p. 37, l. 22, ed. Clinch.

[13] Rufus Ephesius, p. 42, l. 2.

[14] 'De Vocal. Instrum. Anat.,' cap. 4, tom. iv. p. 220, ed. Chartier; 'De Usu Part.,' vii. 11, tom. iii. p. 556, l. 9; p. 561, l. 5, quoted by Oribasius, 'Coll. Med.,'' xxiv. 9, § 16, tome iii. p. 315, l. 17.

[15] Theophilus appears to have taken it for a larger vessel. 'De Corp. Hum. Fabr., iii. 15, § 3, p. 112, l. 2, ed. Oxon., and Note, p. 299. See also the note to Dr. Daremberg's translation of Galen, tome i. pp. 483, 484.

ἀρυταινοειδὲς πέρας,[1] the upper extremity of the arytænoid cartilages.

ἀρχή, when applied to a disease, means the first period, when it is only in its commencement; the other stages being the ἀνάβασις (or ἐπίδοσις, or αὔξησις), ἀκμή, and παρακμή.[2]

ἆσθμα meant originally *shortness of breath* from any cause. When it was used to signify a disease, it is explained by Celsus[3] as being something greater than δύσπνοια, and less than ὀρθόπνοια; but this distinction does not seem to be always observed. The principal passages relating to asthma are referred to in Adams's Commentary on Paulus Ægineta, iii. 29, vol. i. p. 477.

ἀσθμαίνω means simply *to pant*,[4] probably never *to be asthmatic*.

ἀσθματικός sometimes means a person simply *out of breath*, as from running:[5] sometimes a person affected with asthma, *asthmatic*.[6] The phrase ἀσθματικὰ ὀρθόπνοια occurs in Galen,[7] to signify a complication of asthma and orthopnœa (?).

ἀσκίτης, or ὕδρωψ (or ὕδερος) ἀσκίτης, the name of one of the three species of dropsy generally recognised among the ancients, the other two being ἀνάσαρκα and τυμπανίας (or τυμπανίτης).[8] It was so called because the fluid was contained in the peritonæum, as in a skin or leathern bag, ἀσκός;[9] and the name has been retained to the present day. It differed from tympanites, inasmuch as this was supposed to be formed by a great quantity of air with a small quantity of fluid, whereas in ascites, on the contrary, there is a great collection of fluid with a very small proportion of air.[10]

ἀσκληπιός, the name given to certain medicinal compounds:[11] ἀσκλήπιον, a popular name for piles.[12] Both words are connected with Ἀσκληπιός, *Æsculapius*.

ἀσφυγμία is found in Cælius Aurelianus (if the reading is correct, for the word is not recognised by Liddell and Scott, and possibly does not occur elsewhere), and is explained to mean "pulsus defectio."[13] Whether he considers the word to be merely synonymous with ἀσφυξία, which he also uses,[14] and explains by "pulsus parvitas vel amputatio," there is nothing in the context to determine positively.

1 Galen, *ibid.*, p. 553, l. 8., quoted by Oribasius, *ibid.*, p. 314, l. 8.

2 Galen, 'De Opt. Sectâ,' c. 32, tom. i. p. 193, l. 4; 'De Cris.,' i, 2, 8, tom. ix. p. 551, l. 2; p. 580, l. 16; 'De totius Morbi Tempor.,' c. 3, tom. vii. p. 445, l. 9; 'Comment. in Hippocr. "De Humor.," ' i. 3, ii. 14, tom. xvi. p. 70, l. 13; p. 257, l. 7; Pseudo-Synes., 'De Febr.,' p. 72, l. ult.

3 'De Med.,' iv. 4, § 2, p. 182, ed. Targa.

4 Aretæus, 'Caus. Morb. Chron.,' i. 11, p. 73, l. 14, ed. Adams; Galen, 'Comment. in Hippocr., "Aph.," ' vi. 66, tom. xviii. A. p. 77, l. ult.

5 Galen, 'De Compos. Medic. sec. Loc.,' vii. 6, tom. xiii., p. 106, l. 2.

6 Id. 'De Loc. Affect.' iv. 11, tom. viii. p. 292, l. penult.; p. 295, l. 16.

7 *Ibid.*, p. 295, l. 10.

8 Celsus, 'De Med.,' iii. 21, p. 148, ed. Targa; Cælius Aurelianus, 'Morb. Chron.,' iii. 8, p. 469, ed. Amman; Galen, 'Comment. in Hippocr. "De Diaeta Acut." ' iv. 93, tom. xv. p. 891, l. 11 sq.

9 Galen, 'Comment. in Hippocr., "Aphor.," ' iv. 11, tom. xvii. B. p. 670, l. 7.

10 Paulus Ægineta, iii. 48, p. 48 B., l. 33 sq., ed. Ald.

11 Galen, 'De Comp. Med. sec. Gen.,' v. 13, tom. xiii. p. 841, l. 2; Paulus Ægineta, vii. 13, p. 126, l. 28.

12 Leo, 'Consp. Med.,' v. 11, in Ermerins, 'Anecd. Med. Gr.,' p. 177.

13 'Morb. Acut.,' iii. 2, p. 182, ed. Amman. 14 'Morb. Chron.,' iv. 3, p. 507.

MEDICAL INTELLIGENCE.

Professor Corradi's "Museo Medico."

In our July number for last year we announced the intention of Professor Corradi, of Palermo, to publish a medico-literary journal, to be called "Il Museo Medico." We are sorry to find that the Professor has not received sufficient encouragement to induce him to commence his undertaking, so that the opinion which we ventured to express as to "the impossibility of carrying on a journal devoted *solely* to medical literature," remains unchanged.

The Arabic Translation of Galen's Principal Anatomical Work.

In the April number of last year, when we gave an account of the Arabic translation of Galen's work, "De Anatomicis Administrationibus," we copied the statement that the only MS. known to exist was to be found in the Bodleian Library at Oxford. At the time this statement was originally made (viz., in Dec., 1844), we believe it was strictly correct, as it has been often repeated since that time, both in this country and on the Continent, without contradiction. We have, however, within the last few weeks become aware of the existence of a second MS. of the work, which once belonged to the late Colonel Taylor, formerly the English Resident at Bagdad, and which was purchased, along with other MSS. of his collection, by the Trustees of the British Museum, in April, 1860. The following is the notice of the MS. given by Dr. Rieu in his still unfinished Catalogue, p. 629:—"Cod. MCCCLV. Codex bombycinus in fol., ff. 258: Manu Persica exaratus, A.H. 887, A.D. 1482.—Claudius Galenus, *De Administrationibus Anatomicis*, libri xv., interprete Hunain Ibn Isḥák[1]—

كتاب جالينوس في عمل التشريح ترجمة حنين بن اسحق

Init. قال قد كنت في المدة الاولى من صعودى الى مدينة رومية وضعت كتابا في عمل التشريح

"Singuli libri his foliis incipiunt: ff. 2, 18, 36, 59, 76, 94, 112, 130, 145, 162, 176, 193, 203, 222, 240.

"Hunain interpretis annotationes quaedam textui passim admiscentur. In fine libri primi legitur:

تمت المقالة الاولى من كتاب جالينوس في عمل التشريح في تاريخ يوم الاثنين احدى وعشرين شهر رمضان المبارك سنة سبع وثمانين وثمانماية

i.e., 'Explicit liber primus operis Galeni *De Administrationibus Anatomicis*, feria secunda die vicesimo primo mensis Ramadán, anno 887.'

"Folia tredecim ab initio manum aliam exhibent, saeculi, ut videtur, xvii.

[1] "Ex libris illis quindecim novem tantum primi Graece exstant. Vide Fabric. *Bibl. Graec.* ed Harles, vol. v. p. 407. Exemplar aliud versionis Arabicæ in Bibl. Bodle. asservatur. Vide Uri, p. 135. Confer Wenrich., *De Auct. Graec. Vers.*, p. 245, et Wüstenfeld, *Gesch. der Arab. Aerzte*, p. 28. Casiri ex 'Bibliotheca Philosophorum' versionem operis ejusdem Hubaisho tributi. De Hunain, vide supra, p. 186, not. *f.*"

"Emendationes quaedam in marginibus adscriptae sunt; totum codicem a librario collatum fuisse, haec notula illius manu in calce subnexa attestatur: بلغت المقابلة من اوله الى آخره [Add. 23,406.]"

From a cursory examination of the MS. we are inclined to think that (with the exception that thirteen leaves are written by a later hand) it is as complete and valuable as that in the Bodleian Library, and will be of very great use to the editors of the work. We have only to add that the existence of the MS. was pointed out to us by Dr. Daremberg, the Professor of Medical History at the College of France, who was recently sent to this country on a sort of medico-literary mission by the French Minister of Public Instruction. When in London he went to the British Museum to see if any fresh medical MSS. had been purchased since his former visit, and obtained a sight of the unfinished catalogue, in which he at once observed the description of this MS., than which a more interesting one could hardly have been brought before his notice.

British Rainfall.

Upon this subject we have received the following letter:

"*To the Editor of the 'British and Foreign Medico-Chirurgical Review.'*

"SIR,—I have to ask your readers' attention for a few moments to a request on the above subject, the importance of which in relation to engineering and drainage questions is well known. It is now some years since I began collecting returns of the fall of rain—with what success I will mention presently—but my main difficulty has been to find out the persons who keep such records, and one of the most obvious sources of assistance is the public press; I now, therefore, ask from each and every journal in the British Isles their all-powerful aid. When the collection was first organized in 1860, scarcely 200 persons were known to observe and record the rainfall; by steady perseverance, and the aid of a portion of the press, the number has been raised until there are now more than 1200 places whence returns are regularly received. Still I know there are many more, probably hundreds, who have either never heard of the establishment of a central depôt to which copies of all rain records should be sent or they have been too diffident to send them. It is of paramount importance to gather these, and make the tables yet more complete, I therefore beg leave through your columns to ask every reader to think for a moment if he or she knows of any one who keeps, or has kept, a rain-gauge; or who has any tables of rainfall (or old weather journals) in their possession. And if they do know of such persons, I ask them on behalf of science, of my fellow-observers, and on my own behalf, to use every effort to secure their assistance, and to favour me with their names and addresses. We want old records, we want records for the present year, and from many parts of the country we want returns for the future, if a few persons will notify to me their willingless to assist, and to pay 10*s.* 6*d.* for the very cheap and simple gauge now supplied.

"To prevent needless correspondence, I annex a list of the places in Middlesex whence returns have been already collected for the years mentioned in the last column, and shall be very glad of any additions or corrections. Other counties, or the complete list for the whole country shall be sent to any one willing to make good use of it. I may add that an influential committee of the British Association has been appointed to superintend and assist in my investigations, and that they cordially support my present application.

"I am, sir, your obedient servant,

"G. J. SYMONS.

"136, Camden Road, London, N.W.

"The committee consists of J. Glaisher, Esq., F.R.S.; Lord Wrottesly, F.R.S.; Prof. Phillips, F.R.S.; Prof. Tyndall, F.R.S.; Dr. Lee, F.R.S.; J. F. Bateman, Esq., F.R.S.; R. W. Mylne, Esq., F.R.S., and myself."

[Considering the intimate connexion which exists between atmospheric phenomena and many types of disease, and the benefit often experienced from change of residence from dry districts to humid ones, and *vice versâ*, we have much pleasure in inserting Mr. Symons's appeal for aid in the collection of rain registers. Mr. Symons makes no reference to more substantial aid, but we regret to find from his last annual publication, that he has had to bear nearly all the cost of organizing his large corps of "1200 observers," and of collating and publishing their returns. It is too much to expect any gentleman to continue so expensive and extensive a work singlehanded, and we trust his Committee will remember this, and that others will lend a hand to this useful work.]

BOOKS, PAMPHLETS, &c., RECEIVED FOR REVIEW.

Medico-Chirurgical Transactions. Second Series. Vol. XIII. London, Longmans and Co. 1865. pp. 340.

Outlines of Elementary Botany. For the use of Students. By Alexander Silver, M.D., Assistant to the Professor of Materia Medica and of Medical Jurisprudence in the University of Aberdeen. London, Renshaw. 1866. pp. 381.

On the Cure of Club-foot without Cutting Tendons; and on Certain New Methods of Treating other Deformities. By R. Barwell, F.R.C.S., Assistant-surgeon, Charing-cross Hospital, &c. Second Edition. London, Hardwicke. 1865. pp. 231.

On the Treatment of Affections of the Throat and Lungs by Inhalation; with a Paper on the Treatment of Whooping Cough. By W. Abbotts Smith, M.D., &c. Second Edition. London, Hardwicke. 1865. pp. 402.

On Flooding after Delivery, and its Scientific Treatment; with a Special Chapter on the Preventive Treatment. By L. Earle, M.D., Obstetric Surgeon to the Queen's Hospital, Birmingham. London, Hardwicke. 1865. pp. 244.

On Cancer: its Allies and Counterfeits. By T. Weeden Cooke, Surgeon to the Cancer Hospital, &c. London, Longmans and Co. 1865. pp. 226.

A Treatise on the Principles and Practice of Ophthalmic Medicine and Surgery. By T. Wharton Jones, F.R.S., Professor of Ophthalmic Medicine, &c., University College. Third Edition. London, Churchill and Sons. pp. 806.

A Theoretical Inquiry into the Physical Cause of Epidemic Diseases, accompanied with Tables. By A. H. Howe, M.D. London, Churchill and Sons. 1865. pp. 171.

The Practice of Medicine and Surgery applied to the Diseases and Accidents incident to Women. By W. H. Byford, M.D., &c. Philadelphia, Lindsay and Blakiston. 1865. pp. 566.

Manual of Materia Medica and Therapeutics; being an abridgment of the late Dr. Pereira's Elements of the Materia Medica, arranged in conformity with the British Pharmacopœia, &c. By F. J. Farre, M.D., R. Bentley, M.R.C.S., and R. Warrington, F.R.S., &c. &c. London, Longmans and Co. 1865. pp. 614.

A Practical Treatise on Urinary and Renal Diseases, including Urinary Deposits. Illustrated by numerous Cases and Engravings. By W. Roberts, M.D., Physician to the Manchester Royal-Infirmary, &c. &c. London, Walton and Maberly. 1865. pp. 523.

Introduction to Modern Chemistry, Experimental and Theoretic. Embodying 12 Lectures delivered in the Royal College of Chemistry, London. By A. W. Hoffmann, F.R.S., &c. London, Walton and Maberly. 1865. pp. 233.

Nice, and its Climate, &c. Second Edition. By E. Lee, M.D. London, Adams. 1865. pp. 179.

Health Resorts in the South of France, &c. &c. By the same. Second Edition. pp. 213.

The Elements of Prognosis in Consumption. With Indications for the Prevention and Treatment. By J. Pollock, M.D., Physician to the Hospital for Consumption and Diseases of the Chest. London, Longmans and Co. 1865. pp. 432.

Descriptive Catalogue of the Pathological Preparations in the Museum of the Medical College, Calcutta. By J. Ewart, M.D., Bengal Medical Service. London, Smith and Elder. 1865. pp. 323.

Lectures on Inflammation: delivered before the College of Physicians of Philadelphia. By J. H. Packard, M.D. Philadelphia, Lippincott and Co. 1865. pp. 276.

The Malformations, Diseases, and Injuries of the Fingers and Toes, and their Surgical Treatment. By T. Annandale, F.R.C.S., Lecturer on Surgery and Assist.-Surgeon to the Edinburgh Royal Infirmary. (The Jacksonian Prize Essay.) Edinburgh, Edmonston and Douglas. 1865. pp. 292.

Observation in Medicine, or the Art of Case-taking: including a Special Description of the most common Thoracic Diseases and abnormal states of the Blood and Urine. By J. S. Warter, M.D. Illustrated by wood engravings. London, Longmans and Co. 1865. pp. 91.

The Principles and Practice of Medical Jurisprudence. By A. S. Taylor, M.D., F.R.S., Professor of Medical Jurisprudence and Chemistry in Guy's Hospital. London, Churchill and Sons. 1865. pp. 1186.

Clinical Memoranda. By W. Hinds, M.D., Professor of Botany, Queen's College, Birmingham. No. 1. On Pericarditis and Pericardial Murmurs. Birmingham, Mathews. 1865. pp. 22.

On some of the Causes and Effects of Valvular Diseases of the Heart; being the Croonian Lectures of the Royal College of Physicians for 1865. By T. B. Peacock, M.D., F.R.C.P., Physician to St. Thomas's Hospital, &c. London, Churchill and Sons. 1865. pp. 114.

Military Medical and Surgical Essays. Prepared for the United States Sanitary Commission. Edited by W. A. Hammond, M.D., Surgeon-general U.S. Army, &c. Philadelphia, Lippincott and Co. 1864. pp. 552.

Address delivered at the London Hospital Medical College, at the commencement of the Winter Session, Oct. 1865. By W. Rivington, M.S., Assistant-surgeon to the Hospital. Churchill and Rivington.

Handbook for Yellow Fever: describing its Pathology and Treatment, &c. With a Brief History of Pestilential Cholera, and Method of Cure. By T. Anderson, M.D. London, Churchill and Sons. 1866. pp. 80.

The United States Sanitary Commission. A Sketch of its Purposes and its Work. Compiled from Documents and Private Papers. Published by permission. Boston, Little, Brown, and Co. 1865. pp. 299.

On Intra-thoracic Cancer. Part II. Contributions to the Pathology of the Disease. By J. Cockle, M.D., Physician to the Royal Free Hospital. London, Churchill and Sons. 1865. pp. 159.

Australia for the Consumptive Invalid: the Voyage, Climate, and Prospects for Residence. By I. B. Brown, jun., Assist.-surgeon to the London Surgical Home, &c. London, Hardwicke. 1865. pp. 137.

The Harveian Oration, 1865. By H. W. Acland, M.D. Oxon., F.R.S., &c. London, Macmillan and Co. pp. 85.

An Introduction to the Study of Medicine, to which is appended a Report on the Homœopathic Treatment of Acute Diseases in Dr. Fleischmann's Hospital, Vienna. By G. W. Balfour, M.D., Lecturer on Practice of Physic, &c. Edinburgh, Black. 1865. pp. 307.

The Pocket Formulary and Synopsis of British and Foreign Pharmacopœias, &c. By H. Beasley. Eighth Edition. London, Churchill and Sons. 1866. pp. 583.

The Anatomical Remembrancer, or Complete Pocket Anatomist, &c. Sixth Edition. Churchill and Sons. 1866.

Lectures, chiefly Clinical. By T. K. Chambers, M.D., Physician and Lecturer on Practice of Medicine, St. Thomas's Hospital, &c. Fourth Edition. London, Churchill and Sons, 1865. pp. 612.

Some Effects of the Climate of Italy. By the same. Churchill and Sons. 1865.

Resection of the Shoulder-joint. Reprint from 'Army Medical Reports,' vol. v., 1865. By Deputy Inspector-General T. Longmore, (Pamphlet.)

On the Probable Surgical Effects in Battle in case of the Employment of Projectiles of a more Elongated Form, such as the Whitworth Projectiles. By the same. (Pamphlet.) Reprint.

Cullen and Gregory upon Change of Type in Inflammation. By G. W. Balfour, M.D. Read before the Edinburgh Med.-Chir. Soc., June, 1865. (Pamphlet.)

The Antecedents of Cancer. By C. H. Moore, F.R.C.S., Surgeon to the Middlesex Hospital, &c. London, Richards. 1865. (Pamphlet.)

The Introductory Address delivered at Opening of the Medical Department of King's College, London, Session 1865-6. (Pamphlet.)

On Wakefulness and the Physiology of Sleep. By W. A. Hammond, M.D. Philadelphia, 1866. pp. 93.

Diarrhœa and Cholera: their Origin, Proximate Cause and Cure, through the Agency of the Nervous System, by means of Ice. By J. Chapman, M.D. Reprint from 'Medical Times and Gazette.' (Pamphlet.)

On the Speedy Relief of Pain and other Nervous Affections by Means of the Hypodermic Method. By C. Hunter, Surgeon to the Royal Pimlico Dispensary. London, Churchill and Sons. 1865. (Pamphlet.)

On the Congenital Deformities of the Human Cranium. By W. Turner, M.B., F.R.S.E. Reprint from the 'Edinburgh Med. Journal,' 1865. (Pamphlet.)

U.S. Sanitary Commission. Preliminary Report of its Operations in North Carolina, March, 1865, and upon the Physical Condition of Exchanged Prisoners lately received at Wilmington, N.C. New York. 1865. (Pamphlet.)

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The Climate of Malaga, in the Treatment of Chronic Pulmonary Disease, illustrated by recent Meteorological Observations. By T. M. Madden, M.D., &c. Dublin, Falconer. 1865. (Pamphlet.)

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Die Lehre von der Mania transitoria für Aerzte u. Juristen dargestellt. Von Dr. R. V. Krafft-Ebing. Erlangen, 1865.

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Sulle Malattie da Fermento Morbifico e sul loro Trattamento, Memoria Seconda, (Parte Clinica.) Del Dottor G. Polli. Milano, 1865. pp. 97.

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Azione Nervea vaso motrice studiata in sé e nelle sue applicazione alla Patologia. Per G. Pucciante di Siena. 1865. (Pamphlet.)

Meran als Klimatischer Kurort. Rückblicke auf. d. Saison 1864-5. Von Dr. J. Pircher, Wien. 1865. (Pamphlet.)

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Guy's Hospital Reports. Edited by S. Wilks, M.D. London, Churchill and Sons. 1865. pp. 465.

Saint Bartholomew's Hospital Reports. Edited by Dr. Edwards and Mr. Callender. Vol. I. London, Longmans and Co. 1865.

Statistical Report on the Health of the Navy for the year 1862. pp. 316.

Statistical Sanitary and Medical Reports. Army Medical Department, Vol. V. For year 1863. pp. 648.

Clinical Lectures and Reports by the Medical and Surgical Staff of the London Hospital. Vol. II. 1865. London, Churchill and Sons. 1865. pp. 415.

Report of the Sanitary Condition of the Parish of St. Mary's, Islington, during the year 1864. By E. Ballard, M.D.

Report on the Sanitary Condition of the City of Edinburgh, with relative Appendices. By H. D. Littlejohn, M.D., Edinburgh, 1865. pp. 120 and 72.

On the Cleansing Operations of Edinburgh, as compared with other Towns. By the same. Read before the Social Science Association, 1863.

The Morningside Mirror. Oct. 1864—September, 1865. Vol. XX.

The Ophthalmic Review. Edited by J. Z. Laurence and T. Windsor. Oct. 1865. No. 7. London.

The Quarterly Journal of Science. No. 8. Oct. 1865. London.

The Dublin Quarterly Journal of Medical Science. Nov. 1865.

Edinburgh Medical Journal. Oct., Nov., Dec., 1865.

The Journal of Mental Science. Oct., 1865.

The Australian Medical Journal. July, Aug., 1865.

Medical and Surgical Review (Australasian). Vol. III., No. 13.

Journal of the Scottish Meteorological Society. Oct. 1865.

The New York Medical Journal, Nos. 6, 7, 8.

The American Journal of the Medical Sciences. Oct. 1865.

Journal of Social Science, including the Sessional Papers of the National Association for the Promotion of Social Science. Edited by E. Lankester, M.D. London, Chapman and Hall.

The Indian Annals of Medical Science. No. 19. Oct. 1865. Calcutta and London.

THE

BRITISH AND FOREIGN

MEDICO-CHIRURGICAL REVIEW.

APRIL, 1866.

PART FIRST.

Analytical and Critical Reviews.

Review I.

1. *Hautkrankheiten.* Von Prof. Hebra (Virchow's 'Handbuch der speciellen Pathologie und Therapie.' Dritter band. Erste, zweite und dritte Lieferung.) —*Erlangen,* 1860—1864.

   Diseases of the Skin. By Prof. Hebra (from Virchow's' Handbook of special Pathology and Therapeutics.' Vol. III. Parts 1, 2, and 3.
2. *The Student's Book of Cutaneous Medicine and Diseases of the Skin.* By Erasmus Wilson.—*London,* 1864. pp. 517.
3. *Skin Diseases; their Description, Pathology, Diagnosis, and Treatment, with a copious Formulary.* By Tilbury Fox, M.D., Lond.—*London,* 1864. pp. 307.
4. *The Classification of Skin Diseases.* By the same Author.
5. *Handbook of Skin Diseases for Students and Practitioners.* By Thomas Hillier, M.D.—*London,* 1865. pp. 367.
6. *A Practical Treatise upon Eczema, including its Lichenous, Impetiginous, and Pruriginous varieties.* By T. M'Call Anderson, M.D.—*London,* 1863. pp. 129.
7. *On the Modern Treatment of some Diseases of the Skin.* By J. L. Milton.—*London,* 1865. pp. 116.
8. *A Practical Treatise on Diseases of the Skin in Children.* From the French of Caillault, with Notes, Appendix, and Formulæ by R. H. Blake, M.R.C.S. Second edition.—*London,* 1863. pp. 331.

9. *Atlas of Portraits of Diseases of the Skin, issued by the New Sydenham Society.* 1860—1864.

10. *Photographs, coloured from Life, of the Diseases of the Skin.* By A. B. SQUIRE, M.B.—*London.*

FOR many years after the commencement of this century, when Willan constructed his great system of skin diseases, a system which made his name known wherever pathology is studied, and shed an undying lustre on British medicine, dermatology made little or no advance in this country. It seemed as if the giant effort made by their countryman had rendered his followers so proud of his achievement, and so satisfied with his success, that they were content to accept without question what he had bequeathed them, adhering blindly to his plan of arrangement, and teaching what he had taught without taking therefrom or adding thereto. On the Continent, however, this was not so. In France, more especially, a series of independent workers followed, some adhering to Willan's classification, and enriching the subject by original clinical investigations; others, like Alibert, striking out for themselves an independent system, till, after a lapse of many years, dermatology, though it still bore the stamp of the great Englishman's hand, had undergone very material changes, and received very important additions. The credit which French physicians may justly take to themselves for the large share of work that they have thus contributed to the common good, is undeniably great; but at the same time it must be remembered that their opportunities have been much greater than those enjoyed by English observers. The advantages derivable from the comparison of several hundred cases collected from all parts of the metropolis in one special hospital, cannot be too highly rated. Compare the St. Louis Hospital at Paris, or the Department for Skin Diseases in the General Hospital at Vienna, with the little dispensaries dotted here and there about London, and confess that it is the field of observation, not the love of work or the capacity for original investigation which has been, and is still wanting in this country.

The statement just made would seem to be in direct contradiction with the list of English names that appear at the head of this chapter, but it is not so in reality. During the past few years the mania for book-making, which has raged with unprecedented fury in every department of English medical literature, has prevailed with special severity in that which is now under notice. The competition met with all around, and necessitated by England's colossal growth, shows itself to be quite as brisk in the medical as in any other market. Professional success is considered to be attainable only by early authorship, and each aspirant hurries to collect materials for his book, accumulating industriously the work of others, sketching out a new system of classification, and "la besogne est faite." It is thus that a series of works on skin diseases have lately appeared, which are useful in so far as they make known to English readers the principles and practice of continental professors, but tend to damage the reputation for independent thought and original investigation, which the work of men such as Willan has justly earned for this country. The great Morgagni's example might be well followed by many in the present age. He, it will be remembered, passed a busy life, and that a long one, observing and collecting facts, before he ventured to publish his immortal work, "De sedibus et causis morborum."

The name of Hebra, which stands first on this list, is now well known to all who profess any knowledge of skin diseases. He has had advantages such as few can hope to possess, and he has turned them to the best account. For twenty years past professor of skin diseases in the University of Vienna, he has reigned supreme over the large wards of the General Hospital set apart for those diseases. He has studied the Spedalskded in Norway, and the Pellagra in Venice and Lombardy. He is a man of singularly quick observation, very slow to accept a doctrine, and one who teaches nothing that he has not confirmed by his own experience. He has experimented largely in his wards with various remedies, sifting from a heap of rubbish such medicines as are of real use, and discovering new applications and methods of local treatment. He comes before us, therefore, with knowledge based on a careful observation of about eighty thousand cases, independently of his large private practice, and hence his teaching is entitled to much respect. Only three volumes of his book are as yet published, but we propose to give a short *résumé* of what they contain, and to complete the subject as soon as the remainder of the publication appears. Mr. Wilson is termed by Hebra "the first of living English dermatologists." His new book opens with an entirely new system of classification, and differs in many particulars from his last publication in 1863. The books of Dr. Fox and Dr. Hillier are careful compilations, each being prefaced by its own peculiar system of classification. The little work by the former is a well written treatise on classification, in which the systems of different writers are compared and criticised, and his own suggested as an improvement. Dr. Anderson dedicates his treatise on Eczema to Hebra, whose pupil he formerly was, and whose teaching he has clearly and well communicated to his countrymen. Mr. Milton's little book is devoted to the treatment alone of skin diseases, and is therefore rather adapted to supply the wants of practitioners than students. He has collected the various medicines and local applications employed by different dermatologists, more particularly Startin and Wilson; but, as the remedies are all given in detail with very few prescriptions, there is an *embarras de richesses* which is rather bewildering. A few well-selected prescriptions added to the appendix would make this a real aid to the practitioner. Full justice has already been done to Caillault's excellent work (see this 'Review,' vol. xxix, p. 383). The last edition contains, in addition to the translation by Mr. Blake, an appendix by the translator, in which are several prescriptions for children of from two to four years of age, together with some useful therapeutical notes and suggestions as to the best mode of using the various remedies, and the circumstances under which they are to be used. The portraits published by the Sydenham Society are chromolithographs, well executed by Mr. W. West, from original portraits by Elfinger, of Vienna, Burgess, and Hurst. To those even who have not seen Elfinger's original work in Vienna, these copies will appear the most beautiful and correct delineations of skin diseases that have ever been published. Great credit is due to Mr. Squire for his coloured photographs, some of which, especially the two which illustrate eczema and impetigo, are beautifully executed. The delineation of the real features of a skin disease must be allowed to depend, in a photograph as much as in an original painting, upon the colouring; and to the artist, therefore, a large share of the credit must be awarded.

How difficult, nay almost impossible, it is to frame a system of classification that has no weak points open to attack, may be judged of from the fact, that Wilson has already constructed and recommended no less than three entirely different systems. Passing by the plans of the older writers, such as that of Galen and Mercurialis, who divided skin diseases into diseases of the scalp and those of the general surface, or that of Lorry and Schönlein, for whom all eruptions were either local or constitutional, we come to those of more modern times, which may be resolved into three classes:—1. The system founded on the external form or aspect of the disease. 2. Which takes for its basis the anatomical arrangement of the skin. 3. The mixed system.

1. This, which was originated by Plenck, established by Willan, and adopted by Biett, Cazenave, Gibert, and others, looks only at the expression of the features, at the aspect of the eruption. It is compared, and justly so, with the Linnæan system in botany, for there is but little doubt that its founders must have had the latter great system before their eyes while

they were coustructing the former. The great claim to merit that this plan possesses rests on its simplicity; but its faults and deficiencies are so glaring that it can never hold a permanent place. It is faulty in that it places any given eruption in a particular class, simply by regarding that eruption in one phase of its existence, and in not studying the changes which the same eruption may pass through from its beginning to its end, from papular to vesicular, to pustular, to squamous. It therefore associates diseases on the ground of their presenting at any one period of their existence identical appearances, and so groups together cutaneous affections which are pathologically quite dissimilar (*e. g.*, scabies and variola, purpura and rubeola). On the other hand, it does violence to pathology by separating diseases which are most essentially and closely allied (*e. g.*, variola from rubeola and scarlatina.)

2. This was first developed in the last century, and has been, in more modern times, revived by Wilson. It separates one from another the diseases of the different constituents or parts, as epidermis, corium, papillæ, follicles, &c., which make up the skin as a whole. It is, therefore, a classification more in harmony with that employed in the arrangement of diseases in other parts of the body; but it has this great disadvantage, that, owing to the way in which one part of the skin is interwoven with the other, most diseases do not affect one part only, but many simultaneously. It has had but few supporters, and is now discarded by Wilson.

3. This is now in very general acceptation, in that its basis is broader, and consequently more "natural." It regards a disease from many points of view before giving that disease a place in any particular group. It looks at the cause, the anatomical and physiological characters, the course, in a word, the pathology of the disease. Many modifications of this system are in print; one man laying stress on the etiological, another on the physiological point of view, but all are essentially mixed. Wilson, in his last book, suggests a new system, which he calls "clinical," as "arising out of the analysis of a large number of cases." This is likewise a mixed system. Although not one of these is faultless; although, indeed, the probability is great, that a perfect system is a thing impossible, yet the mixed is that to which fewest objections can be raised, and some form of it must eventually become the standard system of classification. Of all that have been constructed, the two especially worthy of attention are that by Buchanan ('Edinb. Med. Journ.,' 1863), and that by Hebra. The latter divides all cutaneous affections into twelve classes. Like a true disciple of Rokitansky, he makes pathological anatomy the foundation stone on which his

fabric is reared; but his, like the others, is unmistakably a mixed system:

Class 1. Hyperæmiæ of the skin.
„ 2. Anæmiæ.
„ 3. Morbid secretion of the cutaneous glands.
„ 4. Exudations.
„ 5. Hæmorrhages.
„ 6. Hypertrophies.
„ 7. Atrophies.
„ 8. Neoplasms.
„ 9. Pseudoplasms.
„ 10. Ulcers.
„ 11. Nervous affections.
„ 12. Parasites.

Omitting all mention of classes 1 and 2, we come to class 3, which, though written by Zeissl, a pupil of Hebra's, is still a faithful record of the master's teaching. The disease called *seborrhea*, which attacks by preference the scalp and face, is characterised by the formation of soft crusts, which differ from those of eczema in being, as Hardy shows, readily detached with the nail, and kneaded or moulded between the fingers, and in leaving beneath the crust an oily glistening surface instead of a pustular or raw and discharging base. The best plan of treatment is to remove the crusts, by soaking them with oil, and then to apply some spirituous lotion, eau de Cologne in private practice, or Hebra's "spiritus saponatus alkalinus," a solution of two parts soft soap in one alcohol. When the disease affects the scalp in women, there is seldom any occasion to cut the hair; but the scalp is exposed by separating one portion of the hair from another, and painting in with a stiff brush a solution of pitch in alcohol.

The question as to the contagious nature of Bateman's "molluscum contagiosum" is not yet cleared up. Hebra, Zeissl, and many others in the clinique, have repeatedly rubbed their skins with the contents of these mollusca without producing any effect. Henderson and Paterson have likewise inoculated themselves without any result. On the other hand, Hardy, who has seen the disease communicated from the breast of a nurse to the face of a child, and from a patient to a nurse, states that he has discovered in the sebaceous substance the spores of a cryptogamous plant, and is fully convinced that molluscum is contagious. Caillault's evidence, who watched the disease spread from bed to bed till thirty children were attacked, is almost irrefutable. Virchow ('Die Krankh. Geschwülste,' 1863, p. 223) cannot deny that it is contagious. He suggests the curious, but rather improbable hypothesis, that it is only by the penetration of the molluscous secretion into the hair follicles of another person that the disease is communicated; that the same morbid process of irritation which gave rise to the formation in the original subject is thus set up in another. Were, however, this the case, one would have expected to find the experiments of Hebra and Zeissl successful. Class 4 is the most comprehensive of all, and includes several most important diseases. This exudative group is characterised either by the presence beneath the epidermis of a fluid product of exudation (*e. g.*, vesicles, pustules), or by swelling of the skin (*e. g.*, boils, tubercles, papules, wheals); or, where neither of these is discernible, by pigment staining or desquamation of epidermis, sequelæ which do not follow simple hyperæmia. It is thus subdivided:

- Exudations
  - Acute
    - Contagious (Exanthemata).
    - Non-contagious
      - Erythemata (including Pellagra).
      - Dermatitis.
      - Phlyctænoses.
  - Chronic
    - Squamous affections (Psoriasis Lichen, Pityriasis ruber).
    - Pruriginous affections (Eczema, Scabies, Prurigo).
    - Acneform affections (Acne, Sycoris).
    - Pustular affections (Impetigo, Ecthyma).
    - Bullous affections (Pemphigus).

While Willan's nomenclature is retained as far as possible, his numerous subdivisions are, in many instances, altogether omitted, one of the greatest services that Hebra has rendered to cutaneous medicine. Nothing more strikes the student who reads any previous treatise on such diseases as eczema, psoriasis, or herpes, than the innumerable adjectives joined to the substantive eruption to designate different varieties of that eruption. Hebra's aim has been to do away with this, to show that most of these long-named subdivisions are but different stages of one and the same eruption, to fuse and amalgamate, to substitute the true for the fanciful, and to render what was diffuse and difficult concise and easy. *Erythema multiforme* comprises all Willan's varieties except *erythema nodosum*, which is regarded as distinct from the others, its pathology being still uncertain, though there can be no doubt that many cases take origin in an inflammation of the lymphatics, and the hypothesis that the disease is in its essence a lymphangitis, carries with it some probability. The subject of *pellagra* has been fully treated of by Dr. Peacock in this Review (see the 'Review' for Jan. 7, 1863). Landouzy has since then confirmed the opinion, which he is there said to hold, that maize has no direct influence in causing the disease. He found, while journeying in Spain, no less than thirteen individuals affected with pellagra, in one small town of Aragon, where no maize is eaten. He further states, as the result of many post-mortem examinations, that softening of the periphery of the brain resembling that met with in the general paralysis of the insane is often found in those who have died of pellagra, and that softening of the cord in a greater or less degree is always met with. The mucous membrane of the small intestines is in some cases inflamed, or even ulcerated. The view supported by Hebra is the most reasonable yet taken, that the disease is caused by bad and insufficient food, especially dry farinaceous food without a due admixture of fat. Dr. Fox well remarks, that the influence of the sun in causing the eruption of pellagra affords a good instance of how a local irritant will act by preference on a skin that is badly nourished.

Whether *urticaria* should be ranked among the exudative diseases is a question. The belief seems to be gaining ground that the whole is, as Gull and Velten maintain, nothing but a spasmodic contraction of the muscular tissue of the cutis (see Gull on "Factitious Urticaria," 'Guy's Hosp. Rep.,' 3rd ser., vol. v, p. 316). *Dermatitis* is subdivided into idiopathic (from wounds, poisons, heat or cold) and symptomatic (erysipelas, boils and carbuncles, glanders, malignant pustule). In *burns* of the second degree, to prevent deformity from contraction of the cicatrix, there is no plan so efficacious as the application of caustic. The nitrate of silver, in stick, or a strong solution, is applied once at least every day to the burnt surface. As soon as the slough, which forms, loosens itself, it is to be removed and the caustic reapplied, till, at last, the slough adheres so firmly that it cannot be removed. The healing process is in this way much hastened, and the scar which forms is not so deep as it otherwise would be. Irrigation is always practised by Hebra in burns of the third degree, if they are limited in extent; if widespread, the "perpetual bath" is the only chance of saving life. This apparatus was placed, it will be remembered, in the Austrian department of the International Exhibition, and a good description of it is published in the 'Med. Times and Gaz. for 1861,' vol. ii, p..615. We had an opportunity of seeing this in use in the case of chronic pemphigus there described, and in another of severe burn, in both with the best effect. Hebra has also used it successfully in some cases of tetanus. It is so cheaply and easily made on Hebra's principle, and is so certainly efficacious, that every hospital in the kingdom would do well to keep one for serious cases of this kind. The subdivision *phlyctænoses* includes those diseases which are characterised by the formation of vesicles or bullæ, which run a short course and are not followed by a relapse. Such are *herpes*, in its different forms, and *miliaria*. Zoster has a wider signification than is generally supposed. There is scarcely any superficial set of nerves over whose course the eruption may not show itself. It follows the different branches of the fifth pair (see 'Syd. Soc. Portraits,' fascic. 3, portr. 8); the posterior cervical, dorsal and lumbar nerves; the brachial plexus; the nerves of the thigh. Hebra has seen it completely surrounding the trunk, a true double Zoster, so that Pliny's dictum, "Zoster appellatur, et enecat si cinxerit," is not infallible.

The *squamous* group is the first among the chronic exudative diseases, including *psoriasis*, *lichen*, and *pityriasis rubra*.

Although Willan described and figured under the name of psoriasis eruptions which are clearly eczematous, and although he erred in employing the name "lepra Græcorum," to designate an eruption which he regarded as distinct from psoriasis, yet he deserves every credit for having been the first to draw attention to, and to describe it as a disease *sui generis*. Hebra discards the word lepra from his nomenclature as one that has engendered the greatest confusion, and makes use of the one term psoriasis for all forms of this scaly eruption, lepra being, in truth, the same disease as, or one phase of psoriasis. This simple plan is by far the best. Wilson abandons entirely Willan's nomenclature in this particular instance, and describes the lepra and psoriasis of the latter under the name of "alphos." Further, which is still more perplexing, the word psoriasis, after being forced from its proper place, is applied to eczema in its scaly stage. The great Viennese dermatologist still more simplifies the matter by showing that the different subdivisions are but different phases of the same eruption. This, when it first appears, is "punctate:" thence spreading till it looks like a drop on the skin it is "guttate;" when still larger it has the size and shape of a coin, and is "nummular;" extending further, it meets with and joins other similar circles, till a large surface of the skin is covered, and the psoriasis is "diffuse." Each circular patch shows a tendency to desquamate and clear up at the centre more than at the periphery, and to assume in this way an "annular" form (Willan's lepra.) If two or more such rings meet together, the well-known bizarre shapes that the eruption sometimes assumes are explained, and the psoriasis "gyrata" is formed. Hebra is the first, we believe, to make mention of the fact that when two such circles meet, the disease is checked at the point of contact of the two circles, and the skin at that point becomes sound or pigment-stained, so that there remain only segments of the previous circles; and it can well be imagined that, the more numerous these are, the more fantastic are the forms developed. Of the real causes we are altogether ignorant, save one, that the disease is decidedly hereditary. The hundred and one causes generally enumerated in books are severely ridiculed, especially the interminable "catching cold" of English doctors. Psoriasis is pandemic. The difficulties of anatomical investigation are very great, inasmuch as the patches become pale after death, and the masses of epidermis are loosened. Careful and repeated examinations enable him to arrive at the same conclusion as Rokitansky, Wedl, and Simon, that the patches in their entire thickness consist of nothing but heaps of epidermis cells lying on a congested cutis. He has not been able to detect any positive change in the papillæ themselves. Of internal remedies, arsenic is the only one believed in. It is a trustworthy remedy, but, though it in most cases cures the eruption for the time being, it has no power to prevent a relapse. The cure is very much hastened by local applications; and, of these, the two in vogue at Vienna are soft soap and pitch. Good soft soap should be somewhat thicker and more consistent than syrup, should have a decidedly alkaline taste, an olive-green or brownish colour, should not contain any gritty particles when rubbed between the fingers, and should dissolve in alcohol without leaving any decided residue. It is to be rubbed firmly into the eruption with a rough piece of flannel, or brush, till the epidermis layers are removed and the base of the patch bleeds slightly; the soap is then left on to dry, and the patient remains without washing, for at least three days in blankets. It is well, if the eruption be general, to divide the body into six or eight parts, and treat each part as above, every day in succession; giving each, in this way, an interval of rest from six to eight days. But it is still better to use the soft soap for softening the skin before the application of the pitch. Of *pitch* there are four varieties, the pix liquida; the oleum fagi, made from the beech; the oleum cadinum, from the juniper; the oleum rusci, from the white birch, which gives the "parfum Russe" to Russian leather, and is, therefore, preferable on account of its pleasant smell, but is very difficult to obtain. The oil of cade is that most used in Paris and Vienna. It is well to be a little careful in first using pitch, and not to apply it to too large a surface, as it is in some cases absorbed, and appears in the excretions, blackening the stools and giving the urine an olive-green or inky colour, with the evolution of a strong pitchy smell on the addition of sulphuric acid. This absorption causes in some people vomiting and cephalalgia. Again, it sometimes sets up swelling and redness of the skin amounting to erysipelas; and it is always apt, when long employed, to cause inflammation and swelling of the hair-follicles, a kind of acne which Hebra names "pitch acne." Success depends altogether on the way in which the pitch is applied. It is not enough, as most suppose, to smear it on, but the following points must be scrupulously attended to. First, the scales must, as far as possible, be removed by bathing or soft soap. Secondly, the pitch must be rubbed long and forcibly into the part affected. Thirdly, the part must be afterwards covered, from two to six hours, with flannel or some woollen garments, which do not absorb the pitch, till all is dry. This must be repeated at least once a day, the preliminary softening of the epidermis being repeated on each occasion. The effect of this is, that the parts which were red began to grow pale; that the excessive formation of epidermis is arrested, and finally stopped; and that the red base of the patch becomes brown or reddish-yellow. Lastly, an energetic remedy well adapted for obstinate cases, where the disease is local and the thickening great, is the following. A bit of flannel is soaked in a solution of the pentasulphuret of calcium or modified Vlemingkx's solution (now in general use for the cure of scabies), is firmly rubbed over the patch till it slightly bleeds, and is then allowed to dry on. The patient next goes into a warm bath, where he stays an hour, and the patch is then smeared with oil. One such operation is generally enough; a black scab forming over the part, and after a short time falling off, when the skin beneath is healthy. This application should not be used over joints, as it stiffens them and prevents their motion.

Willan's *lichen,* which, together with strophulus and prurigo, makes up his group of "papules," is allowed by most writers to have too wide a signification. There is no doubt that many cases figured and described by him as lichen, such as, for instance, those of "lichen agrius," are nothing else than cases of eczema. Hebra, having observed that an admixture of papules forms a part of many eczematous eruptions, simplifies the matter most conveniently for students by amalgamating the disease lichen in its original signification, as heretofore understood, with eczema, under the title of *eczema papulatum;* but he still reserves the name lichen for two peculiar eruptions observed and described by himself alone, "lichen scrofulosum" and "lichen ruber." Anderson follows Hebra. Wilson includes lichen in his group of eczematous affections, thus admitting the close relationship which exists between the two diseases; but he only regards a part of Willan's lichen as true eczema, and retains the term lichen for certain papular diseases which must be considered apart from eczema; those, namely, in which the papules are dry throughout their whole existence, and are never associated with an exudation of any kind. Fox insists on the recognition of a disease lichen as distinct from eczema. The first of Hebra's two pathological curiosities, the *lichen scrofulosum*, is, as its name implies, an eruption which affects only those whom the co-existence of glandular swellings, of caries, periosteal inflammation and lupus, stamps as scrofulous. Papules of about the size of hemp-seeds, perfectly dry and scaly on their surface, are grouped, often in circles which occupy by preference the abdomen, about the trunk. They occur always in youths of from fifteen to twenty-five years of age, and are very obstinate. Cod-liver oil taken internally and used locally is the best remedy. The other, *lichen ruber*, is characterised by the appearance of papules, of a dull red colour, and covered with fine scales, which gradually so multiply that they run together to form a deep-red infiltrated surface covered with scales. The infiltration of the skin is a marked symptom, especially on the palms and soles, toes and fingers, in which parts the skin often cracks, and painful bleeding fissures result. The nails, likewise, become thickened and brittle. The hair is unaffected. When the eruption is very extensive, the itching is decided, but bears no proportion to the real severity of the disease, and is not to be compared with that in prurigo and general eczema. The patient gradually wastes, and dies in a state of marasmus. Examined microscopically, the hair follicles are found to have their mouths curiously widened and their terminations in the skin narrowed, so that they look like little funnels. The papillæ are found enlarged and their vessels dilated.

A still more uncommon disease than the preceding is *pityriasis rubra*, which has many points in common with it when both are seen in their advanced stage. The whole skin is intensely red, and covered with soft branny scales; but there is no infiltration or affection of the nails as in lichen ruber, and no discharge as in eczema. Hillier relates a case of this disease which recovered under mercurial treatment; but of Hebra's three cases all died. With the exception of this disease, pityriasis, like lichen, is amalgamated with eczema.

It had become, for years after Willan's death, so much a matter of custom to talk and write of *eczema* as a vesicular disease, that no one thought of questioning the supposed fact. It is only within the last few years that objection has been made to the description of eczema by Willan, viz., that "it is an eruption of small vesicles on various parts of the skin usually set close or crowded together." The idea of vesicles was so closely bound up with that of eczema that not a doubt was raised as to whether a true eczematous eruption might not run its course from beginning to end without a trace of a vesicle showing itself. French writers blindly followed Willan in their descriptions, and the English copied the French. Hebra has, for years past, taught in his lectures that the elementary lesion of eczema is not necessarily a vesicle, but may be an erythema, a papule, or a pustule. His views on the subject were published shortly in the 'Wiener Mediz. Wochenschrift,' 1862, No. 52; they have since been clearly made known to the English public by Anderson's monograph. Milton, however, had, previously to this date, read a paper before the Medico-Chirurgical Society, in which he endeavoured to show that eczema was not a vesicular disease at all. He still upholds the entire absence of vesicles in eczema, which is an error in the other direction. Wilson has changed his views about this matter. He no longer writes, as he wrote in the fifth edition of his work, defining eczema as "a non-contagious affection of the skin characterised by the eruption of minute vesicles in great number," but takes, as Hebra, a wider view of the subject.

Eczema is, therefore, according to the recent doctrines, a disease which, though occasionally acute, tends to run a chronic course; whose eruption is manifold in character, showing itself here in form of clustered papules, vesicles, or pustules, there as a red patch covered with fine scabs or secreting a sticky discharge; in another part appearing as a mass of green or gummy scabs on a red ground. It may, in short, present itself in five different forms:

1. Eczema papulosum (Syn. "lichen agrius:" "eczema lichenoides.")
2. " vesiculosum (Syn. "solare.")
3. " impetiginosum.
4. " squamosum (Syn. "pityriasis.")
5. " rubrum seu madidans.

Hence the elementary lesion, far from being only vesicular, may show itself under any one of the above varieties. Again, two or more of these varieties may exist simultaneously on the same person. Further, any one of them can be produced at will by an artificial irritation (friction with croton oil, etc.), the character of the eruption depending partly on the severity of the irritant, partly on the idiosyncrasy of the subject. Lastly, in a case of general eczema, each part of the body has a tendency to illustrate, as it were, a particular form of the disease, *e.g.*, the extremities the papular and vesicular form, the scalp and face the impetiginous, the trunk the squamous, the ears and flexures of joints the madidans. Anderson enumerates "infiltration of the skin, exudation on its surface, the formation of crusts and itching," as the four distinctive signs of every eczema, when the eruption is at its height. He goes a step further even than Hebra—the disciple outruns his master—in making prurigo, as well as lichen, pityriasis, and impetigo, a variety of eczema. Eczema may be either acute or chronic. The acute form, well spoken of by Gibert as "une poussée d'eczéma," attacks by preference the face, the penis and scrotum, the hands and feet, or involves the whole surface of the body. The chronic form is best subdivided, according to the localities which it attacks, into eczema of the scalp, hands, &c. One variety deserves special notice as being very peculiar in its appearance, that to which Hebra has given the name of *Eczema marginatum* ("Eczem der Schuster," shoemaker's eczema). The eruption always begins on the inner surface of the thigh, at the part where the scrotum lies in contact with it, as a circular patch, about as large as a fourpenny bit, the centre of which tends to heal, becoming pigment-stained, while the circumference keeps spreading and makes its way anteriorly over the front of the thigh, the pubes, and the abdomen, as far as the navel; posteriorly, over the nates and scrotum, to the same level as in front. The starting-point is generally the left thigh; but if, as often happens, both sides are affected, the two eruptions join one another in front and behind. The skin of the genital organs themselves remains always intact. The eruption is attended with violent itching. It is supposed by Hebra to be due to the continual moisture of the parts kept up in an unnatural degree by the sitting posture. This corresponds with Wilson's experience, who has generally met with the disease in officers returned from India, where it is supposed to be caused by the moist heat of the climate, combined with very frequent bathing.

In Hebra's inimitable 'Clinical Lectures,' where students learn and laugh more than with any other teacher in Europe, he was wont to treat two subjects with more than usual sarcasm: one was the custom of referring every disease to "cold" when you are ignorant of its real cause; the other, the oft-repeated statement, that an eczema too speedily cured or "driven in" sets up, by a sort of metastasis, diseases in other parts of the body. Such an opinion was stigmatised in the strongest language as "colossale Dummheit:" and here, in his book, we read "this is idle fancy, a result of false observation, and an incorrect notion of nature's working in the human body." Strong language will not, however, dispossess many physicians of the settled conviction they must have, that eczema is often vicarious with diseases of other parts, especially the mucous membranes; nor shake them in their belief that in certain individuals the healing of an eczematous eruption is certainly followed by diarrhœa, bronchitis, asthma, irritable bladder, or some other disease. He is honest enough to confess that, with the exception of a few well-known local causes, the origin of the disease is a mystery. He will have no "Deus ex machinâ" in the shape of cold, and disbelieves in teeth, temperament, and diet.

In the treatment of eczema the first point to bear in mind is that, do what you will, the disease is generally slow to get well: the second is to adopt a reasonable plan of treatment, and persist in it steadily. Acute eczema is to be treated, says Hebra, in as expectant a manner as possible. The surface should be dusted with Pulv. Zinci Oxyd. et Amyli: or, if the burning and itching be very intense, cold should be kept constantly applied by rags wetted in soft water. We saw the cold douche employed by him with very good effect in relieving distress when the eruption was acute and general. Anderson recommends the cold starch poultice so much in vogue in the St. Louis Hospital. In some chronic cases, especially those of the very wet eczema, as that of the flexures of joints or palms of the hands, ointments are very useful, but the crusts must be previously removed, and the ointment must be kept in contact with the part, spread on lint and covered with a bandage. The best are Hebra's healing ointment,[1] or the benzoated zinc ointment,[2] which is improved, says Wilson, by adding a drachm of spirit to every ounce of ointment, or one part red ointment of mercury with two or three parts cerate.

Pitch is especially indicated when the eruption is papular or squamous in character, where there is no serous infiltration, discharge, swelling, or heat. It must be applied more often than in psoriasis, as it adheres with little firmness and is easily washed away. Any one who is in the habit of using pitch much in this way must have noticed how much more readily it adheres to the sound than to the unsound skin. It is thus that when it begins to stick more readily to the diseased parts we have a sign that they are becoming more like the surrounding skin, or, in other words, that they are healing. An unpleasant, but often very successful plan, particularly where there is much infiltration of the skin, is the application of a slight caustic in the form of caustic potash in solution (ʒss vel ʒj, ad ℥j), varying in strength

[1] ℞ Olei Oliv opt., ℥xv;
Lithargyri, ℥iij ʒvj;
Coque in ung. molle; dein adde
Olei Lavand., ʒvj.

[2] ℞ Adipis præp., ℥vj;
Gummi Benzoini pulv., ʒj;
Liquefac. c. leni calore per horas 24, in vase clauso; dein cola per linteum, et adde
Oxydi Zinci puri, ℥j;
Misce bene et per linteum exprime.

according to the amount of infiltration present, which is to be painted over the part. In ordinary cases soft soap suffices, a piece of which about as big as a walnut is rubbed forcibly into the part with a piece of flannel, for about a minute, till numerous spots appear of a deep red colour and stripped of their epidermis. At each successive rubbing, which should be repeated twice a day, these spots are fewer in number and at length the skin is sound. Or, in place of this, which is rather brutal, a layer of soft soap spread on rag is to be kept in contact with the part for a few days, discontinued for a time if very painful, and then reapplied till the eruption is healed. As a last refuge and a never-failing one, a solution of one part caustic potash to two of water is dabbed on with a charpie brush, and the part is then rubbed with a piece of rag dipped in water till it becomes covered with a sort of sud as if it had been soaped, and the whole is washed with cold water. Cold wet rags frequently changed are afterwards applied, and at the end of a week the operation is to be repeated wherever there is any tendency in the skin to become red and vesicular, till the whole surface at length assumes a healthy appearance. Glacial acetum cantharidis is strongly recommended by Anderson as a blistering agent when the eruption is limited and chronic. It is to be painted on till the spot becomes quite white, after which a hot poultice is applied. One such painting is often enough to cure the complaint, the skin being found healthy beneath the crust which forms after vesication.

The second of the pruriginous affections is *scabies*. This and the other parasitical diseases have been so recently and fully noticed in this 'Review' (July, 1865, p. 23; April, 1864, p. 381), that it would be superfluous to repeat here what is there to be be found.

Prurigo, which was first well described and ranked among the papulæ by Willan, has been similarly treated by most writers since his time. It is a disease which seems to have been known to ancient writers as far back as the sixteenth century, but was never properly classified among skin diseases till Willan gave it a place amongst his papulæ. Since then, nearly all who have written upon the subject have followed his footsteps with but slight deviations. Wilson, in this his last book, regards prurigo as essentially a nervous affection. For a time the itching is the first symptom, the papules and other changes of the skin are secondary and depend in great measure on the scratching. Fox likewise considers the formation of the papules to be determined by the local irritation. Anderson, as has been already shown, looks on prurigo as a modification or form of eczema. Two only of Willan's pruriginous affections, viz., "prurigo mitis" and "prurigo formicans" are thought by Hebra to be worthy of the name prurigo; while Willan's "prurigo senilis," "localis," and "pedicularis," are taken away and make up Hebra's "pruritus cutaneus," which will be described later under the head of neuroses.

Prurigo simplex (mitis) is the milder and more common form which is characterised by the appearance of sub-epidermic papules of about the size of a hempseed. These are at first to be felt rather than seen, are isolated, itch violently, and are scratched till from the top of each oozes a drop of blood, which dries to form a black scab. After a time the cutaneous surface, wherever the papules are scattered, becomes pigment-stained, a result of scratching. In like manner, the ordinary lines or marks in the skin, especially on the fingers, backs of the hands and wrists, seem to be deeper or wider apart than natural—a consequence of the abnormal thickness and coarseness of the integuments, which are easily judged of by pinching up a fold between the thumb and fingers. The disease may thus remain stationary, or all the above characters may be intensified and the more terrible, though happily more uncommon, *prurigo ferox* (formicans) may follow. Here the papules are larger, the itching horrible, and, consequently, the crusts and excoriations extensive, while the intervening thickened skin is covered with a white mealy dust of desquamating epidermis. Lastly, in the very worst cases, the papules may turn to pustules, which blend together, and, bursting, discharge so as to form immense crusts.

The points to be dwelt upon in making the diagnosis are, first, the peculiar thickening and pigment-staining of the skin; secondly, the seat of the eruption. The hair loses its gloss, becoming rough and shaggy, but the scalp is free from eruption. The face is for the most part unaffected, as also the neck, while the trunk is covered before and behind with papules. But it is on the arms and legs that the stress of the disease always falls; and here it is most important to note, that *the skin over the flexors always remains intact,* while that over the extensors is most intensely changed. Of all parts the leg, along its anterior and outer aspect, is most affected, becoming so file-like that the finger, rubbed sharply over it, makes a noise as if a nailbrush were being scratched, and experiences at the same time a peculiar pricking sensation. Anatomically considered, the papillæ are enlarged; the hair, and probably also the sebaceous follicles, are diseased, while the papules are formed by the exudation of serum into the deep-seated layers of the epidermis and multiplication of cells there, differing from vesicles in the small quantity of serum they contain, and in the thickness of the investing epidermic capsule. Prurigo always begins in childhood, often showing itself as early as the first year, then disappearing, and finally coming out as a permanent prurigo simplex about the fifth to the seventh year. In this statement Hebra is altogether at issue with other writers, especially Wilson, who writes: "It is usually confined to the aged, but may exist at any period of life." The causes ordinarily enumerated are all fanciful. No one knows the cause of its appearance. According to Hebra's observation, it is not hereditary. "Prurigo ferox is incurable." This is a sad assertion to make, especially after the gloomy picture which Hebra paints of the life of a wretched being thus afflicted. Taunted and ill-treated at school, avoided by all in after-life, unable to get employment, debarred from marriage, he is tortured night and day by itching that is intolerable. No wonder that prurigo and insanity are said to be frequent companions, and that suicide has sometimes (once in Hebra's practice) proved the only effectual remedy. No medicine is of the slightest use, but life may be rendered bearable by appropriate local means. Prolonged warm baths, soft soap, and pitch, in some form, soften the thickened integument and relieve the itching better than all other applications. The pitch should be well rubbed in, and, directly afterwards, the patient is to get into a warm bath and remain in it from three to six hours. This procedure is repeated daily till considerable relief is experienced; but even then the disease is only kept at bay by the greatest care, and it is well to recommend the use of a similar bath during the person's lifetime. Sulphur is likewise efficacious, and is employed by Hebra in many cases under the form of Vlemingkx's solution.

It is seriously to be regretted that there has been so much delay in the publication of this work of Hebra's, certainly the most complete and masterly that has appeared since the days of Willan. Medical books should not be doled out in fragments, which break off in the middle of a description like the numbers in some popular work of fiction, and leave the reader waiting for more than a year to resume the thread that he has lost. We hope soon to be able to complete this notice of a book which should be read by all, and which ought to be translated into English as soon as it is completed.

Review II.

Nineteenth Report of the Commissioners in Lunacy to the Lord Chancellor.—London, June, 1865.

Seventh Annual Report of the General Board of Commissioners in Lunacy for Scotland.—Edinburgh, 1865.

The Fourteenth Report of the District, Criminal, and Private Lunatic Asylums in Ireland, with Appendices.—Dublin, 1865.

This triad of blue books is as varied in quality and interest as usual, and, as in former years, both the Scotch and Irish Reports are far better, more comprehensive, and more carefully worked up than is the English one. Perhaps the effect of nineteen years' official existence, or, possibly, the oppressive sense of being concerned with nearly 30,000 lunatics, has benumbed the energies of the London Commissioners, but their Report has of late shown signs of atrophy, and the present edition consists but of a string of curt sentences varied by stories, chiefly of neglected idiots or of accidents, and one suggestion. The Scotch and Irish Reports, on the other hand, present a complete view of the state of lunacy in those countries, and, besides giving the usual details of official visits, and of the condition of the various asylums, enter fully into questions relating to the increase of the insane, the best mode of accommodating them, and other points of moment. They read, indeed, like exhaustive essays on insanity in the districts they deal with, and show how fully the writers have entered into the spirit of their work, and with what energy and ability they have carried out their views. That these reports should differ in character is only what must be expected from the different conditions of the three countries; for though the disease of which they are the theme may be uniform, the circumstances under which it is to be dealt with vary considerably. The wealth and resources of England have provided for its insane on a liberal and even costly scale, but the same provision in districts scantily populated, not very productive and with inhabitants of moderate means, becomes a burden of no light weight. Indeed, so unwillingly is this borne by the thrifty Scots that the appointment of one of the most important elements of their lunacy board, the deputy commissioners, is only provided for reluctantly from year to year, instead of being placed on a permanent footing. In Ireland, the condition of the country throws the same difficulties in the way of a better provision for the insane, as it does in that of any other improvement in that ill-fated island.

There are several broad marks of contrast in the condition and management of the insane in the three countries, such as the number of lunatics, their distribution, the efforts made for their education, and so forth, but there are some points of interest common to all, which it is important to consider at the outset.

At a rough calculation it may be stated that in England and Wales there are about 29,000 lunatics officially known, while in Scotland there are about 6000, and in Ireland 8000, and the great problem is how to provide in the best way for the constantly increasing number of these cases. In England, the question is solved almost entirely in one way, viz., by congregating the insane in large public asylums, edifices costly to erect and equally so to maintain, but in Scotland it is the custom to restrict the use of the asylums to cases requiring special care and treatment, and to care for the chronic and harmless ones in workhouses or in private dwellings. We are speaking now, it will be understood, of only the pauper class, and the above is a fair statement of the great point at issue. In England, the constant cry is to enlarge every existing asylum, or build new, to meet the increasing number of applicants, and it is high time to stop and consider whether this is the best mode of meeting the difficulty. The crowded state of our asylums is really due to the preponderance of old cases, of the demented, imbecile, and paralytic, which do not benefit by nor require the expedients so useful to the more acute cases, and we agree with the Scotch Commissioners in thinking that such a use is not the proper one of an asylum, nor even justifiable, considering the expense it involves to the public. The remarks on this subject in the Scotch report are so forcible and pertinent, that we quote them at length. Speaking of the way in which patients accumulate in asylums, the Commissioners say:

"This is owing in great degree to the Lunacy Acts giving more consideration to the means of placing patients in asylums than to precautions for insuring their discharge in the event of detention ceasing to be necessary. Under the existing system, the duty of discharging patients rests, almost without control, with the superintendent of the asylum. It is he who determines whether the patient has recovered, or whether he continues to be of unsound mind. Great power is thus placed in his hands, affecting not only the welfare of the patients committed to his care, but the pecuniary interest of those responsible for their maintenance. It is very natural that superintendents of asylums should acquire the conviction that the insane can nowhere be under more favorable circumstances than in such establishments, and that they should ever doubt the propriety of discharging any one who has not recovered. But it must not be forgotten that superintendents have but limited means of becoming acquainted with the treatment of patients in private dwellings, and that the question of detention, where pauper lunatics are concerned, should be determined *in the interest of the public as well as in those of the insane*, who, it should be borne in mind, constitute but a section of those having claims on the charity of the nation. It might be proper and humane to provide hospitals for the treatment of all the poor suffering under mental or bodily ailments, in which they would receive the most judicious treatment, and enjoy far greater comforts than they could possibly command in their homes; but the State would shrink from any such general measure of relief, not only as uncalled for, but as detrimental to the independence and moral character of the people."

Referring afterwards to the Scotch Lunacy Act (sec. 71), they observe:

"This enactment satisfies us that it is not the intention of the Legislature that chronic patients should be detained in asylums for no other reason than that they continue of unsound mind; and we would therefore infer that their detention is justifiable only when their discharge would prove incompatible with the safety of the public, or with their own safety and welfare. Accordingly, we are of opinion that it is the duty of a superintendent, while authorising the continued detention of a patient, to satisfy himself not only that there is unsoundness of mind, but that it is of a character which, taken into consideration with the circumstances in which he would be placed on removal, would render it manifestly unsafe or improper to confide him to private care, and which, in the case of a pauper, would justify his maintenance at the public expense. Even supposing that a pauper would be better provided for in every respect in an asylum than in a private dwelling, it would still be open to doubt whether this fact would of itself afford adequate grounds for extending to him the benefits of gratuitous accommodation and treatment. The charity of the nation, like that of individuals, must have its limits; and a choice must therefore be made of the manner in which the national charitable fund shall be expended. Whether the extent in which it should be applied for the relief of the insane poor should be left, as much as is at present the case, to be practically determined by superintendents of asylums, becomes, therefore, a question of deep import."

Being of opinion that the law did not intend that "patients should be detained in asylums simply because they remain of unsound mind," the Commissioners propose as a remedy, placing a limit to the duration of the order on which patients are admitted. They say—

"It is obvious that the bodily and mental organisation of insane patients must be constantly undergoing changes which must of necessity alter the conditions under which the order of the sheriff was granted. To give this order unlimited duration, therefore, is to legislate for the future under circumstances which no man can foresee."

By thus limiting the time for which the order is valid, the proof of the necessity of further detention of unrecovered patients would be thrown upon the superintendents, and the final decision would rest with the Sheriff or with the General Board of Lunacy.

This question is one of the greatest importance to the country, and the opinions of the Scotch Board deserve attentive consideration. Another question of interest presents itself in the case of the so-called criminal lunatics. Their number varies much in the three reports. It was only last year that the English Government opened the State Asylum, the first separate building for this important and troublesome class of patients, and this now contains, we read, upwards of 300, but this is not quite half of the total coming under the category. Of the 323 now at Broadmoor, two thirds are men, the exact numbers being —men 222, and women 101. Of these, 55 men and 34 women had committed murder, 50 men and 11 women had attempted the same crime; 21 men and 2 women were guilty of arson, 32 men and 29 women of petty thefts, and 14 men and 2 women of burglary. Notwithstanding the very large sums spent on this establishment, it is very incomplete; and of some of the arrangements the Commissioners speak very unfavorably, especially of the wards devoted to the more troublesome male patients, and of the airing-courts attached to them.

In the Scotch report only 8 criminal lunatics are enumerated, 7 of them being men, and 3 of them charged with murder; but this can hardly include all that in England are so classed.

In Ireland the Dundrum asylum for criminals which has been devoted to this class for many years, and of which we last year gave an account, is spoken of in very high terms, and contained at the commencement of the year 120 patients, of whom 83 were men. Of these 120, 29 men and 7 women were homicides, and 32 men and 10 women were guilty of assaults.

The general character of the inmates of the criminal asylums differs from the average of those in public asylums, many of them being of a higher social grade; and one proof of this is found in the better education of the former, about 70 per cent.

being tolerably educated, a proportion double that met with in pauper lunatics. Another peculiarity is, that the unmarried at Dundrum, as in all Irish asylums, largely preponderate, fully in the proportion of 3 to 1, and the same also obtains at Broadmoor, whereas the opposite would seem to be the case in the public asylums of England, where the married are considerably in excess.

There is another class of the insane whose condition demands separate consideration, and whose claims are now attracting a good deal of public notice: we mean the imbecile and idiots. Helpless, degraded, and repulsive in form, it is hardly to be wondered at that the idiot has secured so little sympathy hitherto; but it seems as though this sort of deprivation of intellect abolished all ties of relationship and even humanity, to judge by the tales of cruelty and neglect that have been made public. The unfortunate position of the idiot class has, however, of late excited the attention of philanthropists, and the success which has attended their efforts seems likely to bring forth additional help in so good a cause. It is agreed on all hands that mixing idiot children with lunatics in large asylums is very objectionable, not only because so little can be done for their benefit, as they are lost in the crowd of greater interests, but because they acquire a good deal of harm from contact with older and more degraded people. We are glad, therefore, to see that two new special asylums for idiots have been founded—one at Lancaster, for the "northern counties," and another near Exeter, for the "western counties." The former of these two has a fund of £20,000 already, having had the generous gift of £2000 from one individual. The position of such asylums, as regards the lunacy laws, has engaged the attention of the Commissioners, who are inclined to waive any interference, hoping to see some simple legislation adapted for the case obtained before long from Parliament. The Commissioners say, in their 19th Report—

"The benefits to be derived, even in idiot cases apparently hopeless, from a distinctive system, and from persevering endeavours to develop the dormant powers, physical and intellectual, are now so fully established that any argument upon the subject would be superfluous. . . . It is our wish by every means in our power to encourage and promote the establishment of institutions for idiot children; and these, we think, will be most beneficial and successful if upon an adequate scale, and conducted upon the voluntary principle, so as to enlist the sympathies and elicit the liberal contributions of the wealthy and charitable."

In Ireland it appears that the idiots are much less in number relatively to the population than was the case sixteen or eighteen years ago, and one cause is said to be this:

"The famine of 1846–7, and the great destitution which pervaded the country at that period, was most fatally felt by those unhappy creatures, by their own utter helplessness, and the inability of the poor cottiers on whose charity their previous scanty sustenance depended, to afford them relief. As a further cause of their diminution, we would refer to the fact that *imbecile women, to whom they owe their parentage in no small degree*, from finding a permanent refuge in poorhouses, are *gradually ceasing to propagate the malady.* Certain it is that at the close of the year 1864 we learn, from the returns transmitted to us, made up under the authority of the medical officers attached to asylums, that the aggregate of congenital idiots in them *was limited to thirty-one.*"

Turning to the London Report, we find the number of insane of all classes reported on the 1st January, 1865, for England and Wales, was as follows, though there were upwards of 7000 "insane, idiotic, and imbecile" inmates of workhouses not included in this calculation:

In county and borough asylums . . .	22,284
,, hospitals	2177
,, licensed houses	4479
,, naval, military, and criminal asylums .	485
Total	29,425;

of whom, 13,988 were males, and 15,437 females.

Several of the present asylums are being enlarged, and a new one has been built at Stafford, and another in Surrey.

The case of Mary Ryan, the nun removed in a state of mania from this country to Belgium, and which excited so much public attention at the time, occupies a prominent place in the Commissioners' Report, which contains little else of any professional interest.

In Scotland the insane (exclusive of those maintained privately, and without official cognisance) were thus numbered on January 1st, 1865:

	M.		F.
In public and district asylums . .	1542	...	1579
,, private ,, . . .	363	...	451
,, parochial ,, . . .	217	...	273
,, poorhouses ,, . . .	179	...	253
,, private dwellings (about) . . .	720	...	938
Total	3021	...	3494

These figures are not *absolutely* correct, as the cases under the last category are not published later than January, 1864, but it is a close approximation.

The number of private patients reported is about one third of the paupers; but of the former many more are removed unrecovered than of the latter—a fact which tends to make the paupers accumulate more rapidly in asylums. The same preponderance of females is found here as elsewhere, and the Commissioners think that this is due rather to the larger source from which the supply of female pauper lunatics is derived than to a greater proclivity in the female sex to insanity. Thus, while the male and female population of Scotland were in 1861 as 100 to 111·2, the male and female paupers were in 1863 as 100 to 279·3, and the pauper *lunatics* as 100 to 117·7. There is, however, a great difference in the *pauper* lunacy of different counties. For instance, in Argyllshire, out of a male population of 38,928 there were 101 lunatics, and in Ayrshire only 100 out of 96,994 males.

In Dumbartonshire, for some reason or other, there are always more male than female lunatics. In Wigtown and Shetland the proportion of males to females among the pauper lunatics is very similar, yet the relative number of the sexes to the population is very different in the two counties, for in the former there are 107 females to 100 males, and in the latter 142 females to 100 males, the one being below and the other vastly above the average female excess. Linlithgow, also, is in the same position as the two last-named counties as regards its pauper lunatics, yet this is the only county in Scotland containing more males than females, having only 94 females to 100 males!

It appears from tables given (p. 12), that the admissions are most frequent in May, June, July, and August; that, as a rule, the male recoveries are rather above a third of the admissions; that the proportion of female recoveries is considerably higher, and that the number of recoveries for both sexes is lowest in the colder months.

Of those in public and private asylums, and in poorhouses, the deaths for the year were 426, the average number resident being, males 2357·8, and females 2594·4. The deaths are thus classified: from cerebral and spinal disease, 162 (42 from general paralysis); thoracic disease, 150; abdominal disease, 50; and from other causes, 64. Decidedly the most interesting feature of this Report is that portion contributed by Dr. Mitchell, one of the deputy commissioners (Appendix F), who discusses the condition of lunatics in private dwellings, and enters into several collateral points of importance. Among these one of the most curious is the "influence of emigration and immigration on the amount and character of insanity in the districts which they affect."

Dr. Mitchell says:

"When the Highlanders leave home for America, Australia, or New Zealand, *as a rule*, only the strong and sound go. Such of their relatives or dependents as are aged, infirm, imbecile, idiotic, deaf, blind, or crippled, are left at home. Many leave with the intention of sending for their unfortunate relatives as soon as they attain positions of comfort and stability; but good intentions are proverbially weak, and, besides, the emigrant's life is often for years a very unsettled one, and full of hardships and troubles, so that the affectionate leave-taking too frequently and too naturally ends in forgetfulness, indifference, and lasting desertion. Emigration, therefore, leads to an accumulation of the *defective* in the districts from which it is taking place, and the *sum of these cannot be said properly to belong to the population among whom they are found.* It appears clear that there *ought to be* an accumulation of defects of all kinds in any districts from which there is a constant exodus of the sound in mind and body. I have had opportunities of convincing myself that what should be true is true in fact. In the counties of Inverness, Argyll, Ross, and Sutherland, emigration does actually influence the amount and form of insanity which occurs in them. . . . The statistics of those countries to which our emigrants go, and whose population is *in this way* rapidly increasing, fully support these views. For instance, in California and Oregon, with a population of 184,370, there were, at last census, only six deaf-mutes and two blind,—a proportion vastly below that either for those States which have a stationary population, or for those from which emigration is taking place. I find, indeed, that it has been remarked by Peet, an American writer of note, that deaf-mutism is most numerous in those States from which emigration is greatest, and least so in those whose population is receiving accessions from without. I need scarcely point out that what is true of the influence of emigration on deaf-mutism will very probably be true also of its influence on the amount of idiocy, or any other similar defect."

If these views are sound, we ought to find, as nearly all the cases of congenital defect are left behind, the proportion of congenital to acquired insanity much greater in the country from which the emigration takes place, and so it is. In the Highlands there is, we read, "certainly a larger proportion of idiocy, and imbecility. than holds good for the rest of Scotland, larger than rural districts generally show, and much larger than is found in our cities." The converse proof is also at hand, for in the country receiving the emigrants, no such excess of congenital to acquired defect occurs. In New York State, we find only 1 deaf-mute to every 2088 of the native, and 1 in 5053 in the foreign population, and the same holds good in other states. Another fact is, and it is an additional proof, that "of 14,556 idiots (congenital defect) in the whole union, only 5513 were born in foreign countries, or 1 in 26; while of 15,139 insane (acquired defect) 2136, or 1 in 7, were foreign born."

In one county, however, an opposite state of things is found, viz., in Wigtownshire, for from this there is little or no emigration, while there is a continuous stream of immigration from Ireland. Comparing Wigtown with Rossshire, it appears that, while acquired insanity is about equal in both, the congenital forms preponderate in Ross. But in both these counties the amount of congenital insanity is above the average for Scotland, whereas, theoretically, in Wigtownshire, it should be below. This apparent anomaly, however, is explained as follows;

"The distance between Ireland and Wigtown is so trifling, and the passage so easily and cheaply made, that the emigrant leaves with his whole family or dependents, which he would not do if the distance and difficulties were greater. On reaching Scotland, he makes a halt in Wigtownshire, but, finding no work there, he leaves his wife and children, *especially if they are weakly*, to support themselves as they can by 'flowering,' or field-work, and starts himself on the tramp to the nearest railway in progress of construction, or to some of the large towns. Occasionally he fails to reappear, permanently deserting those he left behind him. The county of Wigtown thus acts as a filter to the stream of immigration, retaining many of the defective and useless, and thus raising the proportion which these bear to the general population."

In Ireland, the distribution of the insane known to the authorities on January 1st, 1865, was as follows:

In public asylums	4729
„ private ditto	552
„ workhouses	2563
„ gaols	452
„ maintained by Government	188
Total	8485

Inquiry made through the police showed 8200 more as being "deemed imbecile, epileptic, or in some way mentally affected," but not under official supervision. The Inspectors are, on the whole, well satisfied with the provision for Irish lunatics, and expect that within the lapse of two or three years "no district asylum in Ireland will be deficient in any of the substantial requisites that tend to the health and well-being of the insane."

They calculate that—

"By preparing accommodation in district asylums for 6800 inmates, every legitimate object will be attained, for, if to it be added the provision in the central asylum, now extended to 170 patients, and what is to be found in private establishments for the better classes, we arrive at a total accommodation, in round numbers, of 7600 beds, or, in proportion to the population of Ireland, one, too, not likely, under existing circumstances to increase, of a bed to every 760 of the inhabitants—a proportion fully equal to the most advanced statistics of insanity."

In speaking of the poorhouse, they suggest fitting up wards in a plain way for epileptics, idiots, and the demented, and providing a suitable staff of attendants and others, and thus avoiding the extension of the asylums, which, it is very justly observed, when overcrowded with patients utterly fatuous and possessing a mere animal existence, no longer sustain the main object of their erection. The mortality in the workhouses for the past year was only 6 per cent., and as it was some years ago much above the average, it is fair to suppose that a more generous and humane treatment has secured more favorable results.

The objectionable practice of sending to gaols lunatics considered "dangerous," is again commented on by the inspectors, who remark that "on the most frivolous pretext, persons reputed to "labour under a derangement of mind, with an intention to commit indictable offences," are too frequently immured in prison, through the intervention of relations or poorhouse officials, anxious only to get rid of their charges." To meet this, it has been proposed to empower the magistrates to commit such cases to the asylum instead of the gaol, but this remedy the writers of the report consider would be far worse than the evil itself, for they seem to have a dread of the magisterial view of "dangerous insanity," and intimate that—

"If magistrates, influenced no doubt by charitable motives, and an interest in the well-being of their own respective neighbourhoods, commit, one year with another, from 800 to 1000 exceptional cases to prisons as being dangerously violent, or for having attempted to perpetrate some criminal act, they would at once set about clearing the localities in which they reside *of any person who could be at all considered as affected with mental disease.* In view of this consideration, and when it is recollected that there are as many as 3600 gentlemen holding the commission of the peace in Ireland, it is no exaggeration to assume that in six weeks after the passing of a Bill for the proposed purpose, there would not be a spare bed in any hospital for the insane in this kingdom! !"

The well-known influence of hereditary predisposition to insanity receives a curious illustration in the tables of the Armagh

Asylum, where, it appears, that a female patient was admitted last year, whose three sisters and a first cousin died there; also a widow whose father, mother, and two sisters, had formerly been patients there; but a more extraordinary instance still occurred in the Cork Asylum, where two sisters, young and intelligent-looking girls, and a brother, were all attacked with acute mania on the same evening, on the occasion of the wedding of a brother, who has himself since then shown symptoms of insanity. This subject is still further illustrated by a table (No. 17, App. B), showing the relationship to each other of patients treated in the district asylums in 1864, so far as it could be ascertained. The total number *treated* in the seventeen district asylums, was 5914 in the year, and the following degrees of relationship were traced in 220 instances: parents and their children, 22; brothers and sisters, 71; uncles, and aunts, nephews, and nieces, 26; cousins (first and second), 75; and other relatives, 26.

During 1864, 1242 cases were admitted in the district asylums, 614 being males, and 628 females, and these were classified as follows:

Mania	844
Melancholia	195
Dementia	60
Monomania	65
Imbecility	15
Idiocy	11
Mental affections complicated with epilepsy	52
Total	1242

The deaths for the same were 410: males, 218; females, 192; and of these, 102 were assigned to cerebral affections, 67 to abdominal, and 132 to pulmonary diseases.

We observe that a very proper attention is paid to education, and that a code of rules has been drawn up for the better management of asylum schools, which are now affiliated to the Board of National Education, the salaries of the teachers being paid out of the usual Treasury advances.

Such are the chief features of these Reports, which afford much useful information on the subject of insanity. Considering, however, the power enjoyed by these Lunacy Boards, and the machinery they possess, the results of their labours might be far more profitable in many ways—for instance, in statistics. Instead of the desultory observations pursued by various asylum officers, each of whom frames his figures in what shape he pleases, and publishes them or not as fancy leads him, why should not the English Commission publish the main statistical results of the year in their annual report? They have all the materials in their office, and the statistics of every asylum in the country might thus be presented to the public in a convenient form. Such information could not fail to be valuable, and it is only fair that, when these statistics are furnished compulsorily to the Government, they should be made available for the public benefit, instead of being simply thrown aside as they at present are. The Irish Board does this to a certain extent, and it is very creditable that it undertakes voluntarily a task of no little trouble. The power wielded by these authorities is more considerable than is generally known, and is often asserted in a way but little consonant with the feelings of the public at large. The English Lunacy Commission sprang out of great abuses; it arose when men's feelings were strongly roused by the wicked and cruel treatment the insane suffered, and the public was glad to see such crying evils swept away, even though it were by a somewhat violent stretch of authority; but with these evils has passed the necessity for continuing a central despotism. Instead of resting satisfied with what power they had, the constant aim of the Commissioners has been to add to it, and all the recent Acts passed have been devised to enable them to crush some opposition, or to extend their power, to give them the right to inquire into somebody's business, or to legalise some vexatious interference. There is nothing broad, comprehensive, or statesmanlike, in the Acts of late years—nothing to convey the idea of a wise measure originated to carry out a principle or to secure endangered rights or liberties; on the contrary, the provisions are ludicrously frivolous and trifling, and are a mere collection of devices to overcome some obstacles met with in the would-be triumphant career of these enterprising officials. To be mad now is to become a patient of the State, and the prescriptions should properly be dated from No. 19, Whitehall Place. The tyranny of such a power as the Lunacy Board is so oppressive, as affecting the management of private asylums, that it has effectually extinguished all opposition, and proprietors, however conscientious and upright, are compelled in self-defence to defer to the powers that be. As regards the rulers of public asylums, the case is not very different, for, though country gentlemen do not always find the yoke easy, they too often, conscious of their ignorance, accept the dictum of a body who are always ready to "suggest," and, if their advice is not taken, can embarrass a Committee by persevering opposition, and threaten them with the terrors of the Secretary of State. There must be, however, a reaction at some time, and there are signs that it is already approaching.

Lunatics are apt to be unpleasantly expensive at the best, but their cost has been maintained at the highest by the extravagance of the provision made for them, and for this the Commissioners are in great measure responsible, for they have been chronic stimulants to expenditure. Asylums are in England the Commissioners' panacea, and the expensive follies in which many of them abound, are utterly wasted on the herds of fatuous paupers so accommodated in every county. This system is now openly challenged, nay, even deprecated by the Scotch Commissioners, who provide far more suitably for the chronic insane of all kinds, and this division in the official camp is likely to bring *public* opinion to bear on the general question, for pecuniary interests are proverbially powerful excitants. We readily admit the good of official inspection, and a controlling power, but a continual meddlesomeness is almost worse than the evils it is meant to counteract.

Review III.

The Gray Substance of the Medulla Oblongata and Trapezium. By John Dean, M.D., folio, pp. 75, with portfolio of photographs.—*Washington*, 1864.

When we reflect that the roots of all the cranial nerves, except the olfactory and the optic, may be traced more or less completely to the medulla oblongata, and that it is continuous with the spinal ganglia below, the importance of a correct knowledge of its structure cannot possibly be over estimated. The peculiar features of the medulla oblongata are well expressed in the following passage:

"Being situate between the systems of encephalic and spinal ganglia, it is a common centre of the substrata of both. It contains three fundamental kinds of organs, namely—*first*, those which are proper to it as a series of integrated segments of the spinal cord; *secondly*, those which belong to it as the centre of the corporeal ganglia proper, and therefore of all the vital functions; and, *thirdly*, those which are commissural between it and the encephalon on the one hand and the spinal cord on the other. In the first class are those which co-ordinate the structures of the head and face, including the mouth and throat; in the second, those which co-ordinate the movements of the body, or of its various systems of respiration, circulation, digestion, elimination, &c.; and in the third, the strands, anterior and posterior, which pass from the spinal cord and medulla itself onwards to the encephalon."[1]

[1] Laycock on 'Mind and Brain,' vol. ii, p. 423.

The intricate interlacement of nerve-fibres and cell-processes, and the existence of the various nuclei and the olivary bodies, centres of nervous influence, mark this portion of the nervous system as one especially to be studied with a view to the elucidation of the puzzling pathological phenomena attending paralysis and epilepsy. It is with the view to this practical end that we desire to draw the attention of our readers to the late anatomical researches into the medulla oblongata. It would carry us beyond the scope and the space at command of the present notice, were we to attempt to follow Dr. Dean in his description of the morphological changes that take place in the medulla in its upward course, from the cervical region of the spinal cord, to its termination in the trapezium at the border of the pons varolii. The review of these changes of structure is, indeed, the less required, inasmuch as Dr. Dean, while he adds but little to interest, entirely confirms and enlarges upon the previously published researches of Mr. Lockhart Clarke. We, therefore, restrict ourselves, deriving therefrom the following outline of the origin and connections of the various nerve-roots seen to enter or emerge from the medulla oblongata. The interest with which the obscurity of disease invests the structure of the medulla will directly induce many to go beyond the limitation of our observations to anatomical details of the roots, and nuclei of the nerves.

We commence our extracts with the ninth or hypoglossal nerve, which is distributed to the muscles of the tongue, and some of those attached to the hyoid bone. It is, therefore, designated a *motor* nerve. It appears on the side of the medulla between the anterior pyramid and olivary body. Lockhart Clarke thus briefly describes the attachment of the roots of this nerve to the medulla oblongata:

"In man this nerve is attached to the surface of the medulla, along the *inner side* of the olivary body, between it and the pyramid; but in mammalia generally it lies on the outer side. . . . . . The nerve in man proceeds inwards in several bundles, which wind at first in a sigmoid or serpentine manner through different parts of the olive, and then pass backwards, in nearly straight lines, to the nucleus in front of the central canal."

This *nucleus* consists of

"Two groups of nerve-cells, which make their appearance just above the cervical nerves, in front of the central canal, extending laterally to a considerable distance on each side. These groups seem to be a continuation of the cell-columns from which the anterior spinal roots arise, being situated within what is evidently the posterior portion of the anterior cornua the anterior portion of which, has already been broken up into an open network by the passage of numerous longitudinal fasciculi, to such an extent that the portion in the immediate vicinity of the central canal together with a branching wing on each side of the raphè alone remains distinct." (Dean, p. 13.)

As we ascend the medulla the hypoglossal nucleus enlarges and is close to the *calamus scriptorius*, gradually thence decreasing until it is merged in the auditory nucleus. The processes sent off from the multipolar cells of this nucleus go, like the cell-processes of the anterior cornua of the spinal cord, in various directions, some to the nuclei of the spinal accessory and vagus, and some to the roots of those nerves. Other processes decussate at the raphè, or cross over to the nucleus of the opposite side, while others are continuous with the hypoglossal roots. On the disputed point of this decussation at the raphè, Dr. Dean follows Mr. Clarke and Schroeder Van der Kolk.

The spinal accessory, so called from its association with the pneumogastric, is distributed in the sterno-cleido-mastoideus and trapezius muscles. The roots of this nerve, as pointed out by Mr. Clarke, emerge in front of the posterior lateral fissure of the cord, as low down as the sixth or seventh cervical vertebra, and may be traced to the lower part of the cervical enlargement of the spinal cord, where they come off from the posterior lateral fissure in company with the posterior roots of the spinal nerves; but its upper rootlets are attached in an irregular manner to nearly all the posterior half of the lateral column.

"The nucleus of the spinal accessory between the first cervical and lowest roots of the hypoglossal, is principally formed from the tractus intermedio-lateralis, it is constantly united to and reinforced by cells from the vesicular column." (Dean, p. 21.) Thus, the spinal accessory nucleus consists of two groups of cells, the anterior from the tractus intermedio-lateralis, and a posterior from the posterior vesicular columns. By the extension upwards of these columns, and their consequent connection with the accessory, vagus, glosso-pharyngeal, part of the auditory and trifacial, an explanation as suggested by Mr. Clarke, is afforded of the mechanism by which impressions made on the vagus, and on the incident fibres of the trifacial and spinal nerves, may call into action the whole class of respiratory muscles. Upon this, Dean observes:

"My own observations thoroughly confirm all the important facts pointed out by Clarke and Schroeder Van der Kolk, only differing from the latter in some of the minor details. The main point, however, I consider completely established, that the *respiratory centres* are brought into connection with *descending fibres from the trifacial forming together a system of descending longitudinal fasciculi connected with columns of cells, continuous with those in the cervical and dorsal regions of the spinal cord*, and thus connected with both *anterior* and *posterior cornua*, serving to bring into action a series of movements both direct and reflex."

The eighth pair, vagus, or pneumogastric nerve, is distributed to parts having various functions; it communicates also with cranial, spinal, and sympathetic nerves. It emerges from the medulla between the olivary and rectiform bodies. The roots are given off from the calamus scriptorius; they traverse, in their course, the gelatinous substance, and decussate at the raphe, like those of the spinal accessory. Its nucleus is essentially an upward extension of the posterior vesicular column; it is external to and in close proximity to the nucleus of the hypoglossal and glosso-pharyngeal nerves, and in connection with the post-pyramidal and rectiform nuclei; a possible relationship is also established, by cells in the caput cornu, with the trifacial nerve. The glosso-pharyngeal is the nerve of the sense of taste, and of ordinary sensation of the mucous membrane covering the pharynx, the arches of the palate, the tonsils, the posterior part of the upper surface of the tongue and the Eustachian tube. It passes from the medulla in company with the vagus.

"The course of the glosso-pharyngeal roots, and the distribution of their fibres within the nucleus resembles very strikingly the course and distribution of the vagus roots, and the connection between these two nuclei is very close, the nucleus of the vagus passing forwards as that of the auditory makes its appearance, until the three nuclei (hypoglossal, vagus, and auditory) are fused as it were into one mass, the remains of the vagus nucleus now constituting one of the sources from which the glosso-pharyngeal roots are derived. The transition, however, between the vagal and glosso-pharyngeal roots or nuclei is so gradual, that it is quite impossible to point out any exact line of demarcation." (Dean, p. 30.)

The auditory, or *portio mollis* of the seventh nerve, is distributed to parts in the inner ear, and is the nerve of the special sense of hearing. It appears at the posterior border of the pons varolii, emerging from the lateral tract of the medulla between the olivary and rectiform bodies. The nucleus of this nerve is to be found between the vagus nucleus and the post-pyramidal body, closely connected with the hypoglossal and glosso-pharyngeal nuclei. The auditory nucleus is lost in the cerebellum, merging in the common nucleus of the facial and abduceus. The roots of the nerve present, in the medulla, two divisions, an anterior and a posterior, the latter winding as a broad band around the rectiform body.

The facial nerve or *portio dura* of the seventh is distributed to the muscles of the face, some of the muscles of the soft palate, and the intrinsic muscular fibres of the tongue, communicating with sensory fibres of the fifth, before terminating in the muscles. Its position on the side of the medulla is close to and a little external to the last-named nerve. The nerve enters the medulla on the inner side of the caput cornu, passing to the *fasciculus teres*, which is the continuation of the hypoglossal nucleus and the common nucleus of the facial and abduceus nerves. The roots of the nerve in the medulla present an upper and a lower portion, the latter terminating in the nucleus, while the upper, without entering the nucleus, pass to the raphè, and decussate with their fellows of the opposite side. Some of these fibres, there is reason to believe, do not decussate, but join the longitudinal, to form communications with the trifacial.

The sixth nerve is lost upon the external rectus muscle of the eyeball. Its junction with the medulla is at a point between the pons Varolii and the anterior pyramid. The roots of this nerve, as pointed out by Stilling and Van der Kolk;

> "Form a remarkable exception in their inward course to all the other nerve-roots, bending *outwards* as they approach the floor of the fourth ventricle, while all the other nerves of the medulla bend *inwards* or towards the raphè, where their fibres decussate to a greater or less extent with those coming from the opposite side. As a natural consequence resulting from their peculiar course, the fibres do not decussate, at least directly, and if the opposite nuclei are brought into connection, it must be either through the intervention of fibres derived from cells, or by means of ascending or descending fibres, connecting the nuclei with distant parts, for I have not been able to trace any fibres turning inwards towards the raphè." (Dean, p. 61.)

The fibres of the abduceus may be traced to the nucleus of the facial, and to the floor of the fourth ventricle, with probable connection above, with roots of the motor oculi nerves.

Besides the preceding the fifth or trifacial nerve sends fibres of its lower roots to the medulla, to form connection with the auditory nucleus, the glosso-pharyngeal, vagus, accessory, and hypoglossal nerves.[1]

To bring these anatomical details to their practical elucidation of the phenomena of disease we may observe that one of

[1] 'Van der Kolk,' pp. 123—136, New Sydenham Society's translation.

our most active investigators of the pathology of the nervous system has put this question, with reference to a case of paralysis:—"Where is the disease which suddenly produces together paralysis of the tongue, palate, and vocal cord, all on one side, (and doubtfully of the orbicularis orbis)?"[1] The answer is derived from the recent researches of Mr. Lockhart Clarke, thus:—

"According to Bendz and Claude Bernard, the *lower* rootlets of the spinal accessory nerve are collected into the *external* branch which supplies the trapezius and sterno-mastoid muscles; while the *upper* rootlets go to form the *internal* branch which joins the pneumogastrics, and is through it distributed to the larynx, pharynx, and palate. Now, the *lower* roots, forming the external branch, have been shown by Lockhart Clarke to arise, in common with the anterior roots of the spinal nerves in the cervical and brachial region, from the anterior gray substance of the spinal cord; while the *upper* roots, forming the internal branch of the spinal accessory, having an entirely *different* and a *double* origin—one from its own special nucleus, continuous *behind* the central canal with that of the pneumogastrics; the other from the proper nucleus of the lingual or hypoglossal nerve, in front of the canal. On the other hand, some of the fibres of the lingual nerve *appear* to take their origin from the spinal accessory nucleus."

The demonstration of this close anatomical connection is of the highest interest, both in a physiological and in a pathological point of view, inasmuch as upon correct anatomy and physiology must be based sound views of pathology. It is, therefore, the more to be wondered at that even at this period there should be found pathologists who refuse to see in microscopical investigations a great help to the elucidation of the obscure phenomena of disease. The *cui bono* of these investigations is to some degree shown by the microscopic examinations of the morbid condition of the cord and medulla in paralysis, in a number of diseases, that are to be found in preceding volumes of this Journal by Mr. Clarke and Dr. John W. Ogle, and by other observers in other journals.

We must not close without noticing the proficiency and excellence of the illustrations that accompany Dr. Dean's work, comprising, besides forty engravings from drawings of transparent sections, thirty-six photographs of opaque sections of the medulla, as viewed by reflected light; although we are constrained to regard the latter as no very great advance, in point

[1] "Illustrations of Diseases of the Nervous System," by Dr. J. H. Jackson in 'London Hospital Reports,' p. 363.

of utility, upon the elaborate engravings published several years ago by Stilling, and which were so entirely thrown into the shade by Mr. Clarke's method of rendering his sections transparent, yet they are not devoid of interest as examples of one more of the applications of photography to science.

Review IV.

Metamorphoses of Man and the Lower Animals. By A. De Quatrefages, Membre de L'Institut, Professeur au Muséum D'Histoire Naturelle de Paris, &c. Translated by Henry Lawson, M.D., Professor of Physiology in Queen's College, Birmingham, &c. London, 1864, pp. 284.

The subject of this volume, assuredly one of the most interesting within the scope of zoological science, has been treated in a very able manner by its distinguished author. The work is not merely a digest of what has been done by others, it is much more and of a higher character; it contains the results of the professor's own widely extended researches. What very much adds to its value is, the simple, lucid and animated style in which it is written; and, fortunately, owing to the ability of its translator, it does not lose this charm in its English dress. In its commendation, too, we must not forget to mention another of its merits; this is, the historical notices which accompany the details, in which ample justice is done to the inquirers by whose patient labours one of the most obstruse and difficult portions of biology has been elaborated and brought to its present advanced stage.

The general conclusions which the author arrives at from a large induction of facts accompanied by much critical discussion, displaying great acumen and an admirable method, are the following: first, and chief, a confirmation of the great law, the dictum of Harvey, of *omne vivum ex ovo*; next, that the preservation and perpetuation of species are effected only by the congress of the male and female, or by the action of the male generative element, the spermatozoon, on the female element, the ovum, or in the instance of vegetables, on its anologue, the seed; and that offspring without such a congress, whether by buds, germination, or by slips, fissuration, have only a limited power of production, and no internal principle of perpetuity. The cycles of reproduction in animals and also in plants, to use his own words, "commence by the development of an ovum, or a fertile seed, embrace a certain number of generations, and terminate in the reappearance of truly seminal individuals. No matter how numerous the generations comprised by a cycle may be, all individuals, animal or vegetable, sexual or neutral, which comprise them, are still the direct or indirect product of the same ovum, or the same seed. All therefore are the mediate or immediate offspring of the male and female parents which produced and fecundated the primary germ." The term male and female parent, thus used, imply the male and female sexual apparatus, whether distinct or combined in the same individual.

Different terms have been employed by different authors to express the phenomena of the successive phases of development to which living beings, animal and vegetable, are subject. M. De Quatrefages uses chiefly two, Geneagenesis[2] and Metamorphosis; the latter applicable to the alterations which creatures undergo in attaining their mature growth, of which certain insects and batrachians are striking examples, not excepting man himself;—the former applicable to new individuals, the result of gemmation from or division of the parent-being, examples of which are afforded in the marvellous histories of other insects, such as the aphides, and of many of the marine mollusca and radiata. This term, geneagenesis, he prefers to that of parthenogenesis, invented by Mr. Owen, to express the same organic births; and he gives it the preference inasmuch as in the majority of instances, an ovum, a true ovum, is not concerned in the process; and, also in opposition to Mr. Owen, he considers organic reproduction in all its varities simply a phenomenon of growth—"the individualisation of a certain portion of the parent." In both these particulars, *i.e.*, in rejecting the term parthenogenesis and in the explanation of the non-sexual germ-production, he has adopted very closely the views of Dr. Carpenter, published in this our Review, the volume for 1848-49, to which we beg to refer those of our readers who are specially interested in the subject, and of which our author, as he states, had only a knowledge through the writings of Mr. Owen. To Dr. Carpenter, we think, it must be no small gratification to find that his early conclusions have the approval and the support of so weighty an authority.

Besides what relates to the main subject of this little volume, there is much in it of value incidentally introduced; observations here and there which arrest attention and give rise to thought. Of this kind are his distinctive marks of organic and inorganic nature; the one sexual, the other non-sexual; the one the seat of constant changes, the other in a state of absolute rest; these changes attended, according to him, invariably with

[1] From *γενεὰ*, and *γένεσις*, literally the reproduction of generations.

loss of weight, or, rather, we would remark, with such a loss in the majority of instances.[1] It is with this latter proposition that he begins his work, and he concludes it, maintaining the doctrine hardly now in favour with our most advanced physiologists, that organization, that vital functions and metamorphoses depend on a special vital force—"a vital vortex." We cannot, we think, do better than transcribe what he advances on this subject. The passage is a good example of his animated style, and should it not have approval, we feel confident that it will have the respectful consideration due to it both on account of the deservedly high reputation of the author, and the great obscurity of the problem, so well indicated by the words with which the quotation ends.

"It (the *vital vortex*) alone enabled us to comprehend metamorphosis; it alone explained the far more complex phenomena of geneagenesis. It is impossible to avoid seeing a fundamental law, and in some measure also an immediate cause of the development and completion of living beings in the exercise of this twofold movement of arrival and departure. Notwithstanding the assertions of some naturalists, who desire to arrest their inquiries at this stage, it is necessary to refer this fact to some higher cause; for matter, which is of itself inert, can only be set in operation by the impulse of some force or agent. Every material operation is at first an *effect* before in its form it becomes a *cause*. What, then, is the agent which is here at work in matter? Shall we, like some physiologists, invoke the six or eight forces admitted by chemists and natural philosophers, in explanation of the phenomenon occurring in inorganic matter? Long ago we gave the following reply to this question.[2] Yes; in organised beings we find the phenomenon of heat, light, and electricity; chemical affinity and capillary attraction are momentarily manifested, and possibly we may also find in them processes analogous to those of catalysis and epipolism. But these phenomena are accomplished, and these processes carried on, under the influence of a far higher power, whose existence it is in truth impossible to deny. Electricity, heat, and chemical affinity, operate in living beings, and are certainly engaged in the production of the *vital vortex*. Nevertheless they labour only under the control and regulation of a superior force—life, which modifies all brute forces, and causes them to produce muscle and blood instead of ammoniacal salts; bones instead of phosphate of lime; and plants and animals, instead of mere inorganic lifeless masses.

[1] An exceptional case appears to be that of an hybernating animal, the marmot, which, according to the experiments of Professor Sacc of Neufchatel, as reported by MM. Regnault and Reiset, gains weight during its period of winter-sleep, which they account for from the circumstance that whilst torpid more oxygen is consumed that there is carbonic acid formed.

[2] In my "Souvenirs d'un Naturaliste;" and in many articles in the "Revue des deux Mondes."

"All force is blind, and must necessarily be directed. In order to produce certain determinate species and not a kindred one, in order to avoid being lost amid the various paths of metamorphosis and geneagenesis, it is requisite that even life itself should be placed beneath the control of something superior. This something is the specific nature of each being—that which each plant and animal has received from its ancestors—through the intermediation of the seed or ovum from which it was produced, and which it will transmit to its descendents by the intermediation of the germs which it gives rise to in its turn. If we could go back for generations and ages we should still find the same questions presenting themselves, and invariably similar facts would give rise to like replies. In order to explain organic nature, it would be necessary to refer to the very origin of all things.

"But here, observation and experiment, those two guides which human science ought never to lose sight of, are absolutely unavailing. The true philosopher feels compelled to pause, lest he should set his foot in a land of hypotheses and conceptions, where it is so easy to wander from the proper path, and where truth itself—supposing it to be attainable—cannot be distinguished by any certain test." (p. 282—4.

Review V.

Statistical, Sanitary, and Medical Reports of the Army Medical Department. Vol. V. For the year 1863. 8vo, pp. 648.

Statistical Report on the Health of the Navy. For the year 1862. 8vo, pp. 316.

These reports, published last year, fully sustain the high character of their predecessors in point of interest and instructiveness. As we have more than once had occasion to remark, the large and varied amount of information which they contain on the topographical and geographical prevalence of many diseases, and on the influence of climate, locality, and so forth, as well as on the effects of the observance or neglect of hygienic and sanitary precautions in their development and severity, ought to secure for such volumes the attentive perusal not only of all military and naval medical men, but also of the enlightened physician in civil life. We begin with the army.

The results for 1863 are very satisfactory, as they show the admissions into hospital to have been 100, and the deaths 3·5, per 1000, under the average of the three preceding years, while there has been also a trifling decrease in the proportion discharged from the service as invalids. Compared with the results of 1862, there has been a decrease in all these particulars, showing that the improvement in the sanitary condition of the army is still progressive.

The following table will show at a glance the amount of sickness, mortality, and invaliding among the chief force of the army at home and abroad during the year:—

TROOPS IN	White Troops.						
					Annual Ratio per 1000 of Strength.		
	Mean Strength.	Admissions into Hospital.	Deaths.	Discharges by Invaliding.	Admitted.	Died.	Discharged as Invalids.
United Kingdom	75,945	72,908	673	2,474	960	8·86	32·58
Mediterranean	15,010	11,196	100	249	746	6·66	16·59
British America	15,457	9,987	142	193	646	9·19	12·49
West Indies	1,860	1,830	17	39	984	9·14	20·97
Western Africa.........	...	...	...	...	...	...	...
St. Helena..............	624	513	5	2	822	8·00	3·20
Cape of Good Hope ...	4,297	3,616	48	97	841	11·14	22·57
Mauritius	1,978	1,288	26	23	651	13·10	11·63
Ceylon	884	1,358	26	31	1,536	29·41	35·07
Australasia	6,567	3,774	146	122	575	22·23	18·58
China....................	2,464	4,970	148	45	2,017	60·06	18·26
India	67,527	110,357	1,596	1,366	1,634	23·64	20·23
On board Ship	5,787	4,503	35	...	778	6·05	...
Total	198,398	226,300	2,962	4,641	1,141	14·93	23·39

Let us now compare the hygienics of the French army with our own, as such comparisons are always suggestive and useful when all the facts are made known.

The death-rate, in 1863, among the French army stationed in France (294,149), was 9·22 per 1000 of effective strength; among the army in Algeria (53,772) it was 12·29 per 1000; and among the troops in Italy (13,776) it was 17·92 per 1000.

"The most striking difference in the mortality of the troops serving in France and in the United Kingdom is the high relative ratio of deaths by fevers, diseases of the organs of digestion, and by suicide in the French army; and by diseases of the organs of circulation and respiration, and of the nervous system in the United Kingdom.

"The ratio of deaths from typhoid fever among the troops serving in France amounted to 1·83 per 1,000. It is stated to have prevailed epidemically in the garrisons of Marseilles, Clermont-Ferrand, Angers, Paris, Dijon, Montpellier, Limoges, Tarbes, Toulon, Lyons, and Douai. The high ratio of deaths by fevers in Algiers and Italy was chiefly the result of pernicious intermittent fever, but the mortality by typhoid was also considerable, amounting in Italy to 3·55 per 1,000. Dysentery and diarrhœa were the principal diseases of the organs of digestion, and next to them peritonitis.

"The great mortality by diseases of the organs of respiration is the result of consumption. In the French report it is stated that the deaths returned under the head of chronic bronchitis, were in reality caused by phthisis. If the deaths by consumption, spitting of blood, and chronic bronchitis be classed together, the ratio of mortality in the French army serving in France was 2·24, and among troops serving in the United Kingdom, 3·17 per 1,000 of the strength. In diseases of the brain, spinal cord, and nervous system, the excess in the British army is most marked in the deaths by apoplexy and palsy, while in the French army there is a high ratio of mortality by insanity. Two deaths are also recorded in the latter service from nostalgia.

"Among the accidental deaths in the French army seven are stated to have been the result of duels; 72 men were drowned, being in the ratio of 0·24 per 1,000, while among the troops in the United Kingdom it amounted to 0·47 per 1,000; and one man was killed by lightning.

"Ninety-two deaths are returned under the head of 'Cause unknown,' among the troops serving in France, and only one among those in the United Kingdom."

The classification of diseases, &c., used in the French army, is somewhat similar to that formerly in use in our own army. The first three classes are fevers, eruptive fevers, and virulent or contagious diseases, which include syphilis, scurvy, and carbuncle. Then follow fourteen classes of diseases of the different organs or systems of the body, from the brain and spinal cord to the skin, &c.; to these succeed various diseases, from diverse causes, including all deaths from violence; and, lastly, "cause unknown." The French classification is, in many respects, like that still adopted in our navy. It is certainly much to be desired that our two services should be not so very dissimilar as they are at the present time.

Some painfully interesting particulars are given respecting the terrible waste of life in the Russian army, from 1841 down to 1861; the information is derived from a report in the Russian 'Military Journal,' for 1863. The years 1848 and 1849 are omitted, probably in consequence of these being cholera years; also, the five years from 1853 to 1856, as the results during that period would be exceptionably disastrous to life, on account of the Crimean war. During the first ten of the fifteen years included, the death-rate is stated to have averaged 37·0, and during the last five years 18·7, per 1000 of the strength. That war led, we are told, to various improvements in the military system of the empire.

"A table is given showing the mortality among the troops serving in the Caucasus, in Poland, and in Lithuania, of which the following are the general results:—

	Army of the Caucasus.		Troops in Kingdom of Poland.		Troops in Lithuania.	
	Ten Years, ending 1852.	Five Years, ending 1861.	Ten Years, ending 1852.	Five Years, ending 1861.	Ten Years, ending 1852.	Five Years, ending 1861.
Aggregate strength	1,406,933	237,092	684,304	162,060	556,739	115,525
Total Died	96,589	27,895	33,745	3,251	21,501	1,411
Total Killed in action	8,204	1,000	...	...	...	...
Ratio per 1,000 { Died	68·6	29·8	49·3	20·1	38·6	12·2
Ratio per 1,000 { Killed	5·8	1·1	...	...	...	...

"'Our losses in the Caucasus,' says the reporter, 'previous to the Crimean War, were, as we see, very considerable, and seriously affected the mortality throughout all the army. Taking the average numerical strength of the Caucasian force for that period at 150,000 men, and the mean loss by death at 10,000, it follows that in the course of fifteen years it perished entirely, and not a single man survived to retire on unlimited furlough. On making allowances for furloughs, discharges, and for men removed for disability, the result is that the entire composition of this army is renewed every eight, or at the utmost every ten years. . . . In the army of the Caucasus we discover a repetition of the well-known fact, that the deaths from natural causes bear a very large proportion to the numbers killed in action.'"

The deaths from the latter cause formed not a tenth part of the enormous loss of life caused by disease.

What a lesson to crowned heads and governments!—*quidquid delirant reges, plectuntur Achivi.* There appears to be an enormous amount of suicide in the Russian army, as might indeed have been predicted, considering the terrible system of conscription which prevails in many parts of the empire.

With respect to our Navy in 1862, we find that the total force afloat was 58,870. Of this number, 1944 men were invalided, and 902 died. From the subjoined table, the reader will at once see the annual rates of sickness, including injuries, invaliding, and death, on the different stations;—

STATIONS.	Ratio per 1000 of Men Sick Daily.	Ratio per 1000 of Invaliding.	Ratio per 1000 of Deaths.
Home	48·2	30·8	8·2
Mediterranean	55·6	35·1	9·7
North America and West Indies	56·1	32·3	20·2
Brazils	58·3	40·8	16·9
Pacific	49·6	34·	8·6
West Coast of Africa	54·4	23·2	21·9
Cape of Good Hope	97·3	32·2	23·7
East Indies and China	92·5	46·7	66·3
Australia	43·2	12·6	9·5
Irregular Force	70·1	34·5	9·5

The daily sick throughout the year averaged 57, in every 1000 of the total force. The ratio of the men invalided from the service was 33 per 1000, and that of the deaths was 15·3 per 1000. Dr. Mackay remarks that, in respect of the death-rate in the whole force on the year, if the ratio of deaths from what may be termed *casualties,* including under this head yellow fever, cholera, and wounds, and injuries, is deducted from the total, the result would be a death-rate in the naval service from all other affections of only 9·6 per 1000 of mean force.

But can we fairly, in estimating the amount of sickness and mortality in the service of the navy, exclude, as *casualties,* yellow fever or cholera, any more than various other maladies of the same class? For even if it be admitted as the ground for such exclusion that cholera aswell as yellow fever, "may certainly be altogether escaped by carefully avoiding the localities where it is prevailing," may not the same thing be said of the cases of smallpox, and of very much of the typhoid or gastric fever, which occur from year to year among the crews? These diseases are generally contracted on shore in the first instance. And so it is also with respect to the fevers, which prove so disastrous to the health of ships of war on certain tropical stations, as, for example, on the coast of China. On arrival there, they are often extremely healthy, with a low sick list; but within a week or ten days, particularly if the men have been ashore, it usually becomes doubled or trebled from bowel and febrile attacks. Whatever may be the alleged principal exciting cause or causes of the maladies, whether terrestrial, atmospheric, or personal, it matters but little; for still the fact would remain, that they might be altogether avoided, or nearly so, if the infested localities were unvisited. But as this cannot be, with the duties and exigencies of the public service to be fulfilled, the great point to be considered is, how most effectually to arrest, mitigate, and control evils which will always more or less defy our attempts to entirely counteract or prevent. It is in this field of practical research that the most valuable results are to be looked for in the navy and the army alike; and it will be found, we think, that the very same hygienic and prophylactic measures which are known to be most effectual in respect of typhus and of diarrhœa, and dysentery, are those which should guide us in respect of yellow fever and cholera.

Some very interesting details are given of what befel the expedition to Mexico in the beginning of 1862, in conjunction with the French and Spaniards, and from which we happily withdrew in time, but not before much sickness and a considerable mortality in the force landed at Vera Cruz, consisting of 700 men and officers of the marine battalion. The place is notoriously one of the most unhealthy in the Gulf, and the accommodation found in the town was neither good nor sufficient. The Spaniards, who were the first to arrive, had taken possession of the best quarters, and the French followed immediately upon our heels, and must have fared even worse than ourselves. Within a month of the arrival, there was a heavy sick list from diarrhœa and fever. A month later, or in the first week of March, the sickness had much increased, and yellow fever began to appear. This was the signal for the re-embarkation of most of the troops, and for removing them from the country; they were sent off to Bermuda. A detachment of 154 men and 10 officers remained behind till about the middle of April, when they also left. The total mortality in the whole force amounted to 46 deaths, in the course of three months and a half. Twenty-six men died of fever in the hospital at Vera Cruz; ten of these were cases of undoubted yellow fever. The losses of the French and Spaniards were, it is believed, very much greater. As far as may be gathered from the evidence, the yellow fever seemed to be but an aggravated and more deadly form of the endemic fever; gradually manifesting itself and gaining strength and violence as the season advanced. How difficult, if, indeed, possible, it is to discriminate between milder cases of yellow fever,—*i.e.*, those which recover—from the ordinary remittent fevers, is known to all. This fact alone should suffice to shake us out of any extreme views as to the essentially specific or pathognomonic nature of the former, as if it were as distinct and independent a disease from all others as smallpox or other of the exanthemata. The history of the sickness in the *Rinaldo* by her surgeon, Dr. A. L. Archer, as given at length, might be quoted with advantage on other points connected with the attributes of the fever, besides its alliance with intermittent fever; it will well repay a careful perusal. We can afford space for one short extract only :—

"On its first appearance the existence of yellow fever was barely suspected, but very shortly the true genuine type began to evidence itself in two or three of the cases. Of the twenty-one that appeared, fourteen ran into true yellow fever; the remaining seven were of a milder remittent type, and more amenable to treatment. From the first the cases were isolated as much as possible from the body of the ship's company. For four days after leaving Key West, there being danger of squalls of rain, it was necessary to have the patients on the lower deck, but as soon as a parallel of latitude was reached where there was less probability of this occurrence, the ship was housed in abaft with the awnings, the cots and hammocks slung underneath them, and the isolation was as perfect as possible. As the cases of fever appeared from the first to be daily on the increase, and its malignant type soon evident, the ship's course was diverted from making the Chesapeake to Halifax, and she was run to the northward as rapidly as possible. During the passage everything was favorable to check the further process of the affection, and to conduce to the comfort of the patients. Very fine weather was met with, and a fair wind with a smooth sea rapidly sent the ship into higher latitudes."

There is in the appendix to the 'Army Report,' an elaborate and very valuable memoir by Surgeon-Major Barrow, on the epidemic of yellow fever in Bermuda, in 1864; it would require a separate article to do it justice, but this will be best reserved until the 'War Office Report' on this destructive visitation appears. It may meanwhile be worth noticing that, in the previous year, there was a marked increase of continued fever among the population, both civil and military, in the towns of St. George and Hamilton. For some weeks previous to the appearance of yellow fever in 1864, there seems to have been an unusual number of cases of jaundice and of a mild typhoid fever in St. George's. The first true case of the pestilence occurred on the 25th of June; the second on the 10th of July; both in persons recently arrived in St. George's. The evidence in these two cases, when examined, quite failed in Dr. Barrow's opinion to support the alleged importation of the disease. "It altogether points to the town of St. George's as being the starting-point and nucleus of the fever; and as the two men first attacked were new comers, they were particularly susceptible to the toxic influence." The result of the official inquiries into the history of the previous three epidemics in Bermuda accords very much with the views of Dr. Barrow on the important question as to the introduction *ab extra,* or the indigenous development, of the yellow fever there.

On the connected subject of quarantine, he remarks :—

"The Commissioners appointed, in 1857, to inquire into the causes of the outbreak in the previous year determined that the disease originated in the colony was non-contagious, but diffused by an epidemic constitution of the atmosphere, exploding in attacks of yellow fever where it encountered other necessary elements,—as predisposition in individuals, bad sanitary conditions in localities, or both combined. They therefore recommended that quarantine should still be applied to ships, but to ships as infected localities only—that is, any vessel having yellow fever cases on board should be herself placed in quarantine, but her crew allowed to depart. The vessel should be then cleared out, cleansed, whitewashed and ventilated, &c. This is exactly my opinion on the subject of quarantine. When a supposed case of contagion is brought forward, the question is to determine how much depends upon the local and how much upon the personal cause; and the difficulty in wholly eliminating the local influences is so great that the exact value of the personal can scarcely be estimated, and thus the argument seems likely to go on *ad infinitum*."

Some excellent advice is given as to the sanitary measures required, both in the towns and as respects the military establishments, to prevent the recurrence of such desolating visitations as have, more than once, within the last twelve years smitten the colony. The consideration of this and of other topics relating to the recent epidemic will, ere long, call for a special notice at our hands. Meanwhile all who are interested in the subject will do well to consult a most instructive paper (illustrated with a chart of the group of the islands) by Deputy-Inspector Dr. Smart, R.N., who was recently the principal naval medical officer at Bermuda, in the last part of the 'Transactions of the Epidemiological Society.' His views correspond very generally with those of Dr. Barrow, and serve to throw much light on the epidemiological relations of the disease. Dr. Mackay is an ardent contagionist and upholder of the efficacy of quarantine for the exclusion of yellow fever, more so, we should suppose, than most of the medical officers of his service. "Accumulated and accumulating evidence," he says,—

"Seems to show, that while on board ship, on the one hand, yellow fever may certainly be altogether escaped by carefully avoiding those localities in which it is prevailing, it may on the other be most effectively kept at bay on shore by the stringent administration of quarantine laws."

From yellow fever we pass on to cholera.

Respecting the important measure of moving troops from infected localities, and the necessity of selecting proper sites for their temporary encampment, the following observations will be read with much interest, followed as they are by a most useful suggestion from Dr. Balfour:

"At Agra cholera appeared in the beginning of July, but did not attack the troops till the 21st, when a case occurred in the Royal Artillery. On the 28th the two batteries were moved to Secundra, 'a large walled enclosure,' and the disease still continuing, they were again moved two miles further on the Muttra road. As this change did not appear to have any influence in checking the epidemic, they were, on the 4th of August, removed across the river, when the disease immediately ceased among them. On the 31st of July cholera broke out in the 23rd Regiment. On the following day two companies were sent across the river and encamped. The disease still continuing in the cantonments, three more companies were sent across on the 6th and 7th of August; and it is worthy of remark that not a single case of the disease occurred among the men encamped on the left bank of the river. The Artillery had altogether 18 cases, of which 11 died, and the 23rd Regiment had 14 cases and 10 deaths.

"The history of these outbreaks of cholera furnishes strong evidence of the importance of moving the troops and encamping them with a view to check the disease when it appears as an epidemic, as directed in the General Orders of 24th April, 1862, by his Excellency the Commander-in-Chief in India; but it may be worthy of consideration whether such a measure would not be advisable for the purpose of *preventing* its occurrence in that form among the troops. It seems to be almost invariably the case that cholera appears in the first instance among the native population, in the neighbourhood of the cantonments, and that it does not attack the troops till after the lapse of a short time. It might deserve a trial whether, by encamping the troops on the first appearance of the disease among the general population, and keeping them encamped until it had subsided, the occurrence among them of these severe and fatal epidemics might not be altogether, or at least to a very great extent, prevented."

In the report of Inspector-General Beatson we learn some further particulars respecting the sanitary, natural and acquired, condition of the cantonment of Mean Meer, and of its past history. It has been long known to be an unhealthy locality. There was a severe outbreak of cholera in 1831, again in 1842 and in 1856, and then the terrible ones in 1861 and in the following year. During the six years commencing with 1846-47, the annual death-rate among the troops stationed there was 84 per 1000. In 1851-52 one regiment lost 132, and another regiment no less than 218, per 1000 of its strength, chiefly from fever and dysentery. Subsequently to this period the cesspool nuisance, which everywhere prevailed, must have become, of course, very much worse. What with crowded barracks, the use of water not only brackish, but tainted with fæcal impurities, and the depressing effects of panic, no one can wonder at the disasters of 1861 and 1862. Dr. Beatson expresses emphatically his hope that in future "bodies of men struck with the pestilence would never again be left to its devastating ravages without early and decided action being taken to remove them from infected localities."

The closing paragraph of Deputy-Inspector Dr. Home's report on the epidemic cholera at Shanghai, in 1862 and 1863, is full of practical instruction:

"Four marked characteristics distinguish the better class of the European community, who, I may observe, scarcely suffered at all during the two cholera epidemics, from their less fortunate fellow-countrymen the private soldiers, on whom the disease fell on both occasions very heavily.

"1st. As a rule, they (the civilians) all live extremely well, partaking of animal food always twice, and often thrice a day. 2ndly. They inhabit large airy dwellings, and never sleep, if at all possible to avoid it, on the ground-floor. 3rdly. They never expose themselves to the mid-day sun, and scarcely ever to the night air, except in covered (sedan) chairs. 4thly. They bathe in their own houses every morning, and in the afternoon, either by walking, riding, drilling as volunteers, playing rackets or otherwise, they take care that the system has regular and sufficient exercise. I confess I see but little difficulty in giving the soldier the benefit of all these advantages. If his diet is improved, and I am inclined to lay much stress on this (utterly interdicting the use of salt provisions), his barracks made spacious and airy, sentry and all other out-door duty reduced to a minimum, if bath-rooms and the means of amusement and recreation above mentioned are provided for him, and if the European corps stationed at Shanghai, whether infantry or artillery, be not kept in China beyond the regulated period of three years, I see no reason why the soldier, even at Shanghai, should not approximate at least, if he does not actually reach, the high standard of health in which the civilians in comfortable circumstances there enjoy."

As the Indian troops serving in China appear to have been decidedly less liable to the disease than the European soldiers, and also to recover from its attacks in considerably larger proportion, Dr. Home recommends that "the European portion of the garrison should be reduced as low as possible, and the protecting force be made to consist, as far as practicable, of Indian troops."

If the well-conditioned Europeans at Shanghai suffered comparatively but little, very different was the fate of the poor native population, as we learn from the surgeon (Dr. Morgan) of the Euryalus:

"The mortality among the unfortunate Chinese who had sought refuge in the settlement surpasses belief, which is not to be wondered at where there were so many human beings crowded together in indifferent dwellings. I have been informed upon good authority that they died at one time at the rate of 3000 a day, and as they could only procure a burial-ground that could accommodate 1000 a day, the remaining 2000 were enclosed in boxes of deal boards loosely put together, or packed up in straw or matting, and left to rot in the sun. During the time this ship was at Shanghai there were thousands of dead bodies, more or less hidden from the sight of the passer-by by some loose covering, that were in a state of decomposition, and tainting the surrounding atmosphere with their deadly effluvia."

The crew of the Euryalus (as well as of some other ships of the squadron) suffered severely, 21 out of 39 cases proving fatal. Dr. Morgan considers that the use of the foul river water had much to do in the causation of the disease. "I am inclined to believe that the malarious poison as well as the choleraic poison was conveyed, in this instance, through the river water, which the men freely drunk of." He adds—

"Although I am inclined to believe, from former experience during the prevalence of a choleraic epidemic in 1854, in the Black Sea, that the disease is communicable from one individual to another when there are a number of cases crowded together between the decks of a ship, it is my conviction, after mature deliberation, that the disease was not propagated by infection during the Kahding expedition. The evidence (though not conclusive) is more in favour of its having been conveyed to the human system through the medium of the river, than through that of any other, in this instance. The usual sanitary precautions were promptly adopted immediately after the return of the men to the ship."

But a still more formidable, in many respects, disease than cholera, though relatively, or to the number of the attacked, much less fatal to our ships of war in these latitudes, is the ordinary bowel flux, or dysenteric diarrhœa, which at all times, and in almost all seasons, proves so destructive to the health of the men. Out of a crew of 520 men, the Euryalus had 54 cases entered as dysentery, and 288 cases as diarrhœa, during the six months she was on the station. On arriving at Yokohama the health of the ship was highly satisfactory, the number on the sick-list being only twenty-two. In a few days diarrhœa became prevalent; in consequence, Dr. Morgan thinks, of the bad quality of the water supplied to the ship by the contractor, who obtained it from a reservoir "situated in a paddy field, which was manured with human ordure, and where the Japanese inhabitants were seen to wash their soiled linen." One naturally asks how it comes to pass that our ships of war are left so entirely at the mercy of contractors in a matter of such importance to the health of the crew as the quality of water for their use, and that so little appears to be done to have all the suspected water distilled on board, at least all that is intended for drinking?[1] But the water is only one of many other matters that require the utmost attention in the management of the health of men, in a locality that is infested with alvine flux. There is not a more important problem in naval and military hygiene than clearly to determine all the circumstances which favour the production and aggravate the severity of this disorder. The food, the drink, the clothing, the exposure, the nature and amount of fatigue duties, the accommodation of the men, the moving of ships, the site of barracks—these and many other points require to be carefully examined into. We should like to have ample details from the medical officers of the navy in respect of some of these sanitary particulars; and the more so as there is often no small difference as to the amount of severity of flux in different ships, apparently equally exposed to the hurtful influence of the climate, &c. Would it not be a most useful measure to require of the senior medical officer of the station, or the surgeon of the flag-ship, to prepare a report at the end of the season of what he may consider as the probable cause of such differences in the ships of the squadron? By comparing, on the spot, the health-state of different ships on a station, and at the time when the events occur, the most valuable information would unquestionably be obtained for future guidance.

As to the medicinal treatment of dysentery and dysenteric diarrhœa, the experience of Dr. Morgan is much in favour of the old-established remedies:

"I found great benefit derived from the use of ipecacuanha in small and frequently repeated doses. I gave two or three grains of the drug combined with one quarter or one sixth of a grain of opium, with marked beneficial effects, every two or three hours, according to the urgency of the cases. In from thirty-six to forty-eight hours under this treatment the tormina and tenesmus ceased, and the stools became copious and feculent. Whether directly or indirectly, the remedy acts upon the liver, for under its use the stools became copious and bilious. Probably its action upon the skin and mucous lining of the intestines relieves the congested por-

[1] The addition of the elixir of vitriol, or dilute sulphuric acid, in such proportion as would make a pleasant beverage when sweetened with a little sugar and flavoured with tincture of orange peel, has been suggested as a useful and palatable drink in hot climates and seasons, and also in cholera years in our own country. This is the *limonade sulphurique* of some French hospitals. There can be no question but that the continued use of the mineral acid must have no inconsiderable prophylactic influence, both directly as an astringent upon the gastro-intestinal mucous surface and also by serving to counteract various impurities in the water.

tal vessels, which no doubt in these cases obstruct the flow of bile through the hepatic ducts. In the cases which were attended by well-marked paroxysms of paludal fever, full doses of quinine at the onset of the cold stage were given with advantage."

The Appendix to the 'Army Report' contains several most valuable reports on medical topography. Among these, besides one or two already cited, may be mentioned that by Staff Assistant-Surgeon Gardiner on the Gold Coast, and on the expedition up the country against the Ashantees, which turned out so disastrously to the troops, and which called forth, it will be remembered, severe animadversions in the House of Commons at the time. Think of such a state of things as this:

"Hardly an officer was in sound health by this time, and none had escaped the fever. As already stated, Captain Gabb had died on his way down in his hammock, and with none but natives near him. Staff Assistant-Surgeon Hooper also died just as he reached the coast from the bush. These things, with the fast increasing list of sick men and officers, served to augment the depression among those who remained in the bush, and by the 1st of April eleven officers had gone down to the coast sick.

"The hospitals were now overcrowded—the men suffering from fever, diarrhœa, and dysentery, and lying on the damp ground, in many cases with pools of water around them. It became utterly impossible to make sufficient accommodation for the number of sick.

"The rains had fully set in, and storms of thunder, with furious rains, soon turned the camp into a large swamp; the roads became flooded, slight streams were turned into torrents, and the rivers were impassable until canoes were sent up from Cape Coast, and then the sick had to be ferried over two and two.

"On the 9th April, 1864, the troop ship Tamar arrived off Cape Coast Castle with reinforcements, consisting of the left wing 4th West India Regiment, and two companies 1st West India Regiment. As room in the castle for this increase was impossible, all the houses available in the town were hired, and necessarily without reference to their fitness for barracks."

To talk of carrying on war under such circumstances, and in the climate of West Africa, is worse than folly, even with black troops from the West Indies, as the following remarks show:

"All West Indian soldiers suffer greatly from change of climate. Many, if not most, of the 4th West India Regiment suffered more from the effects of this climate on their first arrival than white men would.

"Thus, the 1st detachment of the 4th West India Regiment landed on the coast in August, 1863, and were at once attacked by fever and dysentery; before December of same year they had somewhat recovered (by no means wholly), and then were employed in the interior, the inevitable results of which, following so soon on constitutions lately shaken, told with most serious effect."[1]

At length the welcome news arrived, in June, from home that the expedition should be abandoned, and that, after all the military stores had been destroyed, the troops should be brought down at once from the interior to the coast, to be, most of them, sent back to the West Indies from which they had come. It is to be hoped that this valuable report of Dr. Gardiner will not be overlooked, whenever expeditionary military operations on the African coast are again thought of.

From the surgeon of H.M.S. Gorgon we have an exceedingly interesting account of the mouth and lower part of the Zambesi, in connection with the aid given by the navy to Dr. Livingstone's exploring expedition, and of the remittent fever which proved so fatal to the Church of England's well-meant, but surely very imprudent, mission to that part of the east coast of the African continent:

"'On the 2nd of February a detachment of six officers and forty-eight men crossed the bar at the mouth of the Kongoni, and anchored close to the right bank. Double allowance of spirits was ordered, and three grains of quinine were directed to be given every morning in wine. I accompanied the expedition myself. . . . . . After steaming on for six days, and finding that from the stoppages for wood, getting aground, and the difficulty the steamer had in stemming the current, we had only made sixty miles, the captain, the paymaster, and myself, with twelve men, left in a gig and whale boat to take the two ladies belonging to the Central African Mission up to Chibisa, immediately below the Murchison Falls. We took ten days' provisions with us, and on the tenth day we had only reached the entrance of the River Rico, about sixty or seventy miles below Chibisa. Here one boat with the paymaster and five men returned.' . . . . . The remainder proceeded onwards, ultimately reached Chibisa, where they found that the bishop had died, and that the mission party were in difficulties for provisions. After communicating with them they returned at once down the river, and, after hard work, pulling day and night, reached the Pioneer in three days. There they found that the other boat that had returned had

[1] The susceptibility of the West Indian negro on first arriving in Africa seems to be an understood fact. Captain Trotter states, in the Parliamentary Report of the Expedition of 1841–42, "that the constitution of the negro, whether of African or American birth, requires an habitual residence in Africa to be exempt from the fever of the country. This is found to be the case in Liberia with the emigrants from North America; they all, with few exceptions, have fever on their arrival, and many die; but those that recover are said to stand the climate afterwards.

"This was fully borne out by the difference in the health of the 4th West India Regiment and that of the detachment of the 2nd West India Regiment, both being subjected to the same influences."

arrived with four men down with fever, and the other two had had fever since. 'During our stay at the mouth of the river every one with the exception of myself had an attack of fever, viz., two officers and twelve men. The gig's crew were attacked there for the first time, the others were all secondary attacks. The mouth of the river, however, was not to blame, for none of the brig's crew, or of those we left on board of her, in all twenty men, ever had a single attack of fever. They had no quinine as a prophylactic, and had been lying in the same place for two months. The wind was from seaward the whole time, which I believe was their safeguard. On board the ship there was no increase of sickness till the 7th of March. Individual cases of fever had occurred, but the principal part of the sick list consisted of diarrhœa and slight surgical cases. On the 6th, however, it blew a heavy gale with constant rain; every one on board got wet; the gale lasted for several days, and on the 7th and 8th twenty-nine cases of fever were added, and before the end of the month only six out of the fifty-four who had gone up the Zambesi escaped an attack."

The terrible plague of the mosquitoes increased not a little the susceptibility to attacks of fever; the irritation was excessive, so that the men could never have any quiet sleep at night. For the details of the symptoms, the mode of treatment, and the results, we must refer the reader to the report itself, which is well worth perusal, and shows what a hotbed of malarial fever this part of the African coast is, and the inevitable hazard to all European missionaries in such a climate; but we must find room for the following practical general remarks on the health of ships' crews on that station:

"Our experience seems to warrant the conclusion that a ship stationed on the East Coast of Africa and Mozambique Channel may expect to visit every port and harbour of the coast without contracting either fever or dysentery, if, as in the case of the Gorgon, her decks be spacious and ventilated by large ports, if she be able to condense water so as never to water from the shore, and if her crew be not exposed in long-boat cruises.

"2nd. That the danger to which the men are exposed in boat cruising, though it will certainly involve some cases of fever, and must severely try the constitutions of the weaker men, is much lessened by avoiding night anchorages in rivers, or sleeping on shore.

"3rd. That boat expeditions up rivers, as that of the Gorgon's boats in March, 1862, up the Zambesi, involves danger of a different nature, and exposure to an inevitable morbific influence of a kind peculiar to these rivers."

In view of the intended adoption of the overland route for the transmission of troops to and from India, there is given an abstract of a report on the medical topography &c., of Suez, by Dr. Fraser, who was sent to organize the hospital there during the Indian mutiny, and which was closed on its suppression. At the present time, when public attention has been specially drawn to Egypt in connection with epidemic cholera, the following particulars about Suez are not without interest:

"In a place like this, which is not only the great highway for intercourse and traffic between the two great western and eastern quarters of the globe, including Australasia, but forms, besides, the chief rout for the devout of the Islam world, both in Asia and Africa—from Bokhara, Turkey, and the Crimea, to the very centre of North Africa—on their journey to and fro for the special object of worship at the shrines of their faith at El Medinah and Mecca—the population must at all times be a floating one. It is especially so for several months in the year, during the period at which this pilgrimage is performed; and there is a further floating population at Suez, as a local port, in visitors from the various ports in the Red Sea with which it holds commercial intercourse.

"Amongst this floating population both births and deaths occur. The invalid from India, in search of health to Europe too late, not unfrequently just lands to leave his bones here. Sick from the two great Steam Navigation Companies' vessels are landed to die here.

"Whole boat-loads of small pox from the ports of the Red Sea are sometimes landed to die."

In reference to cholera at Suez, Dr. Fraser says that—

"It has been always distinctly traced to importation, and has been in every instance brought by the pilgrims on their way either to or from Mecca, but in one instance only *from* Mecca, and that on the first occasion of its general spread from the East in 1830. In late years, its course has been invariably from Alexandria to Cairo, and thence to Suez."

The complaint of the old patriarch in Mesopotamia that "by day the drought consumed him and *the frost by night*," is strikingly illustrated by the following passage:

"A large double-walled tent has also been proposed for the the purposes of an hospital at this station, but that I could think of only as a last resource, even as a temporary measure. Besides being most uncomfortable for many reasons arising from local peculiarities, it would be found unsatisfactory in a sanitary point of view. In the desert the air is very keen at night at all seasons, and in the winter months the cold—that is, the *feeling* of cold—is the most piercing, *penetrating* I ever experienced. My impression is that a tent could be very little, if any, protection against it. This could not but prove certainly prejudicial to Indian invalids, whatever form of disease they might be labouring under. The closeness of the water to the surface, too, as already described, would render a tent, on such ground, very unfit for an invalid, and further, it would be impossible to keep the sand out, and that horrible torment, the flies, still a grievous pest in Egypt."

On the prevalence of ophthalmia in the army, a paper by Deputy-Inspector Dr. Lawson, principal medical officer of the Cape of Good Hope Command, deserves notice; it is treated in a philosophical spirit, is illustrated by a projected diagram, which represents the varying prevalence of the disease in different stations during a long series of years, and is altogether a model of how such inquiries should be carried out. He justly remarks that—

"Investigations into the causes of diseases of the eyes in the army have hitherto been based on facts derived from too limited an area, and from too short a period of time. Had the examination embraced different countries and a longer period, it would have been found that this class of diseases varied much in frequency at the same place at different times, and at different places at the same time; though many of the circumstances to which the men were exposed, and to which affections of the eyes have been attributed, remained with little or no change, for many years in succession, at each or several of the places where such different results were obtained."

The general result of the data collected together by Dr. Lawson is thus given:

"These facts indicate that, in the production of ophthalmia, as with fever and cholera, there is a cause concerned of a very influential nature, and of far more extensive operation than any connected with a particular station or garrison, and, unless its operation is allowed for, no real advance will be made in estimating the importance of the ordinary exciting causes to which the origin of the complaint is more commonly attributed."

Rheumatism is always a prolific cause of sickness and disablement in the Navy. The following table shows the mean ratio of cases for the six years preceding 1862, on the different stations, and serves to show the influence of climatic agency in its production:

Home	64·7	Per 1000 of mean force.
Mediterranean	79·2	
North America and West Indies	67·7	
Brazils	80·6	
Pacific	109·1	
West Coast of Africa	109·2	
Cape of Good Hope	74·8	
East Indies and China	99·6	
Australia	99·3	
Irregular Force	90·3	

The connection between the frequency of rheumatic ailments in warm climates and the influence of malaria, or at least of the effects of malarial disease on the system, is well brought out in the subjoined remarks by Dr. Mackay on this table:

"It is thus seen that next to the Home and North American and West Indian Stations, the smallest ratio of cases of rheumatism during a period of six years is on the Cape of Good Hope Command. The services of the squadron on that station, however, both in 1861 and 1862, were of such a nature as to assimilate it closely to the West Coast of Africa. The constant blockade made in the Mozambique Channel for the purpose of checking the slave trade which was being carried on amongst the Arabs, and the exposure to which the men were constantly subjected, no doubt induced a condition of the system similar to that which is found to obtain amongst the crews of the squadron on the West Coast; and it is a significant fact that during these two years the ratio of rheumatic disease should have risen from 60· and 70· per 1000, between which it had ranged during the five previous years, to 115·4 in 1861 and 121·1 in 1862. It is further to be remarked, as bearing upon the probable connection between the increase of rheumatism in these two years, and the exposure of the men to malarial influences, that while during the five years preceding 1861 the ratio of cases of ague on the Cape Station ranged from 9· to 17·2 per 1000, it was as much as 37·8 per 1000 in 1861 and 154·2 per 1000 in 1862; the ratio of continued and remittent fever during the same years being respectively 122·3 and 210·1, while during the five preceding years the range was from 15·7 to 86·9.

"If it were necessary, however, to adduce evidence that other influences than those of a malarial character excite rheumatism, it would be found in the foregoing table, where it may be seen that the stations on which this affection most abounds are the West Coast of Africa and the Pacific, stations, as previously remarked, dissimilar to each other, not only in their physical characters, but in the duties which devolve on their respective squadrons; and it is curious at least to observe that the next most important stations as respects this disease, and between which also the difference of ratio of cases is merely fractional, are the East Indies and China, and Australia, than which no two stations can be more dissimilar in every respect. Some solution of the difficulties surrounding any attempt to explain this apparent enigma may be found in the fact that while on some stations malarial influences beget a cachectic state of the system, which is liable to be accompanied with rheumatic and pseudo-rheumatic symptoms of varying severity, on other stations, on which no such influences exist, rheumatism may be engendered by peculiarities of climate, impairing the harmony which ought to subsist between the skin and the other emunctories of the body."

Our limits now remind us that we must be drawing our review of these annual reports of the two public services to a close; but we must not omit briefly to notice the results of the experience of Netley Hospital during the first nine months after it was opened in March, 1863:—3567 patients had been admitted, chiefly invalids from India and the colonies. The bulk of the cases were of phthisis, dysentery, hepatitis, and secondary syphilis. The objections which had been made at first to the site of the hospital, on account of the alleged or apprehended malarial character of the muddy foreshore at low tide, have been found to be groundless. With respect to the plan of the building, which had also been condemned by Lord Herbert's committee, Dr. Anderson remarks—

"The corridors of the hospital have afforded the patients, in bad weather, opportunities of exercise which would have been denied them had those spacious and lofty passages not been in existence. I am not prepared to defend the corridor system in this or in any other hospital; but I would, with the utmost deference, submit that with the peculiar class of patients (so few being bed ridden, and there being, I may say, no cases of fever or other infectious diseases) no grave objection to it can exist."

The air in the corridor was found to be, for the most part, very pure. Occasionally, however, an odour, arising in some part of the hospital, might be traced for some way along the corridor, showing that great care is necessary to prevent the transferrence of foul air through the corridor from one spot to another. Many other interesting particulars are given by Professor Parkes about the ventilation of the wards, which well deserve the attention of the military student.[1] The subject of the ventilation and warming of rooms continues to excite much attention in our military buildings, as shown by several papers in the sanitary section of the army report, to which we must refer for particulars. It is surely much to be desired that the attention of the navy was equally engaged in the consideration of this most important matter in respect of our ships of war; for until this be done, and measures be taken to improve the condition of the atmosphere in the berths of the men, it is in vain to look for any considerable diminution in the ordinary rates of sickness and mortality in the service generally, as there certainly might be, or for the prevention of those epidemic outbursts which, every now and then, prove so deadly to many vessels on certain stations. We should like to find that the subject was more frequently and more pointedly alluded to in

[1] The reports of Dr. Parkes on the progress of hygiénic science from year to year will repay perusal by all medical men, whether in the public service or in civil life; they are always interesting and instructive.

the annual reports of the navy than it is, as its omission might lead the reader to imagine that its hygiénic importance is not duly and sufficiently recognised at the Admiralty. We trust also, ere long, to see the example of the army followed by having an appendix to the reports, so that space be given for the insertion of the most valuable contributions of the medical officers on all matters relating to the health of the service. The results of the experience from year to year in our naval hospitals, at home and abroad, could not fail to be most instructive. We doubt not that the present Director-General, whose past labours have done so much for nautical medicine, is cordially disposed to aid every effort to advance the interests of the department over which he presides, and to procure for it all the advantages which the corresponding department in the sister service has enjoyed of recent years.

The introduction of outline maps, showing the boundaries of the different stations of our fleets, and also the index to all the ships and stations, together with a list of the medical officers employed, are acceptable and useful additions to the present report. If the maps contained all the places named and referred to in the body of the report, their value to the reader would, of course, be much enhanced. How is it that the navy is always a year in arrear of the army in respect of the annual reports of their service? It needs not, one should think, a lapse of three years to have received and digested all the returns of the medical officers afloat before the information can be made known. The delay is the more to be regretted as it prevents a timely comparison being made of the health of the two services for the same year.

Review VI.

1. *Sanitary Commission. Hints for the Control and Prevention of Infectious Diseases in Camps, Transports, and Hospitals.* —*New York,* 1863, pp. 36.

2. *London Vestries and their Sanitary Work: Are they willing and able to do it? And may they be trusted in the face of a severe Epidemic? Being a speech delivered August 8th,* 1865, *at a meeting of the Vestry of St. George-the-Martyr, Southwark.* By W. Rendle, Vestryman, and late the Medical Officer of Health of the Parish, &c.—*London,* 1865, pp. 34.

3. *The Administration of the English Poor Law.* By Frederic Hill, Barrister-at-Law.—*London,* 1865, pp. 13.

We have brought together for notice the papers above named for this reason, that they bear on and help to illustrate each other.

We have already, in the present and former numbers of the 'Review' called the attention of our readers to the noble exertions of the United States Sanitary Commission, made during the great civil war, now happily ended, terminating in the extinction of slavery. The first of the above papers, written by Elisha Harris, M.D., &c., is one of the many which this Commission has issued for the use of the medical officers of the United States Army, now published in a collected form, edited by the late Surgeon-General, Dr. Hammond. It is purely practical, and shows, teaching by example, the direful effects of the neglect of measures of hygiéne, and the necessity of segregation to arrest the spread of infectious diseases. We quote one instance out of the many given:

"In the month of December, 1862, a first-class new regiment arrived in New York, *en route* to the seat of war, from its rendezvous in Massachusetts. A case of scarlatina appeared during the trip hither. The men were assigned to close and insalubrious quarters in the lofts of a warehouse in this city. The cases of scarlatina continued to multiply from day to day and from week to week, until nearly two hundred cases had occurred, and until one of the most promising regiments ever organized had become so broken down by this cause that the dead, together with the discharged invalids, amounted to an aggregate of nearly two full companies. This regiment left its pestiferous quarters in the city, after a detention of about two months, with its military effectiveness diminished, as estimated by its veteran colonel, fully *thirty per cent.* upon its original strength of one thousand men. And it is worthy of mention here that the local conditions that had rendered scarlatina so fatally endemic also prostrated the strength of every man, and induced various inflammatory and *typhous* maladies, which broke down the health and vigour of men who seldom had known sickness previously."

After insisting on the absolute necessity of attention to sanitary precautions, and the isolation of the infected, for securing the health and efficiency of troops, directions are given for enforcing them. Very properly the greatest stress is laid on thorough ventilation, as the *sine quâ non* of success, without which and cleanliness disinfectants are pronounced to be of little avail. The disinfectants recommended we need not particularise, except that bromine in vapour, hitherto little used, is adverted to as one of the most powerful, with the caution, however, that it must be employed with great care, owing to its irritating effect when inhaled.

The whole of this paper, drawn up with a great knowledge of the subject, is specially deserving the attention of all officials who have the superintendence of the public health.

And we can say the same of Mr. Rendle's powerful appeal to the London vestries in behalf of the poor. The title of his speech expresses its import; and we regret to learn that the facts which he brings forward clearly show that these vestries are negligent of their duties, and that too many of their number consider the evils oppressing the labouring poor as irreclaimable, and in consequence are disposed to leave them to their wretched fate, inhabiting dwellings altogether unfit for human beings, and, owing to the various unsanitary conditions of these dwellings, better adapted to engender disease, occasion discomfort, and promote vice, than to preserve health and its right concomitants —ease, contentment, and moral conduct. The prevalency of fever may be considered as a pretty correct criterion of the neglect of hygiéne, comprised in crowding, dirt, imperfect ventilation, neglected drainage, deficient water supply, impure water. During the last six years the deaths from this disease in the very parish the vestry of which is addressed has been 429, representing 3400 cases, and these preventible had the sanitary conditions been what they ought to have been. He says—

"It is notoriously the case that the parish is deficient of sanitary inspecting power, and that there is no organization to speak of for obtaining information from others. Moreover, the poor appear to be treated as troublesome if they dare to complain."

He adds—

"It is absurd to expect an officer, in a part only of his time, to inspect regularly and efficiently at least between two or three thousand houses, inhabited by the poorest; to see that shifty agents do the work ordered; and that dirty, impudent, ignorant people do not destroy the work when done."

He expresses an opinion regarding vestries which he says—

"Is coming forth every now and then in indignant words of the press, that vestries are altogether failing in the main object of their duty, and that, so far as the usual majorities are concerned, a power of passive obstruction is uppermost."

He refers with reprobation to one speaker, who maintained that it is of no use helping the class of people in question, impressing his dissent with the remark "that to abandon even the worst and most hopeless is not Christianity," these being, above all others, the very classes he professes to take up. He says well—

"I don't know that the religion which we practise and exhibit on Sundays is of much use to us, or anybody else, unless we can bring a little of it here, or carry it into daily life. We raise (he continues) enormous sums to send its truth to the lowest of human beings afar off. I can fancy some shrewd heathen puzzling some new Colenso. I can fancy such questions put to the missionary as these:—'Is it true, sir, that in the country you come from, low people live by tens of thousands in such a way that it is considered almost impossible to teach them; that you are come out here thousands of miles to teach me, and that these tens of thousands are at your very doors and close to your religious temples?' 'Yes,' would the missionary say, 'I know of small districts, called parishes, in which twenty thousand people live in filth and wretchedness, and live and sleep together without regard to age or sex, quite contrary to the precepts of the gospel I now come to teach you. These people, supposed to have no sense of shame, can in no way attend to even the common necessities of the body without disregarding the feelings of decency which more or less prevail in all countries, Christian or heathen; and this has become so common, that within a stone's throw of some of our Christian temples we may find thousands so situated; and so common is it that we are become callous at the sight of it, and almost hopeless of a remedy for it.' We need not follow the further questioning and replies, the latter all confirmatory and aggravating, leading to the heathen's response:—'Enough; tell me no more, brother Colenso; I and my black brother will wait, for, as your religion tells me, the tree is known by its fruit. It is well that you should send us the great message which so many of you think the noblest in the world; but it is not well that you should come to us until you have earnestly visited day by day all these places, and have done all that man can do, and money can do, to make both worlds a little better for this great multitude of people.'"

After an appeal to the ministers of religion for their aid, Mr. Rendle gives an opportune warning:

"We had better be warned in time. Some points now agitated in the political world are as to the representation of the masses. We had better try to civilise the lower millions, I think, or some day or other the brute passions which were let loose in the French Revolution may be let loose here. Placid content cannot always last with such a state of things as I am portraying here, in St. George-the-Martyr, such as the health officers of the City and Whitechapel have reported of their districts, and such as Dr. Hunter has portrayed all over rural England."

What does Mr. Frederic Hill say? He starts with the question, "Whether the present administration of the Poor

Law creates any obstacles to the reform of criminals and the suppression of vice?" And he replies in the affirmative, resting his opinion on his experience as Inspector of Prisons, an office he once held, and maintaining that the main causes, the great parents of crime, are bad training and severe want:

"That severe want is a powerful incentive to crime (he says) few will probably be prepared to deny; but any one who has doubts on the subject may be reminded of the frequent bread riots which used to break out; of rick-burnings; of the sudden and lamentable increase of female prostitution after one of the strikes at Preston; of the murder of the Rev. Mr. Hollest by a man who, from the press of applicants, had been refused admission to the refuge at Westminster; of the terrible case, in the records of capital punishment, of Mary Jones, who, to procure food for her children, when her husband had been seized by a press-gang, committed a small theft, for which she was afterwards hanged; and the well-known fact that robberies are most frequent in that part of the year when work is most difficult to procure—the winter. Reference may also be made to the saying of the first Napoleon—a saying indicative of his keen observation—that there was no insurrection which did not originate in the belly."

And he (Mr. Hill) adduces instances more than sufficient to show how very ill the pauper class is cared for, especially in London, by those who are called their guardians; and how inadequate often are the existing means of relief to the exigencies of the times. He controverts, we think, very ably the idea that adequate relief, implied in the complete working out of the principle of the English Poor Law, and the free admission into the workhouse of all who are able to work, would entail much additional expense on the country, no one being so expensive to the state as a criminal at large, and that very measure, therefore, which diminishes the motives to crime effects an important saving.

The influence of good training and education, as contrasted with bad, he happily illustrates by the results. A case in point is the following. It is so instructive and so deserving of being followed, that we give it in his own words:

"Some years ago I had an opportunity of making full and minute inquiries into the after-life of the children brought up in the large workhouse of Birmingham—or rather in a separate school, or asylum, as it was then, and probably is still called, in connection with the workhouse—and the examination showed an almost marvellous result from the good and judicious treatment which the children received, and from the habits of industry in which they were trained. At that time the asylum usually contained about 300 children, and, although some cases probably occurred of which there was no information, not a single instance was known of a child brought up there becoming afterwards a pauper. I must remark, however, that an admirable arrangement existed, and I trust still continues, with respect to the children in the Birmingham Asylum, which, so far as I am aware, is not to be found elsewhere, but to which no inconsiderable part of the foregoing most gratifying result is probably attributable. I allude to a regulation under which every child, during the whole time of its apprenticeship, after leaving the asylum, was visited once a year by an officer of the workhouse, who inquired on the spot into the child's conduct, and, when necessary, into the conduct also of the person to whom he had been bound apprentice, and who forthwith took any measure which the case might require."

He adds—

"If such results as these be compared with the subsequent history of children brought up in a bad and ill-organized workhouse, on leaving which they are hawked about with as much difficulty in putting into circulation almost as if they were so many bad coins, the contrast is most striking, and shows in a strong light how much not only the recipients of the good education are indebted to the real 'guardians' under whom they have been placed, but how much the district, as indeed the whole country, owes them; and, on the other hand, how great the injury done by the unfaithful stewards who neglect and mismanage the trust of the poor children with which they in turn are charged."

We trust we need not offer any apology for what we have thus brought under the notice of our readers. No persons, we believe, are more competent to judge of the necessity of reform in all that relates to the treatment of the indigent class than medical men, or more willing to aid in the good cause. The neglect of this class as regards consequences is little worse than that of an army. Why should not their dwellings be looked after, and their sanitary state enforced, after the manner of a camp or barracks? Why should not the poor children of our workhouses be properly taught and kindly watched, after the manner of the Birmingham Asylum? Were this done throughout the kingdom, and were all able-bodied men in want of work, applying for relief, received into our workhouses, given appropriate employment, and kindly and liberally treated, how great and manifold would be the good. The humanity of our government is displayed in the ample provision made for the insane paupers and for prisoners under confinement: are not the poor, who are not insane nor yet criminals, as deserving of its consideration?

Review VII.

1. *Military, Medical, and Surgical Essays, prepared for the United States Sanitary Commission.* Edited by William A. Hammond, M.D., Surgeon-General U.S. Army, &c. *Philadelphia,* J. B. Lippincalt & Co., 1864. 8vo. pp. 552.

2. *A Treatise on Military Surgery and Hygiène.* By Frank Hastings Hamilton, M.D., late Lieutenant-Colonel, Medical Inspector U.S.A., Professor of Military Surgery and Hygiène in Bellevue Medical College, &c., &c. *New York,* Bailliere Brothers, 1865. 8vo, pp. 648.

3. *A Manual of Military Surgery, for the use of Surgeons in the Confederate States Army.* By J. Julian Chisolm, M.D., Professor of Surgery in the Medical College of South Carolina, Surgeon in the C.S.A., &c. *Columbia,* Evans & Cogswell, 1864. pp. 529.

These books all come to us from American sources. One is published at Philadelphia; another at New York; and the third at Columbia. The two former emanate from the "Northern States;" the latter from the "Southern States." The two former were intended for the use of the Federal Armies, the latter is addressed to the surgeons of the "Confederate Army." But the "Southern States" are once more restored to the Union; the "Confederate Army" exists no longer; the war is at an end; we hear now of nothing but the triumphs of peace; and we are glad to regard the volumes before us as an instalment of the benefit which we may hope will accrue to medical and surgical science from the terrible struggle which has been raging in the United States. For four years a war of gigantic proportions has been carried on, under peculiar and somewhat novel conditions. The number of troops which have been brought into the field has been quite unprecedented; they have been moved over an area which presented the most various conditions of soil and climate; negroes have, for the first time, been embodied into regiments, and disciplined as regular troops; while both sides have been furnished with medical officers capable of observing and reporting the matters of scientific interest which passed under their care. Such a war is without a parallel in history; certainly nothing at all approaching to it has taken place since the revival of learning, and since the attention of our profession has been called to those questions of hygiène which regulate the health and physical condition of large bodies of troops. An opportunity has here been afforded of making observations upon a large scale, and of drawing conclusions which can hardly fail to advance medical science.

These works have all arisen directly out of the circumstances of the war. The first consists of a series of essays which were published by the United States Sanitary Commission for circulation among the medical officers of the Northern army. The second is a new edition of Dr. Hamilton's Treatise on Military Surgery, which has been almost entirely re-written in consequence of the added experience which the late war has supplied, while the third is a Manual of Military Surgery, which was compiled at the commencement of the war for the use of surgeons in the Confederate army. When the South found that her ports were blockaded and that her intercourse with Europe and with the North was alike interrupted, she was obliged to depend upon her own literature. But her own literature was scanty and did not meet her wants. Accordingly Dr. Chisolm undertook the task of supplying the surgeons of the "Confederate States Army" with a manual of military surgery and hygiène, which should serve as a text-book for those whose experience had hitherto been confined to civil practice.

The essays of the United States Commission, one of which, viz., that on the control and prevention of Infectious Diseases, we have noticed at page 343, well represent the state of our knowledge on certain subjects at the time the war broke out. They were written chiefly during the earlier part of the struggle, when there had not yet been time to collate and digest the observations and the experience of the military surgeons. We cannot, therefore, expect to find much that is new in this volume; and much the same may be said of Dr. Chisolm's Manual. It was compiled—probably in haste and under difficulties—at the outbreak of the war. Since then it has gone through several editions; but it is only here and there that we find reference made to the experience afforded by the war. It is, therefore, only in the second of these three works—in Dr. Hamilton's surgery—that we can expect to find any additions to our knowledge. Here we may look for some of the scientific results of the war, and in due time we shall hope to receive from the Medical Bureau at Washington a full and statistical account of all matters relating to the sanitary conditions, health, and mortality of the troops who were engaged in this greatest of wars.

The United States Sanitary Commission has been noticed on more than one occasion in the pages of this Review, and its origin and functions are well described by Dr. Hammond in the preface to these essays. We need not, therefore, enter into any lengthy explanations of its objects, or of the means by which it endeavoured to carry them out. Suffice it to say that it was an association formed in the Northern States and supported by voluntary contributions with the view of supplementing the medical department of the army. While it was quite independent of State control or military authority, it co-operated with the medical officers, and helped them to carry out their mission of mercy more promptly and more efficiently than they could otherwise have done. On many occasions the Commission was the means of conferring inestimable benefits on the sick and wounded soldiers of the Northern armies in a variety of ways which the Medical Department could not accomplish.

> "A body of men, representing the loyal and charitable people of the country, with ample means, fettered by no military restraints, and combining in themselves to a certain extent the functions of the medical, the quartermaster's, and the subsistence departments, act with a degree of promptness and thoroughness which can never be attained under the ordinary military organisation."

When the war broke out the demand for surgeons was necessarily very great, and many men entered the army whose previous experience had been confined to civil practice, or who had very little experience of any kind. Under these circumstances it appeared to the Commissioners that it would be extremely useful to publish a series of essays which should deal in a short, plain, and practical way with the most important questions of military hygiène and the diseases to which soldiers are most exposed. Such a series of essays was accordingly issued during the early part of the war and gratuitously circulated among the medical officers of the army. These essays have now been collected, edited by Dr. Hammond, and republished, forming the first of the three works which are named at the head of this article.

The volume contains seventeen essays, which are judiciously arranged under three heads—Military Hygiène, Military Medicine, and Military Surgery. The first group contains papers on "Military Hygiène and Therapeutics," "The control and prevention of infectious diseases," "Quinine as a prophylactic," "Vaccination in armies," and "Rules for preserving the health of the soldier." The second group embraces "Scurvy," "Miasmatic fevers," "Continued fevers," "Yellow fever," "Pneumonia," and "Dysentery." The third group includes the use of "Anæsthetics," "Hemorrhage from wounds," the "Treatment of Fractures," "Amputations," the "Excision of joints," and "Venereal diseases." Each subject has been entrusted to a gentleman who was presumed to be intimately acquainted with it, and fourteen surgeons and physicians have contributed their learning and experience to this cyclopædia of military medicine.

Some of the essays which relate to hygiène and the prevention of disease among soldiers, might be studied with advantage by the military as well as by the medical officers of the army. Gen. Scott is reported to have said that a man who could not make good bread was not fit to be the captain of a company. This is a strong way of stating the case, but it embodies a great deal of common sense. Every officer, who has been a few years in the service, ought to know something about military hygiène, and the sanitary arrangements of camps. Certainly the quartermaster and his subordinates ought not to be wholly dependent upon the advice of the medical officers in selecting a camping ground and placing the tents. Yet these are points of great importance in upholding the efficiency of an army, and upon them may depend the lives of thousands. Where 86,000 men have to be encamped upon a square mile, there must be the utmost need of strict attention to every principle of hygiène and every law of health. It is only by adopting timely and effectual sanitary measures, and by anticipating every controllable source of infection and disease that vast armies can be spared the sweeping pests of typhus, camp dysentery, and hospital gangrene; and protected from the scourges of scurvy, typhoid fever and cholera—diseases which in European armies have proved far more destructive than all the projectiles and sabres of the enemy.

Looking at these essays as a whole, we must say that we have been somewhat disappointed by them. A few are good—for example, those on Yellow Fever, on Military Hygiene, Control of Infectious Diseases, Preserving the Soldier's Health, and on Dysentery: one, the paper on Venereal Diseases by Dr. Bumstead, appears to us excellent; while the two short essays by Dr. Valentine Mott are interesting on account of the general learning which they shew, and the chivalrous spirit displayed by their venerable author. With these exceptions the papers hardly seem to fulfil the conditions required. We expected short clear descriptions of the various diseases and injuries, with a few good formulæ, and a variety of hints as to the use of household remedies and rough-and-ready appliances such as might be found useful in the exigencies of camp life. Instead of that we find that the authors are too often diffuse, entering into discussions upon points of secondary importance instead of stating shortly and clearly the principles which ought to regulate the practice of the inexperienced surgeon, and laying down rules for his guidance and direction. Indeed one can hardly help asking how it happens that men who were fit to be appointed Assistant-Surgeons in the United States army required such a book as this at all? If the young men who entered the army at the commencement of the war required to be informed on the use of quinine—the pathology of pneumonia, or the treatment of fractures, were there no text-books of medicine and surgery current in America which would have given them all this information, and a great deal more besides, in less space than this volume occupies?

The principles which have guided the Commission in selecting the subjects of these essays are not quite apparent to us, and we are inclined to think that some subjects have been unwisely omitted. If pneumonia is to form the subject of a paper, why are not pleurisy and acute rheumatism to receive a like share of attention? Surely the latter diseases are nearly, if not quite, as common in military hospitals as the former, and yet they find no place in the volume before us.

We have already said that the essay on venereal diseases is, in our judgment excellent. It is written by Dr. Bumstead, who is well known as one of the best authorities of the present day on the subject, and whose work on 'The Pathology and Treatment of Venereal Diseases' was favorably noticed in this Review a few years ago. The views which he advocates are those which belong to the most modern school of pathology, and in the short compass of twenty pages he lays down clearly the opinions which he holds with regard to gonorrhœa and syphilis, and gives plain rules and formulæ for their treatment. If all the contributors to this volume had formed as correct an idea of what was wanted as Dr. Bumstead has done, the object of the Sanitary Commission would have been more efficiently carried out.

Before we take leave of this volume we shall offer our readers a few extracts, culled from different essays and relating to various subjects of interest.

Several examples are given by Dr. van Buren of the use of quinine as a prophylactic and preventive of malarious diseases, of which the following is one—

"An overseer agreed to take charge of several rice plantations in one of the sickliest regions of rice culture, undertaking to spend the summer months on one of the plantations. He made no inquiry as to the health of the one chosen as his residence; it was selected from its convenient locality. When warned of the dangers of his residing there in summer, he said he would never have the fever: his confidence in his capacity to resist malarious disease seemed unlimited. The result fully justified his confidence. He lived ten years or more in that neighbourhood, spending every summer on the plantation, varied only by an occasional visit to the healthy pine-land, where his family resided during the summer. He visited his rice fields without hesitation, at any hour, day or night, that his business required. He never had an attack of fever during that time. I saw him after he had been there several years; a finer specimen of robust health it would have been difficult to find.

"It was ascertained, on inquiry, that it was his habit to take quinine daily during the summer, before leaving his house; the quantity he did not know, for he never weighed it. His entire and complete confidence in his ability to resist fever in so malarious a region is strong evidence that he had been in the habit of using quinine, and was well satisfied of its prophylactic virtues." (P. 100.)

Glycerine is now used in a great variety of ways both by surgeons and physicians, but its value as a means of preserving vaccine lymph has only recently been discovered, and deserves to be generally known. The following account of it is given in the paper on "Vaccination in Armies," and the writer is Dr. Collins—

"Having experienced much trouble, particularly of late, in keeping a reliable supply of vaccine virus for public vaccinations, I was glad to meet with any suggestions which would aid me in accomplishing this desirable object. I immediately made some experiments, which have convinced me that, by the use of glycerine, we can probably preserve vaccine virus for a great length of time, and that when we desire it for more immediate use this liquid is by far the best solvent for the solid matter that we possess. It saves us both time and trouble, and enables us to use the matter with much greater economy, which is of importance when our stock happens to be small. * * In my first experiment I pulverised about one-eighth of an ordinary scab on a glass plate, and moistened it with a small drop of glycerine. It is better that the matter be pulverised, as it otherwise dissolves very slowly. The quantity thus prepared served for my vaccinations for several days, amounting in all to twenty-four, among which there were about two failures—a success which I have rarely attained when using water as a solvent."

The writer then details various other experiments which he has made with the glycerine, and concludes by saying—

"If I am correct in the foregoing conclusions, which a little time will determine, the preservation of vaccine virus, and the distribution of it when desired to distant sections of the country, will become an exceedingly simple and easy affair. A single scab, prepared as above, would be sufficient to fill some fifty tubes, each of which would be capable of vaccinating ten or more persons." (P. 154.)

The following extract from Dr. Metcalf's paper on "Miasmatic Fevers," wherein the writer laments the present distrust of drugs in general, as compared with the confidence which all medical men have in the use of quinine, will be read with approval by many practitioners on this side of the Atlantic—

"Amid the manifold uncertainties of medical science, and the perpetual contingencies of medical art; amid the disheartening scientific infidelity which has lately been taking possession of the medical mind, shaking to its deep foundations the firm old faith in the potency of drugs, and threatening to overturn and demolish it altogether, it is gratifying and consolatory to feel and to know that, here at least, we stand upon firm ground—that here we may hold that there is one great and important therapeutical relationship definitely and positively ascertained and established, defying alike the open assaults of quackery from without, and the treacherous machinations of indolent scepticism from within." (P. 221.)

Dr. Valentine Mott's paper on "Hæmorrhage from Wounds" contains the following narrative. It is given to show how necessary it is for all soldiers to know how to apply a tourniquet and to stop bleeding—

"At Antietam two young brothers stood side by side in the ranks of our army, and together bravely fell, the one pierced by a Minié ball through the lungs, and the other wounded in the thigh by a fragment of a shell. He who was shot through the lungs still survives to reveal the horrors of the ensuing night, and to relate the story of the tragic death of his brother, who was wounded in the thigh, by repeated hæmorrhages from the femoral artery, which the two together were unable to control. When the survivor recounted to me the experiences of that fatal night, and told me how his brother bled and fainted, and rallied and bled, and fainted and rallied again, his bosom overflowed with grief, and his eyes became suffused with tears. But his bitterest sorrow was because he knew that, with the proper means, he might have arrested the hæmorrhage, and thus given his brother a chance for his life." (P. 407.)

The way in which this volume of essays is got up deserves a word of commendation. The illustrations, the printing, and the binding are all excellent, and reflect no small credit on the publisher.

We turn now to Dr. Hamilton's treatise on Military Surgery, which is by far the most important of the books under consideration.

The essays of the Sanitary Commission were written for a particular purpose, and that purpose has now been served. As a whole, we shall probably hear no more about them, but Dr. Hamilton's Surgery is likely to be a text-book for many years to come in the hands of the American army surgeons.

It is, as we have already said, a new edition of the author's work on military surgery; but in consequence of the increased experience which the war has afforded, it has been almost entirely rewritten. It abounds with allusions to the late war, and may almost be regarded as a chapter in its medical history. As far as Dr. Hamilton's experience went, he has embodied the results of it in the present edition of his work; and he has endeavoured to tell us, from his own observations, what was the physical condition of a part of the Northern army.

His book consists of two parts. In the first he treats of the examination of recruits, tent and barrack accommodation, the construction of hospitals, and the general hygiènic management of troops. The second part relates to the treatment of gunshot injuries and the diseases which are most common in military hospitals.

The work does not profess to be a complete system of surgery; it presupposes a knowledge of general surgery, and then aims at supplying all that special information which it is necessary for an army surgeon to possess. The volume contains many interesting details drawn from the late war, and many hints are thrown out which can hardly fail to be of use to medical officers in the emergencies of warfare. The practical suggestions which we look for in vain in the essays of the Sanitary Commission are scattered abundantly throughout the pages of Dr. Hamilton's treatise.

It would appear that the status of army surgeons in America is in much the same unsatisfactory condition as it is among ourselves. There is an Act of Congress of February 11, 1847, which declares that "the rank of the officers of the medical department of the army shall be arranged upon the same basis which at present determines the amount of their pay and emoluments: provided, that medical officers shall not, in virtue of such rank, be entitled to command in the line or other staff departments of the army." This Act gave the army surgeons all they could desire. It was intended to relieve them from that position of subordination in which they had so long been subject to petty annoyances, and even to the insults of inferior officers of the line, and to secure for them those courtesies and that respect which they had a right to claim, while it gave no authority to command or to trench in any way on the special duties of the military officers. But, notwithstanding the satisfactory nature of this Act, the army surgeons do not always enjoy the position to which it entitles them; for it appears that there are a few officers of the army and navy who habitually refuse to acknowledge the Act, and persist in violating both its letter and spirit. But this state of things, annoying as it must be, cannot be permanent. With an Act of Congress to back them, the medical officers can hardly fail to obtain the victory in the end. If only they claim their rights with dignity and firmness, they must at last attain that position which the law assigns them.

The opinion which Dr. Hamilton expresses on the indiscriminate use of stimulants in the army deserves attention. If we mistake not, a somewhat similar opinion was given by some as the result of their observations during the Crimean War; and those who discarded the use of stimulants altogether found that they were better able to bear the severity of cold in the trenches than those who took their rations of spirits regularly. Dr. Hamilton says—

"In our own mind the conviction is established by the experience and observations of a life, that the regular routine employment of alcoholic stimulants by men in health is never, under any circumstances, useful. We make no exceptions in favour of cold or heat, of rain, or, indeed, in favour of old drinkers, when we consider them as soldiers. Men who have been hard drinkers, when first enrolled, and deprived wholly of their stimulants, sometimes become exhausted and die, or have to be discharged; but such examples are rare, while, on the other hand, most of these men soon improve in their general condition of health, and not a few are permanently cured of their habits of intemperance." (P. 75.)

This is a strong opinion, but it has evidently been well weighed by our author, for it is supported by the testimony of others; both plans—the plan of giving spirits and the plan of withholding them—were tried in the Northern armies. The subject is one which involves the whole question of the value of stimulants, and that is a field of controversy upon which we cannot here enter; but we commend to all military surgeons the whole of this chapter upon the general hygiène of troops, for it deals with a most important subject—the food and drink of armies; and we all know how much truth there is in the French proverb—"C'est la soupe qui fait le soldat."

Next in importance to the feeding of the soldier is the question of his clothing and personal cleanliness. Dr. Hamilton dwells at length on this subject, and gives a graphic sketch of the evils arising from uncleanliness—

"When no attention is paid to habits of personal cleanliness—when garbage lies everywhere in the company streets, and the air has a noisome odour both within and without the tents, we have found the men slovenly in their habits of dress, negligent of duty, but particularly attentive to sick call: their muskets are rusty and out of order; their knapsacks are badly packed; they are improvident of their rations, and their cooking is badly done; they fall into line slowly and straggle on the march: they are insubordinate, mutinous, without drill, and without discipline. They have no *esprit de corps*—no self-respect—no manliness—no courage; and they will not fight. These are the links which compose the chain; with defilement at one end and cowardice at the other, commencing in the camp at Alexandria, and ending in the rout upon the Plains of Manassas." (P. 92.)

The various kinds of tents and huts are described at considerable length, and their respective merits discussed. The author mentions that on several occasions the Southern troops were found sheltered in remarkably comfortable huts, resembling precisely those which are seen everywhere in the Slave States, and which are used as quarters for the negroes. They were, in fact, chiefly constructed by negroes, who were impressed into the service of the Confederate army, and in this way, among many others, the negroes must have proved a great element of strength to the South, and enabled it to prolong the contest.

In speaking of the value of tents for field hospitals, as compared with buildings of any kind, our author says—

"One single fact which we shall state ought to settle for ever the value of tents for hospital purposes. While we have seen many hundreds of cases of hospital gangrene which have originated in buildings temporarily occupied, in transports, and even in well-constructed pavilions, we have never seen a case which originated in a tent; nor can we call to mind a case which was not at once benefited, if not speedily cured, by a transfer to a tent. Upon this point the testimony of all army surgeons with whom we have conversed is the same.

"At the instance of the author, seconded by several other medical gentlemen, the enlightened and liberal Commission of Public Charities of the City of New York have recently established upon Blackwell's Island, in this city, a fever ward, composed entirely of hospital tents. We shall now be able to ascertain how much pure air alone can accomplish in typhus and typhoid fevers." (P. 134.)

The conveyance of the sick and wounded forms the subject of a very interesting chapter, and a variety of litters and ambulances—including even the "railway ambulance"—are described and figured.

Passing on to the second part of Dr. Hamilton's work, that in which he deals with the more purely surgical part of his subject, we observe with pleasure that he devotes a considerable space to discussing the value of water, in its various degrees of temperature, as a dressing for wounds. The method of dressing wounds has been becoming gradually more and more simple; and now that it has been reduced to one word, *water*, we are beginning to find out what powerful effects may be brought about by the use of plain water in its various degrees of temperature.

The following is a good example of the terrible hardships to which the wounded are sometimes exposed after a battle, and of the unfavorable conditions in which the surgeon may find his patients placed—

"Hernia of the lungs is a very rare accident, especially as the result of gunshot injuries. We have met with it once in the person of a soldier wounded at the battle of Fair Oaks. Our attention was called to him the night after the second battle by one of the surgeons. He had been wounded by a ball on the left side of the thorax, a little below the nipple. The ball had not been found. He was lying upon the ground, in a condition of considerable prostration. The hernia was about one inch in diameter, having escaped from an aperture which was very much smaller. It was completely strangulated, being quite black and insensible to the touch. We applied to the neck of the hernia a strong silk ligature, for the purpose of expediting its destruction, and then made fast the ends of the ligature to the outer surface of the chest by adhesive plasters, to prevent the escape of the ligature within the cavity, in case the hernia should retire after it had sloughed. We saw this poor fellow the next morning, lying in the same place. He had taken a little nourishment, such as we had to give him, and expressed himself as being comfortable, although he had lain without shelter two nights, and during each night he had been drenched with rain. In this respect, however, he suffered only in common with at least two thousand other wounded and dying men. We cannot omit this additional tribute to the bravery of these noble fellows. During all this time—and we were with them every moment, both night and day—there was never heard one cry of impatience or one murmur of complaint, beyond that which was extorted by the agony of suffering." (P. 295.)

The value of anæsthetics is discussed at considerable length, and opinions are brought together from various quarters tending to show that their use ought to be somewhat less general in military than in civil practice. When a soldier in the excitement of battle receives a severe injury he becomes no doubt very much depressed, and if an operation is necessary it may be well to withhold chloroform, which is confessedly a depressing agent. Herein consists the whole case as stated by our author. But it may be replied that the dread of pain is deeply rooted in us all; and the power of anæsthetics to suspend sensibility is so well known to the public, that it is a question whether men could be persuaded to undergo an operation at all if they are not to be allowed to take chloroform. We have, therefore, a choice of evils. On the one hand, there is the repugnance to incur pain, and on the other there is the depressing influence of the anæsthetic. In this dilemma we must be guided partly by the nature of the case and partly by the fortitude of the patient. And after all, it will generally be found necessary to give chloroform to satisfy the patient, though in such a case it should be given only to a slight extent, and its effects very carefully watched.

Dr. Hamilton is decidedly of opinion that capital operations are not so successful now as they were before the introduction of anæsthetics. He holds that they produce certain effects upon the system which tend to prevent union by the first intention, and consequently they must be regarded as, indirectly, causes of suppuration, pyæmia, secondary hæmorrhage, erysipelas, and hospital gangrene. This is an important subject for observation and inquiry. The opinion of a surgeon like Dr. Hamilton deserves the most careful consideration; but we believe it is here opposed to the doctrines which are current among us. The whole question of anæsthetics has received special attention of late in this country, and the same appears to be the case in America. We may therefore hope that our knowledge of the subject will be increased—that we shall be better able to guard against accidents—that the risk will be reduced to a minimum —and that the use of anæsthetics will become an unmixed good.

Under the head of "Exsections," some interesting cases are given. Here is a successful case of excision of the knee-joint for a gunshot wound. The author believes this to be the only successful case of the kind on record. The operation was performed by R. B. Bontecue, Surgeon, U. S. V.—

"A man, aged twenty, was wounded in the right knee, October 22nd, 1862. The ball passed through a portion of the external condyle of the femur, and lodged in the popliteal space. On the second day after the injury, Dr. Bontecue removed one and a-half inches of the lower end of the femur with a saw, and with a pair of bone forceps cut away the articular surface of the tibia, until the bone was exposed. The patella was dissected out. A portion of the wound was healed by the first intention. On the twenty-eighth day the wound had entirely closed; and at the end of a little more than two months from the date of the operation, the patient was discharged. There was then no pain or tenderness about the joint." (P. 514.)

In the last number of this Review we inserted a paper by Mr. Campbell de Morgan on the use of a strong solution of chloride of zinc as a dressing for suppurating wounds. Mr. de Morgan is of opinion that by this means we shall be able to control suppuration and to reduce it almost to *nil*. If such should prove to be the case, what a valuable addition it will be to our resources! In reading the accounts of those ghastly wounds which are inflicted by modern projectiles, and of the formidable operations which are so common in military practice, one cannot help reflecting that if we had an agent which could prevent and limit suppuration, it might be the means of saving innumerable lives.

Dr. Hamilton's work closes with an Appendix, which contains, among other things, an estimate of the value of coloured troops. An extract on this subject will probably interest our readers—

"In the performance of our duty as Medical Inspectors, U.S.A., we had occasion to visit the camp of a newly recruited regiment of coloured troops, stationed at Estell Springs, Tenn. The regiment numbered over one thousand men. We found the police as complete as in any of the older regiments, and the surgeon informed us that the sick reports did not exceed eight or ten daily. We have noticed the same attention to police in other smaller detachments on the field and in their hospital wards whenever we have visited them.

"We have been informed by all the officers with whom we have conversed that they readily acquire the rudiments of the art of war, such as the manual of arms, and the drill. They are remarkably subordinate, and submit cheerfully to discipline. Intemperance is comparatively a rare vice among them. In all these respects they compare favorably with white troops.

"In relation to their endurance in long marches, the testimony is at present insufficient to enable us to form an opinion. They have not, as a general rule, been subjected to very severe tests in this respect, most of them being stationed permanently at posts in various parts of the country. They are also mostly new troops, and for this reason they have not had the training, nor has it been thought advisable to subject them to severe campaigning service. Some of the writers, however, who have attended to this subject, declare that they do not straggle as much as other troops, and possibly because, in the cases referred to, they entertained a greater dread of falling into the hands of the enemy. In the trenches, and as laborers in the construction of roads and bridges, in the felling of trees, and in the performance of all kinds of fatigue duty, they have generally exceeded expectation." (P. 634.)

Among the coloured troops the mortality arising from sickness appears to have been very great; while, on the other hand, the testimony goes to prove that the negro recovers from a wound more kindly and with less danger than a white man.

We have noted many other points of interest which we should have been glad to have brought before our readers, but to mention them all in detail would carry us to too great length. A variety of methods of swinging limbs are explained and figured, some of which seem to be particularly suitable to military practice. The chapter on arrow wounds is interesting, both for its own sake and because the subject is one which does not generally find a place in modern books on surgery. These are two of the points to which we should have liked to have drawn the attention of our readers more fully, but it is time that we should turn to the third book on our list. We will only add that we have read Dr. Hamilton's work with much pleasure, and we believe it will form a valuable addition to the literature of military surgery. We feel sure that our army surgeons will find it an interesting and profitable study.

Dr. Chisolm's Manual differs notably in its external characters from the two books that we have just been considering. They are well printed on the best of paper, illustrated with excellent woodcuts, and handsomely and substantially bound. They have evidently been printed and published under the most favorable circumstances, with all the resources of art at command. But it must have been far otherwise with Dr. Chisolm's Manual. The way in which it is printed, the quality of the paper, the style of the illustrations, and the manner in which it is bound, all testify to the straitened circumstances in which the Southern States were placed at the time of its publication. But if the external features of the book betoken the difficulties under which it was produced, the letter-press is drawn up in a way which is quite characteristic of the energy and self-devotion which was shown by the South. When the work was first undertaken, the war had already broken out, the Southern ports were blockaded, and intercourse with Europe was cut off. Dr. Chisolm must therefore have had a laborious and difficult task in preparing his first edition; and the industry which he has since shown in collating the experience of the war and embodying it, to some extent, in the present edition deserves the highest praise.

Dr. Chisolm, first of all, discusses briefly the questions of military hygiène; he then enters upon the consideration of military surgery, and this he does at some length. The volume ends with a closely-printed Appendix, wherein the various operations are explained and illustrated by a series of plates. The author's style is easy and flowing, one reads his observations with pleasure; and the descriptions of disease are graphic and interspersed with well-selected cases, which add much to the interest. We shall lay before our readers a few extracts, from which they will be able to judge for themselves of the merits of the work.

Here is a good example of the curious course which a bullet will sometimes follow, and of the importance of ascertaining the position in which the patient was at the time he received the injury—

"A case in point was that of Private R——, 7th S.C.V. Regiment who was shot in the neck at the battle of Malvern Hill, June 30th

1862. His wound was considered trivial, and a furlough of thirty days was granted. He came under my observation a few days after the reception of the injury with the neck very much swollen, and a severe pharyngitis, with tonsilar enlargement, seriously obstructing respiration and deglutition. The swelling on the back of the neck caused him to carry the chin touching the sternum. A large orifice, from apparently a minié ball, existed on the left side of the neck, one and a-half inches from, and on a level with, the spine of the sixth cervical vertebra. When the wound was probed it was found to traverse the neck, running over without fracturing the spine of the cervical vertebra, and then to change its course obliquely downward and outward. Profuse suppuration soon came on, the pus burrowing under the right scapula, caused, as was supposed, by some foreign body, probably the ball, as there was but one orifice to the wound. After some days of treatment an opening was made on the right side of the back, above the upper edge of the scapula, and the neighbourhood thoroughly explored. The subscapular region was found undermined, and the neck of the scapula fractured, but no foreign body could be discovered after the most careful search. Suppuration continued profuse for weeks, reducing the patient to the last extremity, with extreme emaciation. He finally rallied, thanks to a good constitution and good nursing, and was at last sent home convalescent. In time an abscess formed in the immediate vicinity of the elbow joint, and a large minié ball was extracted from this situation. When he received this wound he was loading his rifle, and was in the act of biting the cartridge, with arm raised and face depressed. With this history of the case, the position of the ball could be readily accounted for." (P. 176.)

In speaking of the practice of tying both ends of a bleeding vessel at the seat of injury, Dr. Chisolm mentions the following case—

"A very interesting case in point was that of Private B. Creesy, 42nd Virginia Regiment, who was wounded on the 3rd of May, 1863, by a minié ball, which, in its passage through the larynx, above the vocal cords, carried away the epiglottis. On the 12th, nine days after the receipt of injury, while under treatment in the Winder hospital, a severe secondary hæmorrhage came on, to control which the left common carotid artery was ligated. The hæmorrhage ceased, to reappear on the 18th, when the right common carotid artery was ligated, with the same effect of stopping the bleeding. The patient lived thirty-six hours after the second operation. An autopsy revealed the fact that the left hyoid artery was the injured one, and the inference is that a ligature to the artery at the seat of injury might have given a much more successful result, and obviated two very serious operations." (P. 206.)

The two following cases, which are given in the chapter on wounds of the chest, will be read with interest—

"A case in point was reported to the Association of Army Surgeons at their meeting in February, 1864, by Surgeon Thorn, as communicated to him by Surgeons Selden and Moore. A patient, of scrofulous habit, twenty-two years of age, was leaning on his gun, the muzzle in contact with his left side, when it exploded, tearing a hole in the chest of three or four inches in diameter, carrying with the load of shot fragments of the third, fourth, and fifth ribs, and the whole of a very heavy English gold patent lever watch, except the ring to which the chain was attached, which, singular to say, was found in the lining of his waistcoat, on the right side. Dr. Selden found the patient apparently about to expire, and, from the impending suffocation upon the ingress of air within so large an opening, he could make no exploration of the wound. Closing the wound with a large compress and bandage, opium and stimulants were frequently administered. Reaction took place, and in a fortnight sufficient adhesions were established to permit exposure of the cavity of the wound, and to recognise and to remove the metal face of the watch, from some six inches at the bottom of the wound. For several weeks fragments of the watch continued to present themselves and were extracted—some from the diaphragm, others below the clavicle. The lung collapsing was not torn to pieces, though wounded in several points. Both the heart, covered by the pericardium, and the aorta were exposed to view and to touch. Suppuration was enormous—hæmorrhages frequent. The collapsed lung became bound down by adhesions; the whole side of the thorax sunk. Sustained by every article of nutritious food calculated to supply an inordinate appetite, the patient's recovery was slow, until the wound, progressively reduced, could only admit a female catheter. Fragments of the watch and bone, together with shot and other extraneous matters, continued for some time to be ejected by expectoration, with sputa. The patient now possesses every part of the watch except the hands, a considerable portion of the smaller works having been expectorated. The openings into the lung were of a sufficient size to allow a current of air to escape, and, if directed against the flame of a candle, to extinguish it. The patient's health continues feeble, but is as robust as it had been during the past five years." (P. 323.)

The second case which we shall extract from this chapter—the last which we shall lay before our readers—is as follows—

"Should the rapid accumulation of blood in the cavity of the chest cause serious dyspnœa, the orifice may require opening to allow the fluid to escape, and thereby relieve the pressure upon the lung. The effect of this escape of blood from the cavity of the chest was exemplified in the case of Major Wheat, who was shot through the chest at the first battle of Manassas, the ball entering in at one arm-pit and escaping from the other, on a level with the nipple. Soon hæmorrhage caused great oppression, and, finally, fainting. When he partially recovered his consciousness he found himself surrounded by his men, who, believing him dead, had stripped his body of every vestige of rank, so as to prevent recognition by the enemy. One of his men (a powerful sergeant) determined to save the body from indignities, had seized the major's arms at the wrists, and, with the assistance of a comrade, had slung the body over his back, drawing the arms of the supposed dead man over each shoulder, and in this position started off from the battle-field. Major Wheat was himself a powerful man, and his weight, in addition to his chest being drawn forcibly against the broad back of his sergeant, so increased the pressure upon his lungs, as nearly to extinguish the flickering spark of remaining life, when he suddenly felt a gush of blood and air from both arm-pits, followed by such immediate relief that he found his breath returning, and when he reached the ambulance wagon he could stand up. Arriving at the hospital, he found that he had so far recovered, under this rough treatment, that he could walk with assistance. Quiet, with but little medication, soon completed the cure, and in course of time enabled the major to resume his command." (P. 325.)

Dr. Chisolm's Manual was written, as we have said, for the special use of surgeons in the "Confederate States Army." But the "Confederate States" exist no longer—their army is broken up—their military surgeons are dispersed, and probably most of them have returned to civil practice. But though the body for whose benefit Dr. Chisolm wrote has passed away, we hope that his Manual will continue to flourish. It is too good a book to lapse into oblivion; and if the author would endeavour, in another edition, to record as fully as possible the experience of the medical staff of the "Confederate States Army," his work would probably become one of great interest and importance in the history of military surgery.

REVIEW VIII.

On some of the Causes and Effects of Valvular Disease of the Heart: being the Croonian Lectures of the Royal College of Physicians for 1865. By THOMAS B. PEACOCK, M.D., F.R.C.P., &c., &c. *London:* Churchill and Sons. 1865.

DR. PEACOCK's contributions to professional literature are always characterised by industry in the collection of facts, the exercise of careful observation in examining and arranging them, and considerable sagacity in their interpretation. In the department of cardiac pathology he has already proved himself a successful labourer, having investigated the subject of malformations and congenital defects of the heart with more diligence and completeness than any other writer with whom we are acquainted. His present work bears the same characteristics of independent observation and sagacious inference with those on which his reputation is already securely based. In it he directs attention to groups of valvular disease which exist or are produced independently of inflammation or morbid diathesis during life; diseased conditions whose causes have been frequently mistaken or overlooked, but which have risen in importance with the more careful clinical observations and pathological investigations of the present day. We think it scarcely fair to assert that the great attention which has been paid by cardiac pathologists to the effects of the various kinds of inflammation on the valves of the heart has led them to overlook or neglect other modes of origination of valvular disease. Up to a recent period eight tenths of pathological science were comprised in the doctrine of inflammation and its results. It was therefore to be expected that the first advances in our knowledge of cardiac pathology should be made in this direction. Neither is the value of the results obtained by prior workers diminished by a recognition of the existence of other causes of valvular obstruction and insufficiency besides endocarditis. The latter is still the most frequent source of these conditions, whilst to the practical physician it is certainly the most important, from the fact that it is to the greatest extent within the reach of his art.

Dr. Peacock classifies the causes of valvular disease or defect in four groups. The first comprises malformations of the arterial or auriculo-ventricular valves giving rise to regurgitation or obstruction, or obstruction and regurgitation. The second includes injuries of the arterial and auriculo-ventricular valves, either immediate or gradual, causing regurgitation with or without obstruction. The third comprehends alterations in the capacity of the cardiac orifices and cavities which permit regurgitation from erosion or maladjustment. The last group consists of acute and chronic inflammatory affections, chiefly rheumatic, which are the acknowledged causes of impediment or reflux, or of both. The last set of causes is not discussed in these lectures; the author, indeed, hazarding the opinion that they neither require nor admit of further elucidation. The three former afford the material which he has worked up into the very valuable work before us.

Congenital malformation of the aortic valves as a cause of disease in after-life first attracted the author's attention, and was discussed by him in the 'Edinburgh Monthly Journal' in 1853. Precise and reliable information as to the mode of formation and development of these valves is as yet wanting; but there is reason to believe that each segment is originally composed of two portions, which afterwards become blended together. If this be a true account of their mode of production, it is evident that an arrest of the process would result in the existence of supernumerary valves, which are not therefore produced by a redundancy in the formative process, but are simply expressions of arrested growth. Accordingly, excess in the number of valves, Dr. Peacock believes, always depends on the presence of supernumerary segments, which are only partially separated from some of the other segments. In this way four valves are comparatively not uncommon, five are sometimes found, and it is possible that there may be six, although the author states that he has never met with an instance of that number.

The semilunar valves may be diminished in number. In place of the three, there may be only two, or the whole apparatus may be represented by one imperfectly formed curtain. The author conceives that these conditions arise during uterine life, from membranous attachment taking place between the angles and contiguous sides of two or more valves. The segments are originally correctly formed, but adhesion takes place, and subsequently the adventitious tissue constituting the line of union becomes atrophied and disappears. The line of union remains marked by a membranous ridge on the upper or arterial side of the curtain, and by a corresponding shallow groove on the lower or ventricular surface. When the three segments are blended together so as to form a kind of diaphragm with a central aperture, stretched across the arterial orifice, the upper surface of the curtain will present three such ridges, and three grooves on the ventricular side will indicate the original division. The membrane, in such cases of complete fusion, becomes protruded forwards in the course of the artery so as to assume a funnel shape. This latter change is more marked in cases of blending of the valves at the pulmonic than at the aortic orifice, a fact which the author explains by reference to the greater power of the right ventricle during fœtal life, the membrane being then most extensible. This blending of two or more valves he believes to be the result of intra-uterine disease. The explanation which accounts for the presence of two valves only, the one larger than the other, by supposing that the angles of one of the valves have been torn down by accident, is untenable. Such injuries do occur, but they are very rare, and are attended by a serious train of symptoms; but the kind of malformation under consideration may be found in persons who have given no evidence of cardiac disease, and who are not known to have sustained injury. Union of these valves is also found in young children whose hearts present other deviations from natural development of undoubted intra-uterine origin. Thus, imperfection of the ventricular septum, or patency of the foramen ovale with a pervious ductus arteriosus, conditions indicating obstruction at the pulmonic orifice during fœtal life, are not unfrequently accompanied by blending of the valves of the pulmonary artery. The disease producing the valvular malformation is probably of an inflammatory character, for we know that both peri and endocarditis may take place during intra-uterine existence.

The first kind of malformation, the multiplication of valvular segments, does not necessarily interfere with function. Not so, however, the union of the segments; and here the author draws a distinction in the pathological effects produced between those cases in which two valves only are blended, and those in which the three are united into a single curtain with an aperture in the centre. The first condition, he finds, gives rise to incompetency, and more rarely to obstruction; the second produces obstruction, and more rarely incompetency. He thus explains the mode in which the former becomes the cause of regurgitation—

"It is evident that, when there are only two semilunar valves at either of the two arterial orifices, and one of the curtains is considerably larger than the other, the larger curtain, not being adequately supported in its middle, must have a tendency to become stretched, and to fall below the level of the other segments, so as incompletely to close the orifice during the diastole of the ventricle. Regurgitation must thus be permitted; and the regurgitant current once established, will have a tendency to turn back the edge of the valve so as to aggravate the evil. Not only, however, has the united curtain a tendency to yield in this way, but the portion at which the union has taken place, being generally thickened and indurated, is less extensible than the rest of the segment, and so does not adequately expand with the process of growth. The edge, therefore, is, as it were, held back in that situation, and, when the valves are closed, a space is left through which regurgitation takes place." (Pp. 6, 7.)

That complete union of the valves must produce obstruction, whilst the central aperture in the curtain will frequently permit regurgitation is self-evident. But the author observes the fact which has been noted by others, that remarkable malformation of this kind may exist for years without giving rise to marked symptoms of cardiac disease. The condition passes unnoticed until the ventricle, from progressive thickening and induration of the valves, becomes incapable of overcoming the obstruction or until, from other causes, its force is weakened.

This account of malformation of the aortic and pulmonary valves is supported and illustrated by reports of eight cases which have been observed by the author. They will well repay study, and are made more valuable by good woodcuts of the condition of the valves. From his observations, Dr. Peacock concludes that cases in which the whole of the three valves at either arterial orifice are blended together, are perhaps more common than those in which two segments only are joined, and that the aortic valves are more commonly the seat of malformation than the pulmonic, except where other deviations from the natural development of the organ coexist.

It is not a new observation that congenital malformation of the auriculo-ventricular valves may lay the foundation of after-disease. This fact was first pointed out by Burns, and has been confirmed by the authority of Laennec and Farre. Cases are met with where dyspnœa and palpitation on exertion have been observed from early childhood, without any history of the affections which ordinarily give rise to disease of the heart. Cardiac symptoms of early date may have only been aggravated by rheumatic attacks occurring in after-life. Examination shows that the curtains of the tricuspid valve have been blended together by membranous attachment, leaving only a triangular or oval opening in the centre. This condition may exist alone or be accompanied by a similar alteration of the mitral and aortic valves, or it may co-exist with malformation of the heart of evidently intra-uterine origin. It has not fallen to the author's lot to meet with a case in which the mitral valve alone was indisputably the subject of congenital malformation. That mitral disease does thus originate is rendered probable by the fact, "That the most complete cases of union of the valves which are met with occur almost always, if not always, in young persons; and sometimes in those who have never had rheumatic fever, and have been delicate and subject to cardiac symptoms from birth." The continuance of life for several years is scarcely compatible with the supposition that such extensive disease was of sudden origin. Moreover, it is well known that contraction or obliteration of the mitral and tricuspid orifices are occasionally found in children with malformed hearts, and the author refers to a case recorded by Dr. Mossman of Berlin, where recent endocarditis of the mitral and tricuspid valves was observed in a child which only survived twenty hours, and was detected during life. The author records three cases in which one or both sets of auriculo-ventricular valves were adherent and the apertures contracted. In one of them rheumatism had not occurred, in another cardiac symptoms which had existed from birth had been augmented by two attacks of rheumatism; in the third case the history was defective, but early delicacy had been aggravated by a rheumatic attack.

Atrophy of the valves depending upon deficiency in the fibrous basis of their structure, or the partial absence of membrane, is sometimes observed, most frequently "at the angles and sides of the aortic valves and in the attached curtain of the mitral." This deficiency, Dr. Peacock thinks, is generally of little importance unless there co-exist imperfect adjustment of the valves from dilatation of the cavities or orifices. Then it may, by permitting eversion of the edges of the valves, give rise to regurgitation.

Dr. Peacock's observations lead to the conviction that cases of valvular disease originating in malformation bear a much larger proportion to those arising from other causes than has hitherto been supposed. From his records of cases observed during life and examined after death, he finds that of twenty-six of aortic valvular disease, nine probably originated in malformation of the valves, and of seventeen of combined aortic and mitral valvular diseases, two probably so originated. Of the whole forty-three cases eleven, therefore, or 25·5 per cent. were of congenital origin, a larger proportion, he thinks, than would *à priori* be expected.

Life, however, is not necessarily cut short at an early period by the proclivity to disease which malformation affords. In the eleven cases of aortic valvular disease he assigns to a congenital cause, the ages of all the patients averaged 42·3 years, and the extremes of ages were eighteen and seventy-six; whilst in the cases referable to other causes the average age was 47·4 years, and the extremes of age were twenty-one and seventy-two.

Injuries of the cardiac valves from accident are treated of in the commencement of the second lecture. They may be classed into two groups. First, there are cases where apparently healthy valves give way—are torn—under sudden violent muscular exertion, or yield more slowly under long-continued straining or pressure. These are comparatively rare cases, but instances of the kind have been recorded by several observers, of whom Dr. Peacock mentions Corvisart, Marat, Bertin, and Legendre, Latham, Bence Jones, Quain, Wilks, and Spanton. He also records several cases which have fallen under his own notice. Then there are the commoner class of cases in which previously diseased valves give way on shock or during comparatively slight muscular effort. The first are interesting from their rarity. The aortic valves, as might be expected from the heavy pressure they sustain, are most frequently injured. But the other valves do not always escape. Of seventeen cases, the aortic valves were injured in five, and probably in five others; the columns of the mitral valves were ruptured in three cases and probably in one other, and the columns of the tricuspid gave way in three cases—

"The symptoms by which the occurrence of the injuries is indicated are generally very characteristic. The patients usually suddenly experience pain in the region of the heart, sometimes preceded by a sense of something having given way. The pain generally extends from the præcordia to the spine of the back, and to the shoulders and arms. Often it becomes very severe, and is attended by difficulty of breathing, oppression at the chest, palpitation, syncope, and sense of impending dissolution. When the aortic valves are injured, syncope appears to be the most marked symptom; while, in injuries of the mitral valve, the patients rather experience sense of oppression at the chest and of suffocation. Spitting of blood also occasionally occurs shortly after the accident, and in two instances of injury of the aortic valves, the patients perceived peculiar sounds extending up the chest and neck and in the ears. In some cases that were seen shortly after the injuries, the physical signs indicating the defect were perceived. The symptoms which have been mentioned appear generally to subside, in some degree, after a longer or shorter period; but the subjects of the injuries are never after wholly free from evidences of cardiac disturbance or capable of following any occupation requiring active muscular exertion, and usually they die in no long time." (Pp. 42, 43.)

We must now follow our author to the third class of causes of valvular disease of which he treats, viz., alterations in the capacity of the orifices and cavities of the heart. There is no difficulty in understanding the broad fact that the efficiency of the aortic valves may be injured by alterations in the capacity of the aortic orifice. But the careful study which Dr. Chevers has given that orifice has shown that the function of the valves may be interfered with in at least two ways. He has shown that the aortic orifice is a cylindrical canal, "bounded below by the fibrous zone into which the convexities of the semilunar valves are inserted, and above by the angles of attachment of the segments." The depth of this canal is about six lines. Its lower opening, in connection with the muscular structure of the ventricle, is the wider. Either the lower or upper opening may be dilated or contracted independently of the other part of the canal. Dilatation of either opening leads to regurgitation, but in a somewhat different manner—

"If the ventricular portion of the orifice be dilated, either alone or in conjunction with dilatation of the ventricle, or relatively from contraction of the arterial portion, the sacs or sinuses of the valves become expanded, from the pressure of the column of blood in the aorta falling more directly upon them; if, on the contrary, the outlet of the passage be dilated, or the angles of the valves become stretched, the curtains drop below their proper level, the sacs are rendered shallow, and the segments have their spaces of contact diminished. In the former case the tendency is to breaking down of the curtains at their most dependent parts; in the latter, to retroversion of their edges; in either case the apparatus becomes incompetent and allows of regurgitation from the aorta into the ventricle." (P. 49.)

Dr. Peacock relates the particulars of two cases illustrating the forms of defect described by Chevers.

Dilatation of the left auriculo-aperture is not necessarily a cause of mitral insufficiency, for the curtains of the valve may undergo expansion so as to occlude the enlarged orifice. It is only when this compensation does not take place or disease of the valves themselves is superadded that regurgitation results. Maladjustment of the auriculo-ventricular valves, from dilatation of the ventricle, is probably a more frequent cause of iusufficiency than dilatation of the apertures. In the case of the tricuspid valve, this cause of insufficiency was pointed out some years ago by Mr. Wilkinson King, who supposed that the imperfect closure might have a safety-valve function by permitting regurgitation into the systemic veins when there was great obstruction at the left auriculo-ventricular aperture or in the lungs. Dr. Gairdner and Dr. Bristowe have drawn attention to the effect of dilatation of the left ventricle in producing a similar maladjustment of the mitral valve. In such cases the symptoms and signs of mitral defect would be present during life, but no disease of the valves and no alteration of the natural capacity would be discovered of the aperture after death. Dr. Peacock believes that in such instances the left ventricle will be found greatly dilated and altered in shape, the apex being broader than natural. The valves and tendinous cords are stretched, and the fleshy columns reduced in size or more or less blended with the muscular wall. Several cases are related by the author which confirm and illustrate the two positions. The Cornish miners, he believes, afford frequent examples of mitral insufficiency depending on maladjustment from dilatation of the ventricle. This condition is commonly found in connection with the bronchitic or asthmatic affection known as *miner's asthma* or *miner's consumption.* It is not usually a sequence of rheumatism, nor can it be traced to shock or injury. He ascribes it to the enormous stress laid on the circulating and respiratory organs by the mode of egress from the mines. In but few mines are any mechanical means provided for bringing the men to the surface. They come up by ladders, and after working for eight hours in an impure heated atmosphere they have to spend an hour or more in climbing. The men reach the surface exhausted, out of breath, and their hearts beat violently. At or before the age of forty they are disabled, suffering from chronic bronchitis and exhibiting all the signs of emphysema, with mitral regurgitation and a dilated heart. In the Northumberland and Durham lead mines, where the men work in an equally impure atmosphere, but are let down and brought up from their work by machinery, they do not have cardiac disease more frequently than do workers above ground, although they suffer severely from asthmatic affections. It seems clearly the province of the Government to interfere by insisting on the employment of lifts to bring up the Cornish miner from his work, and to prevent the employment of boys whose immature vigour offers no resistance to the injurious influences by which the miner is surrounded.

Dr. Peacock's remarks on the relative frequency of different causes of valvular disease, and on the weight and size of the heart in health and disease—the latter occupying the greater part of his third lecture—are examples of careful observation, arrangement, and analysis of facts, and will well repay study. We have only, however, space to notice the conclusion of the book, in which he sums up his experience of the effect of treatment on valvular disease, and we select his remarks on the vexed question of the action of digitalis as of especial value. He thinks that digitalis is given too generally and indiscriminately in cardiac affections. In aortic obstruction and regurgitation, conditions which especially tax the heart's action, to overcome which it contracts violently, he believes that digitalis by impairing the power of the organ must be injurious. He has seen no reason for accepting the assertion that digitalis exerts any tonic influence over the muscular structure of the heart; but, on the other hand, he has known cardiac incompetency greatly aggravated by its use, and remarkably lessened by its discontinuance. In mitral valvular disease, however, he believes that digitalis by its *diuretic* action is eminently useful. It lessens the amount of the blood, relieves congestion and promotes absorption. This expression of opinion, coming from a physician of undoubted eminence as a clinical observer, is entitled to consideration. On no therapeutical subject has there been a greater diversity of belief expressed than on the action of digitalis in diseases of the heart. But we may safely assert that few, if any, of the disputants are more qualified by intimate knowledge of cardiac pathology, and by careful interrogation of nature, to pronounce an opinion than is the author of the lectures we have now reviewed.

Review IX.

Medico-Chirurgical Transactions, Second Series, Vol. XLVIII, London, 1865, pp. 340.

The present volume, besides the usual lists of fellows, officers, and Council, and an index, contains twenty papers, of which we give the following abstracts.

I. *Remarkable Instance of a Growth springing from the Epiglottis, which was successfully removed with the aid of the Laryngoscope.* [By George Duncan Gibb, M.D., LL.D., M.R.C.P. —Dr. Gibb commences his paper by remarking, that specimens of epiglottic growths are rarely to be met with in the metropolitan and provincial museums, although he has met with a few after a careful scrutiny. The case related was that of a lady, aged 60, who had suffered for two years from an affection of the throat, supposed at first by her son, who was a medical man, to be due to the presence of a stricture of the œsophagus, but on examination he discovered a polypoid tumour at the back of the mouth, and recommended its immediate removal. When Dr. Gibb saw her, she was pale and somewhat emaciated, and her speech was indistinct, as if the mouth was full of food. The character of the voice was peculiar, and indicated that the parts above the glottis were affected. She could not swallow fluids, but could take a little farinaceous food, or a little soft bread or an egg. She had no dyspnœa, except when she lay on her back in bed at night, during which time, but not in the day, she coughed and expectorated incessantly. On inspection with the laryngoscope, a distinct roundish, projecting tumour, of the size of a small walnut, was discovered at the back of the tongue. The epiglottis was not seen, nor the interior of the larynx, and it seemed as if the tumour grew from the lingual surface of the epiglottis. As the nature of the case was thus rendered obvious, it was determined that the growth should be removed, as the symptoms were urgent; there were no constitutional contra-indicating circumstances, and the appearance of the tumour was not malignant. The operation was accordingly performed by Dr. Gibb, assisted by some medical friends. The instrument used was the écraseur, the loop of wire being, after some difficulty, passed over the tumour, which was *snared* to its very base, and then on drawing the wire home, the growth was detached, and at the same instant, Mr. Ure, who was assisting, passed in the Vulsellum forceps, by which he removed it from the mouth. After a little cough and expectoration of blood, which was relieved by some astringent application, the character of the voice was remarkably altered for the better, and swallowing could now be effected, although with some difficulty. The tumour seemed to be epithelial, and Dr. Andrew Clark, who examined it, considered it to be formed essentially of connective-tissue, and to be of a benignant character. The operation was performed in July, 1864, and was followed by great relief of the symptoms; but in April, 1865, Dr. Gibb again examined the throat, and discovered a fleshy-looking growth, of the size of a small walnut, springing from the left half and edge of the lingual surface of the epiglottis. This growth was also removed in the same manner as on the former occasion, but the nature of the disease was now found to be malignant, and to present the character of epithelial cancer. The relief afforded by the operations must therefore be regarded as only temporary.

II. *On Vascular Protrusion of the Eyeball.* By THOMAS NUNNELEY, F.R.C.S.—Mr. Nunneley, after referring to some former cases recorded by him in the 'Transactions,' relates the particulars of three others, together with two post-mortem examinations. The first case was that of a publican, who was thrown from his horse when in a state of intoxication, and whose symptoms indicated fracture of the base of the skull, but in addition, the globe of the eye was swollen and protruded so much that it could not be covered by the lids, and vision was materially impaired. Although the case was so acute, and had so rapidly increased that Mr. Nunneley thought deligation of the carotid the only measure likely to be successful, palliative treatment was at first tried, but the patient became much worse, and accordingly the left common carotid was tied. Immediate relief followed the operation, and in course of time the eye recovered its normal aspect, and the sight was restored. At the time of the report, the man was perfectly well. The second case was that of a woman in whom the disease originated spontaneously, but the history is imperfect, as all treatment was declined. The third case was that of a street musician, who was attended by Mr. Nunneley for two years and a half, and who eventually died. He had protrusion of the right eyeball, together with bronchocele, and also a large tumour on the sternum, and there were two other tumours, one at the outer side of the orbit, and another on the right parietal bone. Although the nature of the affection was doubtful, Mr. Nunneley thought that the predominant distressing symptoms might be relieved by tying the carotid, and he accordingly performed that operation, which was, however, attended with much difficulty, owing to the large size of the thyroid gland, and the many considerable arteries and veins in the vicinity. Immediate relief to the tension and prominence of the eyeball was afforded, and, although the patient was accidentally attacked with erysipelas three days afterwards, he recovered sufficiently to return to his home, which was ten miles from Leeds, but about ten months afterwards he fractured his right arm from a slight exertion, and in seven months more he died from diarrhœa and exhaustion. The post-mortem examination showed that the tumours were all malignant, but Mr. Nunneley especially refers to the fact, that by the side of the sella turcica there was a mass of disease communicating with the disease in the orbit, and obliterating the right cavernous sinus, and the ophthalmic vein was lost in the mass and compressed. Mr. Nunneley then relates the necropsy of a case formerly related in the 'Proceedings of the Society,' and in which the carotid had been tied with success. The patient, however, subsequently died of bronchitis and serous apoplexy. In this case there was found, after death, on the right side of the sella turcica, a circumscribed aneurysm of the ophthalmic artery, just at its origin, as large as a hazel-nut. In his observations on these cases, Mr. Nunneley argues, that the idea of the protrusion of the eyeball being caused by aneurysm by anastomosis in the orbit, is altogether erroneous, and ought to be abandoned. In the great majority of such cases of protrusion of the eyeball, he contends that there is no disease whatever of the orbit, and that the seat of the affection is mostly intra-cranial. The protrusion of the eyeball is passive, and the other distressing symptoms are secondary, depending upon obstruction to the return of the blood through the ophthalmic vein, just as happens in those cases of popliteal and axillary aneurysms, where the limb swells below the tumour, because this last presses upon the accompanying vein. He admits that an aneurysm of the ophthalmic artery or even a tumour may exist in the orbit, but he thinks that the conditions which would make a small amount of pressure considerably felt do not exist in the orbit, but that they do exist in the cavernous sinus. The contents of the orbit are soft and yielding, and a small degree of pressure would scarcely produce such urgent symptoms as are observed in sudden and acute cases of protruded eyeball, whereas the walls of the cavernous sinus are dense and unyielding, and a comparatively small cause would produce a much greater effect. After alluding to the cases where protrusion of the eyeball is caused by bronchocele, emphysema, chronic bronchitis, asthma, and other similar causes, Mr. Nunneley concludes his paper by remarking, that in the most acute attacks of the affection described, whether spontaneous or traumatic, the best treatment is tying the carotid of the affected side. Two cases of cure by compression of the carotid artery have been lately reported, one in Padua and one in Verona, but Mr. Nunneley considers that the common carotid is one of the least promising of the large arteries to subject to compression, and should a clot form in that vessel and become detached, it would readily be conveyed to the brain, there producing embolism, and threatening danger to life.

III. *Case of Ulcer of the lower portion of the Ileum communicating with the Bladder.* By JOHN MORGAN, F.R.C.S.— In the case related there was an opening from the terminal portion of the ileum into the bladder, existing for nearly seven months, and the whole of the fæces were passed through the aperture for the last three months of the patient's life. The subject was a gentleman, aged sixty, who first complained of some pain and fulness in the left groin, and a tumour in the left iliac fossa soon became perceptible, gradually increasing in size, and making its way towards the mesial line of the body, when it began to cause some irritation of the bladder, and frequent desire to pass water. One day he was alarmed by the passage of flatus through the urethra, and afterwards semifeculent matter became mixed with the urine, and the deposit of mucus with shreds of lymph in this fluid, indicated inflammatory action of the bladder. The urine continued to be mixed with semifluid fæces, but the bladder appeared to become by degrees accustomed to the change, and the quantity of feculent matter discharged by the anus gradually diminished, while that voided through the urethra increased. The patient suffered very great pain, which was allayed only by large doses of opium and stimulants; he gradually became emaciated from being large and corpulent, and at last he sank. On a post-mortem examination it was found that the tumour had disappeared, but an ulcer, of the size of a sixpence, was discovered communicating directly between the ileum and the bladder. The portion of intestine above the ulcer was much distended, but that below it was of the natural size. The chief indications of treatment during life were to administer fluid or semifluid aliment, and to allay pain and irritation. The general impression made on the medical attendants was, that the disease was of a malignant nature, but this turned out not to have been the case.

IV. *Remarks upon Osteo-Myelitis consequent on Gunshot Wounds of the Upper and Lower Extremities, and specially upon the treatment of Stumps affected with Osteo-Myelitis after amputation necessitated by such Injuries.* By THOMAS

Longmore, F.R.C.S. Eng., Deputy Inspector-General.—Osteo-myelitis is one of the special consequences of gunshot wounds of bones, and is therefore of considerable interest to military surgeons. It consists of an inflammation of the medullary membrane, continued from the central canals of long bones into the cells of the cancelli and the Haversian canals. It is not supposed that there is any essential difference between osteo-myelitis developed by gunshot wounds and that which arises occasionally after the ordinary injuries and amputations of civil life, but it occurs more frequently in military practice, and has therefore been much studied by army surgeons. From the results of treatment in the late Russian and Italian campaigns, it has been argued, especially by some distinguished French military surgeons, that death followed after amputation in such a large proportion of cases, that disarticulation of the diseased limb was the only measure likely to be attended with success. This plan was therefore adopted by M. Roux in twenty-two successive cases, all of which terminated favorably; and the question involved in the views advanced by the last-named surgeon, is, whether the inflammation in osteo-myelitis necessarily extends throughout the whole medullary system of the bone? If it does, then disarticulation is the only remedy; but Baron Larrey, in a discourse delivered in Paris in 1860, arrives at the conclusion that such a result, although frequent, is not invariable, and that resections of joints and amputations in the shafts of bones are not always to be abandoned in this disease for disarticulation. In this view Mr. Longmore coincides, believing that the weight of evidence is in favour of Baron Larrey's conclusions and opposed to the views of M. Roux. But Mr. Longmore desires to give still more precision to the settlement of the question as to the treatment of chronic osteo-myelitis, especially in cases where want of success has seemed to follow partial operations for this disease. Mr. Longmore disagrees both with M. Roux and Baron Larrey, as to the necessity of disarticulation, and he adduces arguments from some preparations, four in number, in the Museum of the Army Medical Department, and from the records of actual practice. The preparations (which are illustrated by plates), appear at first sight to support the doctrine as to the necessity of treatment by disarticulation, for in all of them there has been primary amputation for a gunshot wound, but inflammation of the medullary membrane has subsequently advanced so far as to lead to the operation of exarticulation in three of the cases, while in the fourth, death, occurred without any operative interference. It would appear, therefore, that endosteitis was the result of the shock of the original gunshot wound, and that it probably existed throughout the whole shaft of the bone at the time of the amputation, and did not subside afterwards in that portion which was left in the stump. Mr. Longmore then refers to three cases in which osteo-myelitis occurred after gun-shot wounds, but in which exarticulation was not performed, and yet the patients recovered. The treatment consisted in the removal of the necrosed bone from the portion of the shaft remaining in the stump. From a review of all the facts brought to light, either by actual practice or by the examination of morbid specimens, Mr. Longmore is led to the conclusion that in all cases, however severe, of endosteitis and endosteitic necrosis after gunshot injuries, a cure should be attempted by careful and complete removal of sequestra before resorting to the extreme measure of exarticulation. It is not of so much importance to avoid exarticulation of a humeral as it is of a femoral stump, for the one is almost always without danger to life, while in the other life must always be seriously endangered. While agreeing generally with Baron Larrey in reference to the nature, progress, and treatment of osteo-myelitis after gunshot injuries, Mr. Longmore thinks that exarticulation should not in any case be resorted to for the removal of the diseased stump, until the effects of the complete removal of every particle of dead bone by proper surgical measures have been ascertained.

V. *Case of Aneurysm by Anastomosis of the Scalp, treated successfully by Ligature of the Common Carotid and Setons.* By George Southam, F.R.C.S. Eng., Surgeon to the Manchester Royal Infirmary.—The patient was a married woman, aged twenty-eight, who had been suffering from dilatation of the vessels on the right side of the scalp, for upwards of eight years. The disease was at first limited, but it gradually spread over the parietal and temporal bones towards the occiput, and to the frontal bone. At length a small ulcer formed over the parietal protuberance, and hæmorrhage occurred from it on several occasions, but was at first easily arrested by lint and bandaging. But the last bleeding was so severe that the patient was admitted into the Manchester Infirmary, and Mr. Southam found that the disease extended over all the right side of the scalp. The temporal artery and its branches were much enlarged, some of them to the size of the little finger, and communicated to the hand a distinct arterial thrill. The pulsation of these arteries was completely suspended by compression of the common carotid. On attempting to remove the lint covering the ulcerated surface, arterial hæmorrhage occurred to such an extent that Mr. Southam called a consultation with his colleagues, and with their consent placed a ligature on the trunk of the common carotid, which measure had the desired effect. There was some difficulty in completing the operation, in consequence of the occurrence of severe constitutional symptoms, but the patient rallied, the hæmorrhage ceased, and the ulcer became dry. Four setons of worsted were now passed through the diseased structure, one across the temporal fossa, the others through the parts of the scalp where the vessels were most distinct. These setons all suppurated freely, and some more were introduced, and after some sloughing of the parts had taken place, the ulcer eventually healed, and the patient left the hospital nearly well. Mr. Southam remarks, that deligation of the carotid must not alone be confided in for aneurysm by anastomosis of the scalp, because this disease does not consist in a morbid condition of a single vessel, but involves the entire temporal system with its arteries, veins, and capillaries. Still, deligation even when not required for the suppression of hæmorrhage, has its advantages, in causing a temporary interruption to the circulation through the diseased structures, and thus affording a favorable opportunity for the application of other remedies. The use of setons was therefore resorted to as soon as it was evident that the scalp was supplied with blood sufficient for reparative purposes. The setons were first passed across the trunk and branches of the temporal and occipital arteries, and also through the parts of the scalp where the vessels appeared to be most dilated, and others were afterwards introduced wherever any return of pulsation showed itself. But setons alone cannot be trusted in the treatment of these cases, and their success in the instance recorded by Mr. Southam can only be attributed to the quiescent state of the circulation produced by the ligature of the carotid.

VI. *Congenital Hydronephrosis in a boy four years old; repeatedly tapped; recovery.* By Thomas Hillier, M.D., Lond. —The child when first seen was three years and four months old, and it was stated that the abdomen had been large ever since birth. The enlargement, however, was greater at some periods than at others, and the circumference of the abdomen had varied from twenty-four to twenty-seven inches. The child had always had a large appetite, and nothing abnormal had been observed in the alvine or urinary secretions. The abdomen, when first examined, was very large and tense, and it was supposed that there was fluid in the peritoneal sac. Under this impression, the medical treatment consisted in the administration of diuretics and purgatives, which was not followed by any benefit. The swelling increased and afterwards diminished, and as the general health was not affected, he was sent home, but came again under treatment in a few months afterwards. On being examined, it was now found that the swelling was greater below than above the navel, and greater on the right side than on the left, and midway between the navel and the end of the ensiform cartilage there was a distinct sulcus, more marked on the left than on the right side, and moving with respiration. This sulcus was found to correspond with the lower border of the transverse colon. From these and other circumstances it was now obvious that the boy had a large cyst with not very thick walls, springing from the right side of the abdomen, and very probably connected with the right kidney; and it was resolved to tap the cyst to relieve the patient of his burden. A trocar being introduced, 102 fluid ounces of a clear yellowish fluid escaped, faintly acid in reaction, containing no albumen, and having all the characters of dilute urine. Chemical analysis proved that it contained a large proportion of urea, some chloride of sodium, a little uric acid, and some phosphates and sulphates. From this analysis it was concluded that the cyst was the dilated pelvis of the right kidney. There were no bad symptoms consequent upon the tapping, but the swelling gradually returned. An experiment was now instituted to determine whether the fluid which was re-collecting in the cyst was of the same character as the urine, and accordingly some ferrocyanide of potassium was given internally, and the urine and the fluid in the cyst were tested with perchloride of iron. The urine gave a dark colour with the perchloride, but the fluid in the cyst gave no reaction with this test. On subsequent examinations it was found that the fluid which the patient passed from the urethra was almost identical in character with that of the cyst, and it now became evident that the cyst and the bladder communicated together. An attempt was made to keep the cyst open by means of a canula, but this was done only for a short time, as the fluid soon ceased to flow through the canula, and the wound healed. Notwithstanding all these unfavorable symptoms the boy improved in health and gained flesh, and was discharged from the children's hospital to the convalescent establishment at Mitcham. The abdomen was still swelled, its size varying from time to time.

VII. *On the Solvent Treatment of Urinary Calculi: an Experimental and Clinical Inquiry.* By WILLIAM ROBERTS, M.D., Physician to the Manchester Royal Infirmary.—The object of the author of this paper is to determine the question as to the possibility of dissolving urinary concretions, or any class of them, within the living cavities. The present opinion is, that calculi lodged in the urinary organs, and too large to be discharged from the natural passages, cannot be dissolved by any mode of internal treatment; but the experiments of Dr. Roberts point at results which are likely considerably to modify the existing views on this subject. They do not, however, indicate the general possibility of substituting a solvent for a mechanical treatment of vesical calculi, but they suggest an essential improvement in the treatment of renal concretions, and they also indicate that uric acid and cystin, under certain circumstances, are capable of solution in the bladder by means of medicines administered by the mouth, at a rate admitting of practical application. In certain selected cases, Dr. Roberts thinks that the solvent treatment deserves to be resolutely tried before having recourse to the more dangerous methods of lithotomy and lithotrity. He does not profess to have discovered any new solvent for the stone, but draws attention to a better application of a treatment long known in principle, but abandoned from the faulty manner in which it was formerly carried out. In Dr. Roberts's paper urinary concretions are considered, for the sake of convenience, as consisting of uric acid, oxalate of lime, or the earthy phosphates. Cystin falls into the same category with uric acid, and carbonate of lime with the phosphates. The paper is divided into two parts, the first, which is much the longer, being devoted to observations and experiments relating to the solvent treatment of uric acid calculi by alkalizing the urine, through the internal administration of medicines; the second part contains the results of experiments on the solvent treatment of uric acid by injections into the bladder, and on the solvent treatment of oxalate of lime and phosphatic concretions. The experiments are numerous, and the results are given with great precision, but they all tend to show that none of the solvents possess any real efficacy in the case of stone, except those which are capable of rendering the urine alkaline. The chief agents, therefore, are the salts and other preparations of potash and soda, and Dr. Roberts divides one class of his experiments into two parts, in one of which he shows the effects of these alkalies on calculi out of the body, and in the other, their effects upon patients actually suffering from calculus. The potash-salts, as might be expected from their chemical nature, were found sensibly to excel the soda-salts as solvents for uric acid, and the strength of the solutions was found to affect their solvent capacity more than any other condition. The greatest solvent power was found to lie in solutions containing from forty to sixty grains of carbonate to the imperial pint; with stronger solutions the fragments were covered over with a white crust of alkaline bi-urate, which sensibly interfered with the solvent action. The most convenient method of alkalizing the urine in the living subject is by administering the alkaline bicarbonates, acetates, and citrates, and after a large number of experiments, the acetate and citrate of potash were found to offer superior advantages over any other preparations. They form draughts which are nearly tasteless, they do not interfere with digestion, and they are extremely soluble. In illustrating the application of the solvent treatment to cases in actual practice, Dr. Roberts commences by quoting some passages from Mascagni, who treated renal gravel of uric acid successfully in his own person, by taking the alkaline carbonates, and Dr. James Jurin is said, in 1740, to have relieved himself of some vesical calculi by the same means. Dr. Roberts then relates the particulars of three cases treated by himself, and although the results were not quite satisfactory, they offer some promising features in favour of the solvent system. In one case the calculus was composed of uric acid, in the second, of alternating layers of uric acid and oxalate of lime, and in the third, of oxalate of lime. The experiments made on the solvent treatment of uric acid calculi by injections into the bladder have hitherto been unsatisfactory, and Dr. Roberts considers, from his researches on the solvent action of alkalies with these calculi, that there is no prospect of any useful practical application of this method of treatment. Only uric acid calculi can be dissolved by the potash-salts; those of oxalate of lime are practically unaffected by acid or alkaline solvents; and phosphatic calculi, although unsusceptible to the action of alkaline solvents, may perhaps be affected by the use of acid injections into the bladder. Our limits have prevented us at the present time from giving more than a brief sketch of this very elaborate paper: but we shall probably have a future opportunity of again drawing attention to the subject, as we trust that the Gulstonian Lectures which Dr. Roberts gave at the Royal College of Physicians this year, "On the Use of Solvents in the Treatment of Urinary Calculi and Gravel," may be published.

VIII. ***On Delirium, or Acute Insanity during the decline of Acute Diseases, especially the Delirium of Collapse.*** By HERMANN WEBER, M.D., F.R.C.P., Physician to the German Hospital.—Dr. Weber does not refer in this paper to the mental aberrations occurring during the increase and height of acute diseases; but to the delirium or the acute insanity which occasionally breaks out when the disease has already entered into the stage of decrease, when the fever has almost or entirely ceased, and when, perhaps, the patient has just been declared convalescent. Many authors have previously described this kind of mental aberration, as Thore, Griesinger, and others, but

Dr. Weber thinks that many medical men have never observed it, and he therefore describes it as he has met with it in several cases. The instances which have come under his own observation are rather numerous—a circumstance he can account for only by that peculiar caprice by which it happens that remarkable and rare forms of disease occur sometimes in clusters to one man in a certain period, while they are but rarely or never seen by others, or by the same observer at other periods. The cases are seven in number, two occurring after measles, one after erysipelas, two after pneumonia, and two after typhoïd fever. In all, the symptoms were on the decline when the delirium supervened; they all terminated favorably, and the treatment consisted in the administration of opiates, wine, and nourishment, and in keeping the patients quiet. But although the delirium manifested itself when the pyrexia had almost ceased, and the general appearances indicated the absence of fever, the thermometer proved that the temperature of the blood was higher than normal, except in one case. As to the condition of the brain and nervous system on which these derangements depend, or with which they are found connected, it is generally agreed that it is "anæmia," as is shown by the coldness of the hands and feet, the cold, pale, shrunken face, the state of the heart and skin, and of the whole body. But there is a difference between the anæmia caused by repeated losses of blood, or want of food, or excessive excretions, and the condition of the brain connected with the instances of mental aberration described by Dr. Weber. In the latter there is probably only a sudden and transitory change in the capillary circulation of the brain, and through this an equally transitory change in the nutrition and action of the brain-cells, a change which may be caused by a sudden sinking of the heart's power. In this sense Dr. Weber uses the term "delirium of collapse," meaning, in the present instance, only the temporary collapse just described, and not that which often occurs suddenly in acute diseases, or that which immediately precedes death. The affection described in Dr. Weber's paper, although characterised by violent and maniacal delirium, does not appear to be dangerous, and the prognosis may generally be considered favorable, the derangement disappearing in the course of a few hours or days, under proper treatment, without leaving any traces except those of a very vivid dream. Sometimes, however, it would appear that the mental disorder commencing during the decline or convalescence from acute diseases may become chronic, but Dr. Weber has no personal experience on this point.

IX. *On Intermittent Hæmaturia; with Remarks upon its*

Pathology and Treatment. By GEORGE HARLEY, M.D., F.R.S., Professor in University College.—In the first case described by Dr. Harley, the intermittent hæmaturia appeared to be due to malarial poisoning; in the second, it seemed to be simply the result of the direct effects of cold acting upon a predisposed constitution. The first case was that of a medical gentleman who had resided many years in the West Indies, but who had been compelled to give up practice and return to England, in consequence of repeated attacks of ague. He passed at different periods of the day three different kinds of urine, one voided at 8 a.m. being in every respect normal, another passed at 2 p.m. being of a dark chocolate-brown colour, of specific gravity 10·32, containing a large quantity of albumen, and a great excess of urea; and a third kind passed in the evening, being again almost if not quite normal. The dark-coloured urine contained renal tube-casts, and a large number of free granules of a brownish-red hæmatin colour. In the second case, which was that of a man living in London, the chief peculiarity appeared to be that he passed the blood-coloured urine when he was cold, and this feeling of coldness was independent of external temperature, for it existed when he was sitting in front of a large fire. On applying the thermometer it was found that the temperature of the palms of his hands was only 60° Fahr., while that of Dr. Harley's hands, taken at the same time, was upwards of 95° Fahr. As both the above cases appeared, after full examination, to be of a functional character, and to present some of the phenomena of ague rather than of disease of the urinary organs, the ordinary line of treatment recommended in hæmaturia was abandoned, and an alterative and anti-periodic plan was substituted. Aperients and mercurials were therefore ordered in order to remove the congestion of the chylopoietic viscera, and quinine was given to relieve the paroxysmal discharge of blood; and in both cases the treatment was quite successful. Dr. Harley remarks, that it is of great clinical importance to make a correct differential diagnosis between such cases as those described and those of ordinary hæmaturia, for otherwise it would be impossible to treat them with much chance of success. In a short appendix Dr. Harley refers to three cases related by Rayer, but not falling under the observation of that physician, in which there was intermittent hæmaturia, and in all of which relief was obtained by the use of quinine. One of these cases was described by Dr. Elliotson as occurring in University College Hospital in 1832.

X. *Notes of Four Cases of Intermittent Hæmaturia.* By WILLIAM H. DICKINSON, M.D. Cantab., Curator at St. George's

Hospital.—The first of the cases described by Dr. Dickinson was that of a man who had often been under observation at St. George's Hospital, and was attended at different times by most of the physicians of that institution. He had had an attack of tertian ague more than twenty years ago, but recovered without any bad consequences, as far as was known; but in the autumn of 1859 he caught cold, and passed black and apparently bloody urine. From that time he had been subject to short attacks of hæmaturia, which were brought on by no other cause than exposure to cold. The urine passed during one of these attacks was frequently examined, and was always found to present the same characters. It was black and turbid, acid, contained much albumen, and was generally above 10·20 sp. gr. Between the attacks it was natural. This man was subjected to various kinds of treatment, none of which seemed to affect the disorder. The hæmorrhage always ceased on the removal of the cold which produced it, and recurred with undiminished readiness on the next exposure. Two other cases of intermittent hæmaturia were supplied to Dr. Dickinson from the practice of Dr. George Johnson; and a fourth from the practice of Dr. Frederick Cock. The last case was that of a country gentleman who had never had ague, and who always traced the attacks to cold. He had very little medical treatment, and his complaint did not seem to give him much inconvenience. Dr. Dickinson thinks the cases of this affection are still too few to allow much to be said of their pathology. From the small size of the cylindrical casts in the urine, and the rare occurrence in them of renal epithelium, he thinks that the blood flows from the Malpighian bodies, while the uriniferous tubes retain their natural lining. The disease must therefore be considered rather as connected with the blood than of primarily renal origin, and it presents some features in common with ague. Dr. Dickinson considers it to be a malady *sui generis*, and he does not believe that any treatment can do more than mitigate its effects; in the cases related in this paper the attacks were without any periodical tendency, and, where the experiment was tried, they were unaffected by quinine.

XI. *Supplement to a paper entitled Further Observations on the Structure and Treatment of Uterine Polypi, published in the Forty-fourth Volume of the 'Medico-Chirurgical Transactions.'* By Robert Lee, M.D., F.R.S.—This supplement contains the details of fifteen cases of uterine polypi which have been successfully treated by Dr. Lee; in addition to fifty other cases, also successful, formerly reported. As the greater number of large uterine polypi are fibrous tumours covered with the lining membrane and a portion of the muscular coat of the uterus, and have large arteries and veins distributed throughout their substance, Dr. Lee does not venture to drag them out of the vagina and divide their roots with the knife. He usually applies the ligature in such cases with the bent rod, and the sloughing polypus is removed when the ligature is long in dividing the root. Even when the polypus is small and the ligature has been applied with the double canula, Dr. Lee does not consider it safe to leave the ligature many days round the root of the polypus, when the circulation through the tumour has been stopped and the mass of the tumour is in a sloughing condition. To this plan of treatment Dr. Lee attributes in a great measure the uniform success which has attended his operations.

XII. *On Amputation of the Leg by a long Rectangular Flap from the Calf.* By Henry Lee, F.R.C.S., Surgeon to St. George's Hospital.—The plan adopted by Mr. Lee is a modification of that recommended by Mr. Hey, and that recommended by Mr. Teale. The former consists in making a long flap from the back of the leg, and the latter in making a flap from the front of the leg, the last-named forming a covering composed of parts free from large nerves and furnishing the material for a soft cushion movable over the ends of the bones, and which enables the patient to bear a part of his weight upon the extremity of the stump. In Mr. Lee's operation, which is illustrated by a sketch, two parallel logitudinal incisions are made along the sides of the leg, and these incisions are met by a third transverse incision below and behind, and a fourth incision is made transversely through the skin in front of the leg so as to form a flap in this situation one fourth only of the length of the posterior flap. The largest flap is thus formed from the posterior part of the leg, and is much thicker than when taken from the front; it is consequently much less liable to slough, and affords a much more efficient protection to the ends of the bones and a thicker and softer pad on which to rest a part of the weight of the body when an artificial leg is applied. Mr. Lee describes three cases in which this operation was successfully performed, and he refers to two others all falling under his own care, and constituting the only cases, as he believes, in which this particular operation has been performed up to the present time.

XIII. *An account of a Case in which a Sixpence was lodged in the Larynx during Ten Weeks.* By J. Burdon Sanderson, M.D., F.R.C.P.; and John Whitaker Hulke, F.R.C.S.—The patient was a healthy man, aged twenty-seven, who was talking with some friends, having a sixpence in his mouth, when he laughed at something occurring in conversation, and the sixpence disappeared and he immediately fell to the ground suffocated. He was taken to St. Mary's Hospital, where his throat was examined, but it is stated that nothing could be detected, and in a short time his breathing became easier, and he was discharged from the hospital, at his own request, the day after his admission. He does not seem to have experienced much inconvenience for the ensuing ten weeks, except that he complained of occasional sore throat and pain or difficulty in swallowing liquids. But at the end of this period his breathing became somewhat suddenly embarrassed, and he came under the notice of Dr. Sanderson. It appeared from his statement and from the character of the dyspnœa from which he was suffering, that the sixpence was still in his throat, and on laryngoscopic examination it was seen without difficulty. It was placed horizontally in the glottis, below the false vocal cords, and between the edge of the coin and the arytenoïd cartilages there was a transversely oblong breathing space, apparently not wider than a tenth of an inch from front to back. It was at once determined to attempt the extraction of the coin, with the aid of the laryngoscope, through the upper opening of the larynx; but this proceeding, although varied by several contrivances, was unsuccessful, and it was resolved to have recourse to laryngo-tracheotomy. This was accordingly done, and the patient being placed under the influence of chloroform, the crico-thyroïd membrane, the cricoïd cartilage, and two or three of the upper tracheal rings, were divided; the wound was then held apart above and below, and search was made for the coin with a forceps introduced from below. It could be felt, but not seized, and after several unsuccessful efforts made to extract it, it was at length displaced in such a way as to be drawn forwards over the epiglottis on to the root of the tongue, and at this moment the patient made a sudden gulp and swallowed the sixpence. The case now went on favorably, the coin was discharged the next day per anum, the external wound cicatrized, but the respiration was not quite healthy when the patient last presented himself for inspection and examination, and the mucous membrane of the larynx and of the epiglottis was still unnaturally red and turgid.

XIV. *On a Case of the "Hæmorrhagic Diathesis."* By W. Morrant Baker, F.R.C.S., Demonstrator of Anatomy at St. Bartholomew's Hospital.—The patient was a tradesman, aged twenty-three, who came from the country to London, in 1862, having previously been strong and healthy. Soon after coming to town he was seized with an illness, supposed to be due to some affection of the kidneys, from which he recovered, or nearly so, but about the beginning of 1863 he again fell ill, vomiting, purging, and hæmaturia being the most prominent symptoms. At about the same time a small swelling, like a little boil or pimple of acne, appeared near the angle of the mouth, and after being accidentally rubbed, bled profusely at short intervals for a fortnight, and it was thought that he must have lost about two quarts of blood at this time. But the bleeding ceased and did not recur for nearly a twelvemonth, after which time another small pimple on the face began to bleed with great violence. On this occasion the hæmorrhage continued until a common-sized wash-hand basin was nearly full of blood, notwithstanding the efforts made to stop it by pressure and astringents, and even by the actual cautery. Similar hæmorrhages took place at intervals for about seven weeks, the blood, apparently arterial, usually spurting out quite suddenly from one of the pimples on the face, and often with a jet to the distance of a foot or more from the patient. It was always difficult, and sometimes almost impossible to stop the bleeding, pressure being the most effectual method, but even with this the blood welled from beneath unless the pressure was very firm. The treatment adopted was without any other than temporary good effect, either on the hæmorrhages or the patient's general health, and it was considered best to remove him to the country, which was accordingly done, and with remarkable benefit. On returning to London, however, he again fell ill, but had no return of hæmorrhage except from the rectum, and in December 1864 he was well, having had no further hæmorrhage, but he had resided most of his time in the country. With regard to the cause of the disease in the present instance, no definite information could be gained, and no theory could be formed, except, perhaps, that the change from a country life to one in London, where the patient lived in a close and ill-ventilated dwelling, might have had some injurious influence; an opinion which is somewhat strengthened by the circumstance that immediate benefit was derived from change of air.

XV. *A Second Series of Fifty Cases of Ovariotomy, with Remarks on the Selection of Cases for the Operation.* By T. Spencer Wells, F.R.C.S.—Mr. Wells thinks that he has now collected a sufficient number of cases to begin the inquiry as to the selection of persons for the operation; the rejection of those in whom there can be no reasonable hope of recovery; the conditions in which hope and fear are equally balanced in reference to the results; and the circumstances which are so favorable that the operation may be properly recommended to a patient as one of moderate or small risk, compared with the danger of the disease. After giving a summary of the fifty cases, Mr. Wells observes that of the whole hundred cases he has related (fifty on a former occasion and fifty on the present) in which the operation was performed, sixty-six recovered and thirty-four died, and that in both the series of cases the numerical results were precisely the same, namely, thirty-three recoveries and seventeen deaths. In reference to the influence of age upon the result of ovariotomy, it is shown by a table that the smallest mortality is below the age of twenty-five and above the age of forty, and that between these ages the mortality is comparatively high. The death-rate was pretty much the same whether the patients were married or unmarried. Comparing the results of the cases treated in the hospital with those occurring in private practice, it was found that in the former the mortality was 29·6 per cent., while in the latter it was 39·1 per cent. In the hundred cases the pedicle was secured and kept outside the peritoneum by a clamp in sixty-six patients; and according to the statistics given, it may be inferred, so far as regards the selection of cases for ovariotomy, that the probability of success is nearly doubled when the attachments of an ovarian tumour permit the end of the secured pedicle to be kept outside the cavity of the abdomen until it has separated. Several other questions of practical importance are not at present determined, because the number of cases is as yet too small to give value to the calculations, but on one important point Mr. Wells thinks that experience has been sufficient to warrant the acceptance of some such rule as the following, namely, that "The probable result of ovariotomy can be estimated with far greater accuracy by a knowledge of the general condition of the patient than by the size and condition of the tumour." In other words, a large tumour, extensively adherent in a healthy person having a well-regulated mind, may be removed with far greater probability of success than a small unattached cyst from an anæmic or leukhæmic patient, or one whose mind is too readily acted upon by either exciting or depressing causes.

XVI. *On the Obstacles to the Re-establishment of Natural Respiration after the Performance of Tracheotomy Cases, with Remarks.* By THOMAS SMITH, F.R.C.S., Assistant-Surgeon to St. Bartholomew's Hospital and to the Hospital for Sick Children.—In this paper Mr. Smith draws attention only to those hindrances to the withdrawal of the canula and to the restoration of the passage of the larynx, which take effect after the operation of tracheotomy and in consequence of it. In other words, they have no reference to the persistence of any cause of obstruction which may have necessitated the introduction of the tracheal tube. From the history of the cases related, Mr. Smith considers that among the obstacles to the re-establishment of natural respiration after tracheotomy are the following, namely, 1st, a narrowing or complete obliteration of the passage of the larynx by the growth of granulations above and around the canula; 2nd, an impairment or complete loss of those functions of the muscles of the larynx which regulate the admission or exclusion of air through the rima glottidis; and, 3rd, adhesion of the opposed surfaces of the vocal cords. In order to obviate the occurrence of these contingencies, Mr. Smith draws attention to an instrument invented by Luer, of Paris, to be worn in the orifice of the canula as soon as practicable after the performance of tracheotomy. This instrument, by means of a bullet-valve, ensures a free entrance of air by the wound through the canula, while it obliges all air leaving the cavity of the chest to pass out through the larynx. The advantages of this instrument are, that its occasional and early employment favours the expulsion of false membranes and tenacious mucus from the cavity of the larynx during coughing; that it restores the power of speech and enables the laryngeal muscles to exercise some of their ordinary functions, so as to prevent injury from disuse; and that by accustoming the larynx to the passage of air through its cavity it may diminish the irritability of its muscles. Mr. Smith, while approving the use of Luer's instrument, has devised a more simple method for attaining the same end, and for the bullet-valve of Luer he substitutes an india-rubber flap, the attachment of which to the canula is secured by a plug-shaped extremity. The apparatus, as recommended by Mr. Smith, is light and inexpensive, and answers all the purposes of the more costly and more weighty instrument.

XVII. *The application of Sutures to Bone in Gunshot Fractures, with Cases: also, Remarks on their similar Use in other Fractures and Operations.* By Benjamin Howard, M.D., late Assistant-Surgeon, Regular Forces, United States Army.—Dr. Howard observes that in active warfare it is hopeless to anticipate a mode of transportation for wounded soldiers which shall ensure perfect rest of a broken limb; and he therefore endeavours so to improve the condition of the parts as to diminish the motion of the fractured ends of a bone during removal to an hospital. His plan consists in cutting down upon the seat of fracture, removing all the fragments of bone, making a clean section of the fractured ends of the shaft, effecting perfect apposition, maintaining it by metallic sutures, and with the assistance of a light splint, securing both apposition and rest during the transport of a patient from the field of battle. Two cases are related in which this plan was adopted, and there are several drawings by which the proceeding is illustrated. Its advantages are that it secures the absence of everything which may act as a foreign body and a dangerous source of irritation, and it effects a complete coaptation of the parts and perfect rest.

XVIII. *On the Pathology of Tetanus, illustrated by Cases and Drawings.* By J. Lockhart Clarke, F.R.S.—Mr. Lockhart Clarke observes that no observer, before himself, has hitherto discovered anything like definite lesions, or indeed any abnormal appearance, in cases of tetanus, except a general increase of vascularity or a variable and local congestion. But in this communication Mr. Clarke thinks that he shows, beyond the possibility of a doubt, that in six cases examined by himself, the cords exhibited lesions of structure of different kinds and frequently of surprising extent. Some of the cases referred to by Mr. Clarke were of the traumatic kind, one was idiopathic, and in others the history was not explained. The peculiar morbid appearances observed by Mr. Clarke in transverse sections of the spinal cords are illustrated by drawings, but are not easily described; they seem, however, to consist mainly of disintegration and softening of portions of the gray substance in the centre of the cord, which appeared in certain parts to be in a state of solution. The fluid thus formed, however, is at first more or less granular, holding in suspension the fragments and particles of the disintegrated tissue, but in many places it is perfectly pellucid. After detailing the actual pathological appearances which he observed, Mr. Clarke proceeds to investigate the questions which are involved in the discovery which he has made. He asks whether structural lesions are present in the cases of tetanus which recover? and he thinks it probable that they do not exist in such cases. Again, he asks whether the structural lesions or disintegrations of tissue are the effects of the functional excitement of the cord manifested in the tetanic spasms? and he answers this question in the negative. Again, Are the structural lesions of the nerve-tissue the cause of the tetanic spasms? Mr. Clarke answers that they are not the direct or sole cause, for in those cases of paralysis in which similar lesions exist, they do not give rise to tetanic spasms or convulsions; and lastly, in reference to the question, On what do the tetanic spasms really depend? he replies that they probably depend on the conjoint operation of two separate causes, namely, the injury of the peripheral nerves and a hyperæmic and morbid state of the blood-vessels of the cord, and the resulting exudations and disintegrations.

XIX. *On Morbid Changes in the Stomach and Intestinal Villi present in persons who have died of Cancer.* By SAMUEL FENWICK, M.D., Assistant-Physician to the City of London Hospital for Diseases of the Chest.—Dr. Fenwick observes that in many cases of cancer it is difficult to account for the death of the patient, as there is no secondary formation in any important organ, and the failure of strength is out of proportion to the local mischief, so that we are forced to admit either that the blood has become infected, or that some fatal change of a non-cancerous nature has occurred in the viscera. It is probable, therefore, that the medical practitioner will derive more advantage from a knowledge of the changes which will be found to take place in the vital organs than from further inquiries directed exclusively to the locally diseased tissues. Dr. Fenwick accordingly states the results obtained from the microscopical examination of the stomach in fifty-seven, and of the intestines in seventy-three, cases of cancer. The appearances are described in detail, but the general features seem to be an alteration of the secreting tubes of the stomach and a congested condition of the villi of the intestines. The gastric tubes could be readily separated from each other, and contained gastric cells more transparent than usual, breaking dowu into granular matter with very slight pressure. The chemical composition likewise varied, the softness of the mucous membrane being connected in certain cases with an increased formation of fat, albumen, or gelatine. The villi of the intestine, when examined by the microscope, were seen to be loaded with dark cells and nuclei, but Dr. Fenwick admits that the dark appearance of the villi is not peculiar to cancer. But the question arises as to the relation, if any, existing between these conditions of the mucous membrane and cancer, and whether they are produced by cancer or are both the results of a common cause; or does cancer originate from a disease of the digestive organs? To these inquiries Dr. Fenwick does not think that a definite answer can at present be given, but he argues that a knowledge of the frequency with which diseases of the digestive organs present themselves in cancer cannot fail to prove useful to the practitioner. He will see in these alterations in the blood-making organs an explanation of the anæmia that so often ushers in and accompanies malignant disease, and he may hope to discover means to arrest the progress of these alterations. If also further inquiry should teach that certain varieties of gastric and intestinal disease always co-exist with particular forms of cancer, the state of the digestive organs ought to be considered in determining the advisability of operation, as success must depend upon the condition of the internal organs.

XX. *Fibroïd Degeneration of the Lungs.* By HENRY G. SUTTON, M.B. London., M.R.C.P. London.—Dr. Sutton commences his paper by remarking that death from acute disease alone is very rare in persons previously in good health, and that the fatal result is generally due to the supervention of acute changes upon chronic degeneration of the tissues. Hence has arisen the conviction in the minds of the profession that the success of treatment does not depend so much on the particular disease itself, as on the healthy or degenerate condition of the tissues of the patient's body. The "Fibroïd Degeneration of the Lungs" is described by Dr. Sutton as follows:—On section, a part or almost the whole of one or of both lungs is found to be solid, to be increased in weight, to sink in water, to be firm and tough, breaking down under the finger with great difficulty, and in some cases grating when cut with the knife. The microscopical examination of morbid specimens has led him to the conclusion that in such cases there had been a formation of new tissue-elements, and that these are such as are usually considered to represent newly-formed connective or fibroïd tissue: that the newly-formed fibroid tissue has invaded and in some parts destroyed the normal lung-tissue; and that these fibroïd elements are found to be most highly developed in the immediate vicinity of the connective tissue surrounding the minute bronchial tubes, lobules and lobulets, and also in the vicinity of the thickened pleura. The sufferers from this affection are well-built persons, more frequently males than females, and usually in middle or advanced age; and its progress seems to be slow, some cases extending over months or years. Dr. Sutton attributes this morbid condition, in a very great measure, to habits of long-continued and excessive spirit-drinking. He believes that there are certain features of the body by which, in a great many cases, this fibroïd diathesis, on which the local disease depends, may be diagnosed, and he states that there is evidence that such fibroïd changes are not due to a so-called scrofulous constitution. The build of the patients, their hereditary tendencies, and their general symptoms and habits, are very different from what is found in cases of tubercular phthisis, and a distinction ought therefore to be drawn between the two diseases.

REVIEW X.

On the speedy relief of Pain and other Nervous Affections by means of the Hypodermic Method. By CHARLES HUNTER, Surgeon to the Royal Pimlico Dispensary, &c. *London*, 1865. Pp. 64.

IT is interesting to trace the steps of any discovery, whether it be that of a great mechanical invention or of any important method of medical treatment. We make the remark, referring to the manner in which the hypodermic mode of encountering disease has been advanced, as stated by the author of the valuable pamphlet before us. Only a few years ago the method in question was unknown. Dr. Wood of Edinburgh, Dr. Kurzack of Vienna, and Mr. Rynd of Dublin, seem about the same time —1843, 1844—to have made the first trial of it. Whether Dr. Kurzack or Dr. Wood is entitled to claim priority of discovery, we do not appear to have the requisite data for determining. This, however, seems to be certain, that to Dr. Wood the profession in this country and in France is mainly indebted for its promulgation and for the proof of its efficacy in certain painful affections, locally applied, *i. e.*, to the spot where pain is felt on pressure, he maintaining that it was efficacious when most successful in disease of centripetal, not of centric origin. The next advance appears to have been made by our author, and this a very important one, viz., that the injection of the remedial agent need not be made where the pain is experienced, but anywhere, provided it be fairly introduced beneath the cutis into the cellular tissue, and that for certain reasons a distant part, such as the inside of the arm, is preferable; and, further, that the curative power of the agent is not restricted to local affections, such as neuralgic pains in centripetal affections; but is efficient in those of centric origin, such as delirium tremens, acute mania, melancholia, chorea. He entered on the inquiry, he states, in 1858, and has since, judging from the evidence in the pamphlet before us, carried it on with great zeal and eminent success.

This pamphlet consists partly of matter previously published in papers contributed at different times to medical journals and partly of additional and new matter. Owing to this construction there is a little repetition in it, which may well be overlooked, considering the very great importance of the subject.

That the hypodermic method has great claims to attention cannot be questioned; and we think it can hardly be questioned that it is deserving of preference whenever powerful medicines are to be administered, especially such as morphia, atrophine, strychnia, and the other vegetable alkaloids. Such is the expressed opinion of a physiologist of high authority, formed from his experiments on animals. He, M. Claude Bernard says—

"Je pense même, à raison de ces circumstances, que l'absorption sous-cutanée, qui n'a été employée jusqu' ici, sur l'homme que par exception, devra devenir méthode generale pour l'administration de tous les medicaments energiques, et à l'etat de puretè."[1]

The great advantage of this method appears to be that the agent, as to its action, being rapidly absorbed and conveyed into the circulating blood, escapes all the uncertainties to which it would be liable if otherwise administered, especially if taken into the stomach, the latter organ and viscera connected with it at the same time not being exposed to direct disturbance of function from the injected medicine. The cases which Mr. Hunter adduces are strongly confirmatory of this view. Its minor advantages are that the operation of injection, with ordinary skill and care, is easily performed, and, as it were, in an instant, and without the loss of a drop of blood, and with no more sensation of pain than that produced by the puncture oı a needle. The author gives a figure of the little instrument which he uses, made by Messrs. Whicker and Blaise. It is of ingenious construction, the funnel of glass, with silver fittings, the end of the pipe sharp-pointed, fit for penetrating the skin, and tipped with hardened gold to prevent rusting; its piston works by a screw-rod, a half a turn of which impels half a minim as a fine drop from the end of the pipe. The preparations which he prefers for injection are the perfectly neutral tinctures of the alkaloids, from the use of which, never injecting more than thirty minims, he has experienced no bad effects locally, neither suppuration nor inflammation in the punctured part, the simple precaution being taken to cover the puncture with a *narrow* slip of adhesive plaster—and this chiefly for greater security against the escape of the fluid.

Whilst he considers the hypodermic method the best for administering potent medicines, he cautions his readers that it is not to be considered specific—he inculcates that it requires to be employed with discriminating judgment in some cases, for instance, of a perilous and urgent kind *instanter*; in others less urgent, and with mixed symptoms, after a preparatory medication; and in many—those slight cases—not at all.

Not the least valuable part of his paper is that in which he treats of the peculiar effects of the agents which he has most

[1] "Recherches experimentales sur l' opium et ses Alcaloïdes," 'Arch. Gen. de Med.,' 1864.

employed—morphia and atropine. Morphia, he is of opinion, is a tonic and antiphlogistic, reducing the pulse and the rate of respiration; and whilst diminishing the oxygenation of the blood, increasing cutaneous action, and, consequently, it should follow, reducing the temperature of the body. Atropine, which he is the first to have made the subject of careful inquiry as to its medicinal effects on man, he endeavours to show, and we think successfully, is, as it were, the opposite of morphia—that, instead of retarding, it accelerates the heart's action, produces a glow and warmth of skin with acceleration of respiration—in brief, is a stimulant rather than a sedative, and acts not like morphia—which he calls "the food of the brain and the sustainer of nerve-force"—on the central ganglia, but rather on the spinal and nerve-conducting material. The potency of this alkaloid should ever be kept in mind; one sixtieth of a grain he considers a medium dose for injection. A great peculiarity, he thinks, belongs to it, viz., that "it strikes at the root of disease," as if restoring at once to its normal and healthy state the deranged part—the seat of the malady.

These effects are all supported by cases many of which are very remarkable in their results, relief of suffering so speedily following the application, and this in several instances without a recurrence of pain or morbid action as manifested by symptoms. We give, by way of illustration, of the striking cases detailed, two, one in which morphia was used, the other in which atropine was injected.

"T. B., aged 41, was admitted April 12th, 1859, into St. George's Hospital, under the care of Mr. Tatum. For the first three weeks he suffered acutely; the conjunctiva of both eyes was greatly chemosed, red and tender; for the greater part of this time he *seldom closed his eyes day or night*, notwithstanding leeches, blisters, calomel, and opium, and finally morphia, a quarter of a grain every three hours, were employed. The injection of half a grain of morphia into the arm was then tried; it eased him for some hours, but did not cause sleep. Two days after, the injection was again employed, one grain being used this time. The patient describes the effect of the injection as 'something which instantaneously ran through his frame, round his head, and which seemed to go out of the back of it.' *The pain was gone*, and he slept in about ten minutes. The sleep lasted about six or seven hours. The patient went out about three weeks after the second injection, during which time he had no more pain, and slept well every night."

"A lady, aged 60, consulted me on the 3rd of June last, for sciatica, which had afflicted her constantly for about two years. Rheumatism, and great mental anxiety, had for a long time preceded the sciatica, which caused her to walk lame. For two years she had taken large quantities of morphia, without which she could not sleep, though she became restless and heavy.

"I injected into the arm the thirtieth of a grain of atropine, at 4 p.m. The next day the patient described her symptoms. A red flush had come out all over her body, and she felt very hot, soon after the puncture. She dined at eight o'clock, retired at eleven to bed, and dreamed extraordinary things, and to-day finds the pain gone, and the leg stronger to walk on. The pulse was 80, and the tongue clean. I saw the patient five weeks afterwards. She had 'not had a particle of pain since,' had walked for some hours a few days before, and found the leg stronger every week. It is interesting to remark that the morphia, which she could not be persuaded to leave off, has not nullified the benefit derived from the single dose of injected atropine."

Even in incurable diseases, Mr. Hunter states he has found the hypodermic method of great service. He says—

"In my own hands it has been a boon to many patients for months perhaps together; some with incurable neuralgia, others with *cancer*, or the agony of diseased joints. Life is made bearable by this plan to the poor sufferers."

The following is the scheme of diseases to which the author considers the hypodermic plan of treatment applicable.

The Hypodermic Treatment of Disease.

The injection of medicines into the cellular tissue beneath the skin may be made

- Locally with
  - 1. Caustics for injecting; nævi, aneurisms, &c.
  - 2. Anodynes, as by the narcotic injection of Wood, 1843; Rynd, 1844.
    - Neuralgia (centripetal)
    - Sciatica
    - Local pain
- And generally by hypodermic method of the author, 1858.
  - In cerebral,
    - Sedative / Hypnotic / Nerve tonic
      - Insomnia.
      - Melancholia.
      - Mania
      - „ puerperal.
      - „ a potu.
      - Delirium tremens.
      - Chorea, hysteria.
      - Neuralgia, tic douloureux,
  - spinal,
    - Anodyne / Anti-spasmodic / Hypnotic / Sedative / Tonic
      - Neuralgia, tic douloureux.
      - Severe pain, deep or superficial, as in rheumatism, lumbago, sciatica, &c.
      - Tetanus.
      - Retention of urine, &c.
      - Colic, &c.
      - Eclampsia (puerperal).
      - Epilepsy.
      - Paralysis, certain forms of.
  - and sympathetic nerve cases, affections of the vascular system, and in blood-disease
    - To diminish vascular and inflammation
      - Peritonitis, pericarditis.
      - Enteritis, &c.
      - In ophthalmic surgery
        - Various cases
        - Herr v. Grafe,
    - To give tone and arrest morbid action
      - Fever
        - Ague, &c.
        - Continued fever.
          - Mc Craith.
          - Desvigne, &c.
      - Dysmenorrhæa.
      - Phagedæna.
      - Diarrhœa, vomiting, cholera.
      - Sea-sickness.
    - To palliate incurable disease
      - Cancer, &c.
      - Hydrophobia? intero-susceptio, &c.
  - As an antidote and anæsthetic
    - For opium, strychnia, and other poisons.
    - Also as a diagnostic test, and a therapeutic test.
    - Before, during, and after operation.

We quote, also, his practical conclusions. He introduces them with the remark—

"Leaving for the present the consideration of the *modus operandi* of this plan of treatment, we may justly, from the experiments on animals, but *chiefly* from the cases just reviewed, sum up with the following practical conclusions:

"1. That certain medicines may be introduced into the cellular tissue beneath the skin with safety and advantage.

"2. That medicines so introduced have a *general* as well as a local effect.

"3. That the general effect of medicine so introduced is exceedingly rapid.

"4. That this mode of administration is *more certain in its action* than is the action of a stomachic dose: for the *exact* amount introduced is known, and the whole of it takes effect, which *may* or *may not* be the case with stomachic doses; which may, on the other hand, be retained unabsorbed, vomited, &c.

"5. It is also, and for the same reason, a more trustworthy method for certainty of action of a remedy than are endermic, enepidermic, lingual, and rectal methods.

"6. Medicines are *more purely received* into the system by this method than when given by the stomach, in which organ they may become contaminated or decomposed.

"7. A given amount of medicine employed hypodermically has greater effect upon the system than the *same* amount administered by the stomach.

"8. Medicines are *more rapidly absorbed* into the system when thus administered than by the stomach. The desired effect, therefore, is *more quickly gained.*

"9. A given amount of medicine employed hypodermically has a greater and more rapid distant effect than when employed *endermically*, enepidermically, or iatraleptically.

"10. That the medicines for which this mode of introduction is especially applicable are the various *narcotics* and *sedatives, hypnotics* and *nerve-tonics.*

"11. That this plan of treatment is more especially indicated for the relief of the *affections of the nervous system:—*

"1stly. When the immediate and decided effect of the medicine is required.

"2ndly. Where medicines administered by the usual methods fail to do good.

"3rdly. Where the effect of a medicine is required, and the patient refuses to swallow.

"4thly. Where from irritability of the stomach, or other cause (such as idiosyncrasy, &c.), the patient cannot take the medicine by the stomach.

"12. That to produce a general effect it does not signify whether the remedy be injected into the cellular tissue of the body or of an extremity.

"13. That to relieve or cure a local neuralgic affection there is no necessity to localise the injection.

"14. That whether the object be to treat a local or general affection, it seems advisable each time to change the site for injection, should it be more than once required.

"15. That this mode of introducing medicines is the most accurate one we possess for *testing their true action* upon the system generally.

"16. That *antidotes* to certain poisons can, by this mode, be rapidly introduced into the system."

Of these conclusions, some perhaps may be open to objection and some may require modification. From such experiments as we have made on animals, using the most violent poisons, we are led to infer that it is not quite a matter of indifference where the subcutaneous injection is to be made; we are inclined to believe that the effect will be most rapid and secure the more the part operated on abounds in vessels, and the circulation of the blood in it is least exposed to retardation or obstruction. Mr. Hunter, we trust, will continue the inquiry and elucidate the subject more and more.

We cannot part from him without tendering him our meed of thanks for what he has already accomplished—this, the demonstration of a remarkable method of a new kind not limited to local ailments, likely to effect a great change and a great improvement in the treatment of some of the most severe and distressing diseases to which man is subjected.

REVIEW XI.

Lectures, chiefly Clinical. By THOMAS KING CHAMBERS, M.D. &c., &c. Fourth Edition. *London:* Churchill and Sons, 1865. Pp. 605.

IT is now but two years and nine months since we reviewed the first edition of this book. As it seemed to us then, it might be compared to a lusty young gallant in new attire, quaint and questionable, with much that was pretentious, much of the mock-heroic in his bravery; but yet withal a generous high-spirited clever youngster, full of belief in his mission, full of contempt for old world inconsistencies, fallacies, and failures, having a thorough faith in his own power to set things to right, and to renew the life of the worn-out system medical. Our gallant friend has improved with age. He has laid aside his motley, and now appears in the respectable decent garb of a douce sober citizen. He no longer wounds our professional susceptibilities by the vagaries of his boyhood. He has pruned away the exuberances, and the oddities which annoyed whilst they amused us; and he has developed into the clear-headed, thoughtful, earnest, albeit opinionated teacher; retaining his belief in his own power, and justifying that belief by good work, keen observation, and tempered self-reliance. Of course the identity of the lad is preserved in the man; but there is as much difference between them as there is between the undergraduate fresh from college, who astonishes his mother and sisters with his wonderful airs and harmless fopperies, and the hard-working curate who a few years afterwards is held worthy of canonization by half his parish.

To leave metaphor, however, Dr. Chambers has greatly altered his book, and has altered it for the better. As it at first appeared, by its originality, its thoughtfulness and its earnestness it had large claims to professional favour, but it offended professional taste and alarmed professional prejudice by its title, its appearance, and, if we may use the word without offence, its sensational style. As it is now given to the world, it is a valuable repertory of his individual opinions and bed-side observations, and a model of lively, pointed, and earnest clinical teaching. He has preserved all its better characteristics, expanded its scope and utility by the addition of new matter of value, repeated, as every independent thinker and observer has a right to do, the conclusions at which he has honestly arrived, and has won for himself the high praise in this age of therapeutical Pyrrhonism of being a sober, original, and successful investigator in the art of curing disease, and an eloquent expositor of rational Medicine.

In the short space which we can allot to a second notice we shall not attempt a systematic review of the book, but shall content ourselves with selecting here and there a subject for comment and analysis. We shall thus, we think, give our readers a better idea of our author's merits than by a mere cursory survey of his whole work. We select, to begin with, the lectures on continued fever. Dr. Chambers treats of both typhus and typhoïd under the one generic name of typh-fever. He neither asserts nor denies the identity of the poison producing the two forms, but considers that it is still an open question. Clinically, he thinks it is not necessary to distinguish them, as the treatment he adopts is the same in both. Some of the cases on which the clinical lectures are based are certainly such as would make an observer pause before he asserted the absolute distinction of the two poisons. For instance, Dr. Chambers records the case of a young woman who was admitted into St. Mary's in February, 1863. She had had fever a week. Her bosom and abdomen were thickly covered with dark livid spots, many of them as deep purple as those of purpura hæmorrhagica. Two days after she had been in the hospital an eruption of several pink slightly raised spots was observed on the abdomen, which had all the characteristics of typhoid spots. Again, in the following month a young woman was brought into St. Mary's from a house where her aunt had just died of indubitable typhus, her sister was also ill of the same fever, yet this girl's attack proved to be typhoïd and not typhus. She had rose spots coming out in successive crops, accompanied by pain and tenderness in the right iliac fossa. Such cases, at all events, give colour to the view that, however distinct in typical cases, these fevers may appear, they are to be considered rather as well marked varieties than as distinct species, and justify the author in his assertion that if there are two distinct poisons that cause typh-fever, they must be much nearer allied to one another than those which originate the usual eruptive fevers—such as scarlatina, smallpox, &c.

The author's remarks on the causes of these fevers and the mode in which their poison is introduced, although they contain nothing absolutely novel, are still striking. He admits that the balance of evidence goes to prove that one of the chief exciting causes is a poison generated by decomposing organic matter, and received into the body from without. Although for the production of fever it is not only necessary that there be a poison, but there must be a body apt to be poisoned. But he goes farther, he asserts the probability that the poison may be produced "within the body itself—out of its own substance—idiopathically." We confess to misgivings on this point. We believe that persons may suffer from febricula, may become "typhous," as we see every day after surgical operations and accidents; but we do not believe that typhus or typhoïd fever, with their typical eruptions and chains of symptoms, can arise in a person who has not been exposed to the poisons of those fevers, or to the circumstances under which those poisons are known to originate. There is no more evidence that typhus can arise within the body apart from such external conditions as have been over and over again proved to produce its poison, such as overcrowding and filth, than there is evidence that rinderpest or smallpox may thus arise.

With regard to the mode in which the poison is introduced, Dr. Chambers is inclined to think that it enters by the digestive canal; and he is confirmed in this opinion by his experience of the remarkable power of emetics to cut short the disease in its early stages. This, of course, is not a new observation, but like many old truths it has fallen of late into oblivion. In fact, the use of emetics is pretty generally tabooed by the modern expectant treatment which is just now so fashionable. Dr. Chambers states that he has seen repeated instances where, after the stomach has been emptied by the action of an emetic, the fever has disappeared and convalescence has been immediately entered upon. He records one striking instance of the kind. A lad aged fifteen came into the hospital on September 5, 1860, with hot skin, rigors, excessive muscular languor; pain in the back, limbs, and head of four days duration; gurgling in the right iliac fossa, and rose spots. During the first twenty-four hours after his admission no medicine was administered, but the urine was kept and analysed. In quantity it amounted to 1000 cubic centimetres, its specific gravity was 1·027, the amount of urea was 50·63 grammes, and of uric acid ·43 gramme. An emetic was then administered, and the urine passed during the next twenty-four hours was examined. Its quantity was 530 cubic centimetres, the specific gravity 1·028, the amount of urea was reduced to 29·37 grammes, and there was only a trace of uric acid. The next day the urea was reduced to 14·79 grammes, and the change continued during a rapid and complete convalescence. Such cases really suggest a suspicion, in contravention to the teachings of the fashionable school of the present day, that the practice of our forefathers was not always either useless or harmful.

The lectures on rheumatic fever commence with the curious assertion that "rheumatic fever is a pleasant disease." The author adds, "I mean for the doctor to treat, though not for the patient to bear." We wish we could endorse this statement; but some statistics which occur a little further on, and which we fear represent but too faithfully the power or rather the want of power of the medicinal treatment now in vogue over the disease, lead us to think that if the pleasure referred to be the pleasure of curing, the profession must wait before they can lay claim to its enjoyment. Dr. Chambers states that between June 1851 and Christmas 1863 there have been in St. Mary's, under his care, 257 cases of rheumatic fever. Of these 26 were treated with ʒj of nitre three times a day; 174 were treated with bicarbonate of potash, viz., 141 with ℈j or more every two hours; 33 with a less quantity; 32 were treated during the first year in various other ways; 25 "have had none of these supposed curative drugs, only a little opium when the pain was very severe, and a purgative when the bowels were too costive." The following were the results of these various modes of treatment on the duration of the disease—

"Of the 26 treated with nitre the mean stay in hospital was 40·0 days. Of the 141 treated with ℈j bihoral doses of bicarbonate of potash the mean stay in hospital was 34·3 days. Of the 33 treated with less quantities of the potash the mean stay in hospital was 40.0 days. Of the 25 treated without curative drugs the mean stay in hospital was 27·7 days." (P. 147.)

We fear that if the same experiment were tried and the same statistics kept in other hospitals besides St. Mary's, the results would not be far different. The "pleasure," however, of which the author speaks is partly, he says, derivable from the feeling which the physician has that he can ensure the sufferers against several perils to which the nature of their complaint exposes them. The perils to which he refers are, of course, inflammation of the membranes of the heart, and the great means by which this is to be guarded against is by enveloping the patients in blankets.

"The patient's bed is made in a peculiar fashion. It is a standing order that no linen is to touch the skin. A slight calico shirt or shift may be allowed, but if the patient possess under-clothing only of the prohibited sort they are better naked. Even a linen front to the shirt is dangerous. Sheets are removed, and the body is carefully wrapped up in blankets, which are so arranged as to shut off all accidental draughts from the head. The newest and fluffiest blankets that can be got are used. The bed-clothes being put so, are kept so, and the attendants and students are warned that when the sounds of the heart are listened to, they must not throw off the wrappings, but insert a stethoscope (first warmed), between the folds." (P. 127.)

We fully recognise the importance and excellence of the principle on which this advice is founded, although the "danger" of the linen front of the calico shirt is introduced by the author, we should think, as a pleonasm. Statistics, however, seem to prove that this process of blanketing exercises a most marked and important influence in preventing the occurrence of heart complication. Dr. Chambers' figures appear, indeed, to show that the proportion of cases in which pericarditis occurs is nearly four times less when the disease is treated by bicarbonate of potash than by nitre, but even this good result he is inclined to attribute to the circumstance that nearly all the cases treated by the alkaline method were also subjected to blanketing.

"Up to May, 1855, no difference was made in the bedding of my patients with rheumatic fever from that of others in the ward; but after that date they were ordered to be rolled up in blankets, and no linen was let touch the skin. In nearly every case the orders were strictly obeyed. Of 63, either bedded in sheets or who had wilfully thrown off their blankets, 6 contracted newly pericarditis at least, if not endocarditis as well; 3 had relapses of pericarditis on old cardiac lesions; 1 had endocarditis alone; on the whole 10, or nearly 16 per cent., had inflammation of the heart, and 4 died. Of 184 in blankets none have contracted newly pericarditis: none have died; 1 had a relapse of pericarditis on old cardiac lesions; 5 had endocarditis alone; 1 a relapse of endocarditis on old cardiac lesion. One of these included cases of pericarditis was brought on during convalescence by the patient being dowsed with cold water for an accidental hysteric fit. Not 4 per cent. have had any acute affection of the heart; when it came it was of a milder character, and was generally to be accounted for by some imprudent exposure. That is to say, that bedding in blankets reduces from 16 to 4, or by a good three-quarters, the risk of inflammation of the heart run by patients in rheumatic fever, dimishes the intensity of the inflammation when it does occur, and diminishes still further the danger of death by that or any other lesion; and at the same time it does not protract the convalescence." (Pp. 148, 149.)

If this be so, the best thing to be done with our rheumatic cases would seem to be to wrap them up in blankets and leave them to nature. We are not, however, inclined to accept this conclusion. Dr. Chambers shows clearly that under some medicines the duration of the disease is much shorter than under others. The nitrate of potash was unquestionably an improvement on the calomel and opium of former days; the alkaline treatment is most likely an improvement on that by nitre, and if treatment by medicine has an effect in prolonging the affection it is more than probable that some treatment by medicinal agents may shorten it. What the really effective plan is, has doubtless yet to be discovered. The alkaline method comes recommended by our present knowledge of the pathology of the disease, and Dr. Chambers' statistics show that a very small proportion of cases so treated present signs of acute heart affection. It may be that a more extended series of observations will demonstrate that it has more power of cure than his present statistics show. We hope that a future edition may give us the duration of a series of cases treated by the author conjointly by the blistering plan of Dr. Herbert Davies and the alkaline of Dr. Garrod.

Gonorrhœal rheumatism, which many of us have found a very intractable disease, gets well, it would seem, with tolerable certainty in St. Mary's Hospital. Dr. Chambers holds that it depends upon blood poisoning, the poison being absorbed from the affected mucous membrane. He therefore believes it to be analogous to pyæmia. He traces the analogy in the strong tendency to disorganisation of the affected part—in the disease not exhausting itself by the inflammations which arise—in its chronic character not tending to recovery, but getting worse and worse if not arrested—and in its accidental occurrence, it being an accident of gonorrhœa, just as pyæmia is an accident after wounds. The treatment he adopts is decided—leeching, blistering, and poulticing the affected joints, starvation and large doses of iodide of potassium.

We have said that the author still maintains the opinions which gave character to the former editions of his work. Restorative medicine and the renewal of life are still favourite terms in his work, and he exercises no little talent and ingenuity in making various and opposite modes of treatment illustrate and add weight to his pet theories. But it is only fair to acknowledge that in thus doing he often throws new and unexpected light upon every day methods of cure. The great charm of the book is its originality and suggestiveness. We may not, in fact we often do not agree with him, but we cannot help giving its due mead of praise to his genius and his earnestness. Take, for instance, what he says on the action of blisters in pleurisy as a specimen of the fresh colour he imparts to the most common therapeutics—

"The action of vesicants is first to destroy the epidermis, and to cause the exudation of a fibrinous serum beneath it. Very probably a similar but more remote effect is produced on the neighbouring tissue of the pleural sac. Nevertheless it is not at this stage of the process that the chief benefit accrues. If you watch carefully the line of dulness marking the upper margin of the collection of fluid in the chest, you will find that it falls—not when the blistered skin is full of liquid and is discharging serum—not when the counter irritation may be fairly concluded at its height—but after it is all over. As the sore heals, then the level goes down with the greatest quickness. That is to say that the true use of blisters in such cases is to start a healing process, or a renewed life on the outside skin, in order that it may spread to the neighbouring viscus inside. As long as this influence continues to be exerted, you will gain no time by a recommencement of the process, and your too hurried repetition of blisters would add to the patient's distress, without conducing to his cure. Wait till the effect of one blister has gone off, before you order another." (Pp. 180, 181.)

One of the most original, if somewhat paradoxical, lectures in the book is that on atrophy of muscles, the effect of overwork. The power of nerve over muscle is, according to the author, capable of any amount of increase by education, but the muscular fibres themselves do not admit of proportionate augmentation. When they have been exercised up to their full force they admit of no further improvement. If, then, over-work continues, as in over-training, "the nerve force expends itself in exciting the continuous function of the muscles, destructive assimilation exceeds the constructive—the body 'eats into itself for lack of something else to hew and hack,' and loses weight beyond the mere loss of fat." Some very good cases of atrophy of particular limbs, the result of special employments, are given. Dr. Chambers doubts the popular theory of the production of hypertrophy of voluntary muscles by over-use. The blacksmith's arm and the dancer's leg do not bring him conviction. He allows that the biceps or gastrocnemius will, as a consequence of use, contract more strongly, will be more readily acted upon by the voluntary nerves, and in contraction will be harder and appear more prominent, but he has never been able to satisfy himself that their bulk is increased. The remaining leg of an active man who has had the misfortune to lose a lower limb continues to bear the same proportion to his arms as do those of other individuals. At least such is the result of Dr. Chambers' observations. The arm of the blacksmith, he asserts, only bears the ordinary relative proportion to the legs. Its muscles, when relaxed, are not disproportionately large; they only contract more firmly when the nerve stimulus is applied. He explains the effect of lead poisoning in producing dropped wrist by the exhaustion of the extensors from over work. The absorption of white lead into the blood destroys its red globules, and when carried to the muscles it removes their red colour also, and renders them incapable of contracting. Its paralysing effect on the intestines is due, he thinks, to its early coming in contact with their muscular fibres, and to the fact that the muscular coat of the intestines is "weak" and has constant work to do. (We would ask, parenthetically, however, why, according to this theory, the heart and respiratory muscles should not in turn become paralysed also?) The extensors of the wrist suffer first because they are already disposed to atrophy by the enormous exertion to which painters subject them.

Here we must close our excerpts from the clinical lectures. We had intended to quote or examine the author's opinions on the subjects of pneumonia, heart disease, the action of digitalis, blood-letting, and several other topics. We must devote, however, our small remaining space to the lectures on mucus and pus, which differ from the remaining portion of the book in having been prepared for a more advanced audience than the clinical class at St. Mary's.

The three lectures on the formation of mucus and pus were delivered by the author before the College of Physicians, in 1863. They are an amplification of the views held by Henle, Virchow, Simon, and others, as to the relation between epithelium and mucus, and between the latter and pus. The author sets out with the assertion that the office of a mucous membrane is not to secrete mucus. He says that typical health consists in the absence of mucus; that many robust people pass weeks without expectorating, that others never unfold their pocket-handkerchiefs, and that the urinary and intestinal mucous membranes contribute only an infinitesimal quantity, which may be fairly attributed to a temporary departure from the healthy state of some portion of their surface. This, it may be observed, is a re-echo of the statement made some time ago by M. Simon, who asserts that mucus, as a copious fluid secretion, has no existence in health, and that the only natural secretion of a mucous membrane is its epithelium, which ought not to exist in sufficient quantity to constitute a discharge. We are tempted, however, to ask whether this is true of all mucous membranes? Is it true of the mucous membrane lining the oral cavity? Certain it is that mucous corpuscles are constantly found in the fluids of the mouth of healthy people, and Kölliker has shown that they are the produce of neither the mucous nor salivary glands. That they are excessively common we know. Are they always abnormal, or is it not the office of some mucous membranes to produce in the state of health, and it may be as different, but simultaneous, parts of the same function, epithelium and mucous cells?

About a quarter of a century ago Henle published a paper, in which he traced the so-called mucous globules to the substance which, when fully developed, is epithelium. In other words, the globules of mucus are identical with the nascent epithelial corpuscles as seen in the rete mucosum of the skin, or the inner layer of the epithelium lining the internal surfaces. What, then, determines the destination of the corpuscle? whether it shall grow and take on the characteristics of the epithelium column or cell, or roll off as the abnormal mucous globule? How, again, is it that in disease the production of mucous globules seems so far to exceed the capabilities of the membrane if we accept this theory of their origin? How is it, for instance, that the mucous membrane of the bronchi, with its single layer of epithelium, can produce mucus in such quantities as is seen in catarrh? To these questions Dr. Chambers proposes an answer founded on his observations of the character of the globule and its mode of multiplication. He believes that each globule is formed by the coalescence of granular matter so as to form nuclei, and each nuclear mass becomes multiplied by a double process of budding and splitting. He argues that a rapid multiplication by these methods is not evidence of a high degree of vitality. The more highly vitalised tissues do not so rapidly increase. They lose their power of multiplication in proportion as they become fitted for higher functions.

"When organic matter destined to form part of an animal has attained the end of so becoming a member of a consistent whole, it ceases to multiply itself. Cells do not normally go on splitting up and producing cells similar to themselves *in situ*. The highest development of their vitality is ceasing to exist as growing matter. A fully formed epithelium scale does not produce another scale, nor the nucleus of a muscular fibre another nucleus. The retention of reproductive force is an expression of the lower and an exclusion from the higher functions of life. In the mucous globule, then, we find organic matter, whose destination was the formation of epithelium, arrested in its development when it has attained only the lowest degree of life—that lowest degree of life being the function of reproduction." (P. 47.)

The appearance of pus indicates a further deficiency of vitality. The mucin or transparent medium in which mucous globules float stands to them in the relation of fully formed organic substance or cell. The formation of mucin is, according to Dr. Chambers, the highest development of the life of the globule, for it answers to the formation of tissue from nuclear matter. But as inflammation goes on, pus takes the place of viscid stringy mucus. The nuclear matter cannot form mucin, it can only multiply, and hence its stringiness disappears and it becomes pus. The further deficiency of vitality is shown by its internal self-multiplication and in the non-production of mucin.

Such is a sketch of the foundation of the argument which the author works out in these lectures. We regret that want of space will not allow us to follow him throughout. But we hope that our imperfect notice will lead our readers to study his book. We can promise them it will yield instruction as well as lighter gratification. We concluded our former notice by saying that the book had a heart in it; we will add now that it has both heart and head.

PART SECOND.

Bibliographical Record.

ART. I.—*Lectures on Clinical Medicine.* By A. TROUSSEAU, Professor of Clinical Medicine in the Faculty of Medicine, Paris, &c., &c. Translated and edited with Notes and Appendices, by P. VICTOR BAZIRE, M.D., &c. *London,* 1866. Part I. pp. 276.

THE reputation of Professor Trousseau, as one of the most accomplished physicians and teachers of Paris, has been for some years established in this country. Many practitioners have become familiar with his teaching, and more still with the preface to "Grave's Clinical Medicine," written by him in commendation of the value of that work, and which showed his high appreciation of the labours of British physicians. The reputation heretofore possessed by M. Trousseau will, we have no hesitation in asserting, be largely enhanced by the appearance of his clinical lectures in an English dress. Like those of Dr. Graves, they abound in sterling sense and in good practical lessons, whilst their style is remarkable for clearness. Although by their number and length they constituted two bulky volumes in the French issue, the first edition was speedily exhausted.

This rapid sale in France affords, as Dr. Bazire remarks, "a valuable test of the high estimation in which the work is held there, and of the appreciation by his countrymen of the extraordinary skill with which the gifted author has analysed and digested the immense number of facts which he has been enabled to collect through his long and successful career,"—extending over a period of nearly half a century. The translator has not produced the lectures in the order in which their author published them, but has selected, for this first Part, "those lectures which relate to diseases that are likely to excite the most interest at the present time," the subjects of the last three being new and little discussed in contemporary English medical literature. The liberty thus taken by Dr. Bazire would, as a rule, be unjustifiable on the part of a translator, for it may generally be assumed that an author is the best judge of the order and manner in which he may most satisfactorily unfold his views to the public. And in the present instance we should have been pleased to have learnt that the arrangement now adopted had met with the approval of M. Trousseau and that the translation had been made with his sanction and co-operation.

However, as Trousseau himself did not contemplate a systematic arrangement and discussion of the topics embraced by his lectures, the dislocation of the order he adopted by his translator does not detract from the value of the course as a whole. On the other hand, the collection produced by Dr. Bazire is of unusual importance, and well calculated to induce purchasers—a trade-matter probably not lost sight of in the compiling this first Part.

The subjects selected are;—I. Venesection in Cerebral Hæmorrhage and Apoplexy. II. Apoplectic form of Cerebral Congestion and its relations to Epilepsy and Eclampsia. III. Epilepsy. IV. Epileptiform Neuralgia. V. Glosso-Laryngeal Paralysis. VI. Progressive Locomotor ataxy. VII. Aphasia.

At the present time and whilst the work remains incomplete it is not our intention to examine the medical doctrines of M. Trousseau. Suffice it to observe that experience has taught this most able physician that blood-letting and antiphlogistics are useless and inconvenient in cerebral hæmorrhage and apoplexy, and should give place to supporting treatment by proper food in moderation. The six other lectures are much more full than the first one just now referred to, and treat the subject in a very exhaustive manner, adding much to the routine or regulation descriptions found in systematic treatises, by practical comments and well drawn comparisons of symptoms. To the lecture on Epilepsy a valuable section is appended on the relations of that malady to insanity; a subject of vast importance to every medical practitioner, and also to every lawyer in its medico-legal bearings.

Progressive locomotor ataxy is a topic which will command the attentive study of every medical man, both on account of its recent recognition and of its intrinsic interest and importance. The full recognition of this disease as a special pathological fact, is among the most satisfactory results of modern study, and an encouraging proof of the advance of medical science. However, it is certainly discouraging in reference to therapeutics, to find that a physician like Trousseau, who can so learnedly and lucidly describe its pathology, is unable to indicate any certain measures of treatment. The nitrate of silver, which has of late figured in the brief notices of the disease in English journals, as the appropriate medicine, is discarded by Trousseau as unreliable and useless, and a similar judgment is pronounced upon it by Topinard and by Dr. Bazire, both of whom have largely tried it.

Aphasia is the term now generally substituted for aphemia, to express loss of speech and a condition quite unlike to aphonia. As treated of by M. Trousseau, it constitutes a copious lecture, wherein the much disputed question of the localization of the faculty of speech and also that of the concurrent mental states are largely considered. It is a chapter of unusual interest.

As to the manner in which the translator has executed his task, a very satisfactory verdict may be pronounced. He has, he says, endeavoured to give a faithful translation, without condensation or abridgment.

"Any English practitioner, therefore, not familiar with the French language, may safely consult the present edition, as in every respect the faithful repository of Professor Trousseau's experience and teaching. The text has been studiously preserved throughout, and the additions which I have made, whether original or otherwise, have been kept perfectly separate and distinct."

The additions made occur in the form of foot-notes and of supplementary sections or appendices, and to a great extent consist of notes of cases observed by the translator, narrated in further illustration of Trousseau's teaching. They therefore possess in themselves an independent value.

The lecture on epilepsy is supplemented by six pages of additional matter from Dr. Bazire's pen, respecting the employment of bromide of potassium in that disease, on which topic Trousseau is silent. Again, the notes on the lecture on progressive locomotor ataxy, by the translator, record a complete history of eight cases of the disease with copious comments, in which the experience and conclusions of Topinard (who has written especially on this malady) are brought under notice.

When completed, the publication of Trousseau's lectures will furnish English medical men with one of the best practical treatises on diseases, *as seen at the bedside,* and in this part of the world. The conversational style adopted in lecturing renders the perusal of the work the more pleasing and lends animation to it. The translator deserves credit for having so well preserved the easy and ready style that characterises the original.

A uotice prefixed to this Part informs us that various other nervous maladies (of which a list is given) will constitute the subject matter of the lectures to form the second Part, to appear "early in 1866." We hope this promise of an early appearance of this following section of the work will be strictly kept; for a languid issue of the succeeding parts, at long intervals, would not only be most annoying to purchasers, but also seriously detrimental to the success of the production in a business point of view.

ART. II.—*Rhinoscopy and Laryngoscopy: their value in Practical Medicine.* By S. F. SEMELEDER. Translated from the German by EDWARD T. CASWELL, M.D., with wood-cuts, and two chromo-lithographic plates.—*New York*, 1866. Pp. 191.

THIS is an American translation of two contributions by Semeleder, published in 1862-1863, 'On Laryngoscopy and its value in Practice,' Vienna, 1863; and 'On Rhinoscopy and its value in Practice,' Leipzig, 1862. The translator, Dr. Caswell, remarks in his preface, that American "medical literature offers but very little on these subjects." In combining the two monographs within the compass of a single volume, "he has omitted some repetitions which necessarily occurred in two treatises upon subjects so closely related; others still remain. . . . A few brief notes and an Appendix have been added." This last, which is headed "Translator's Appendix," does not, however, consist of original matter contributed by Dr. Caswell, as might be supposed, but is a reprint from the American 'Medical Times,' of a translation by him of the history of "two cases of extirpation of polypi in the larynx," reported by Semeleder in 1864.

From an examination of this work we are disposed to agree with its translator, that medical practitioners "will find this volume a valuable manual of reference and instruction in the diseases of the throat and of the nose." Its value, however, will be much more appreciated in connection with the practice of rhinoscopy than with that of laryngoscopy. Indeed, we know of no description of rhinoscopy nearly so full and complete as that offered in this monograph by Semeleder. It conveys a good account of the method of practising the art, and illustrates its practical applications by cases and figures. The important subject of the catherisation of the Eustachian tube is largely treated of, with especial reference to rhinoscopy in aural surgery —we say important subject, for, as Semeleder writes,—"If it is true that three quarters of all diseases of the ear proceed from the Eustachian tube, or must be treated through this tube, no one will deny the importance of rhinoscopic investigation to aural surgery."

When we read, in this treatise, that rhinoscopy has "gained no foothold outside of Germany," we must remember that this remark was penned in 1862, and does not apply to England and France at the present time. Yet, without doubt, rhinoscopy has become much less popularised among medical men than the allied art, laryngoscopy; a circumstance attributable to its less engaging character, its more limited application in practice, and the admitted greater difficulty in employing it.

The deficiencies of the work appear when we examine the section on laryngoscopy. They mainly depend upon the comparatively long period (considering the novelty of the art and its rapid development in the hands of numerous practitioners), since Semeleder wrote the monograph translated; and the translator must be pronounced in fault in not having brought the subject to the level of the information prevailing at the present day, by careful foot-notes and appendices. There are, indeed, foot-notes here and there, but they fail altogether to secure the desirable object just specified, and the impression is unavoidable, that Dr. Caswell has not kept himself *au courant* with the literature of laryngoscopy, either in Germany or France, or in England.

Among the more prominent defects may be mentioned the absence of any intelligible account of the treatment of laryngeal ailments by so-called pulverized liquids; the imperfect account of aphonia, and the omission of any notice of its treatment by galvanism; the want of a satisfactory description of the plans and instruments for extirpating tumours of the vocal cords and adjoining parts, &c. Respecting the first-named subject, we have the following confused account, supplemented only by a foot-note, briefly stating that Dr. Gibb "has devised a very simple and useful instrument, a caoutchouc bag attached to a silver or gold tube, curved somewhat like a catheter; at the end of a (*sic*) tube is a platinum capsule with very fine perforations," —a description suggestive of the writer being personally unacquainted with the instrument.

To return to Semeleder's text, as it stands in the translation:—

"Still another method of applying pulverized remedies is the inhalation of liquids, containing a dry fine dust, by means of various well known apparatus which exist in various shapes, from that of Sales Girons down to the simple and well adapted one of Schnitzler. I may take it for granted that these are known to the readers of these pages. After numerous efforts, there is now no longer a doubt that pulverized solutions may in this way be introduced into the larynx and the trachea, even when these organs are diseased. Thus is established the great importance and future usefulness of this method for the local treatment of the diseases which now engage our attention; valuable observations upon this method already lie before us."

The sentence, "the inhalation of *liquids containing a fine dust*," we presume, in the absence of the original German text, to be a mistranslation; for it involves an erroneous meaning, and in fact stands corrected by the subsequent expression, "pulverized solutions." But apart from this ambiguity, the account of the treatment considered in the paragraph quoted, is sadly incomplete and practically useless to the reader, and required to have been elucidated and made complete by the translator.

Abnormal growths of the glottis are treated under the name of "*neoplasms*," and a good account given of their pathology, but the proceedings and instruments for extirpation are, for all practical purposes, unrecorded in the text of the treatise, although indeed in the Appendix we are presented with the history of two operations by Semeleder, performed in the autumn of 1863, and with an account of the surgical appliances he used. In his first case, Semeleder succeeded after two sittings, and in the second, after four, leaving, however, the stump of the growth unremoved. His first attempt was made with Voltolini's guillotine, but this instrument failed in the object sought, and he subsequently resorted to some ingenious contrivances invented by himself and Leiter, the surgical instrument maker of Berlin, which are figured and described in the Appendix.

But after allowing its full merit to the Appendix, there still exists an insufficient account and description of the operative proceedings for extirpating morbid laryngeal growths.

The translated text is for the most part clear and intelligible. Nevertheless it is, in places confused, obscure, and ungrammatical, and odd words are introduced. The German word *orientiren,* is translated *to orient,* an expression which will convey no intelligible meaning to readers generally, the noun orient, in English, having no direct relation to the German word, which means to search for, or to discover one's position. It occurs in several places, and among others, at page 58, where we are told that "the representation . . . . is remarkable from (?) the numerous alterations which have taken place in the structures, and which may, perhaps, make it difficult for many to *orient* themselves." Another word of a pedantic sort is introduced in the following paragraph, respecting ulcerations in the naso-pharyngeal space:—"An exact diagnosis in each individual case is only made possible by the *anamnesis,* and by the consideration of its peculiar condition." And again we are instructed that in cauterizing the larynx, "it is not advisable to use very strong solutions at first, and never to cauterize without having had previous diligent practice upon the cadaver or the phantom." (P. 120.) Other odd phrases occur, such as a perforation of the velum, "as large as a copper," and many sentences in which the German idiom maintains its ground, though the words are English. A small amount of care and reflection on the part of the translator would have avoided these blemishes in a work possessing such excellent claims upon our attention.

ART. III.—1. *Parturition and its Difficulties.* By JOHN HALL DAVIS, M.D. Second edition. *London;* 1865.

2. *A Handbook of Obstetric Operations.* By W. S. PLAYFAIR, M.D. *London;* 1865.

3. *On Flooding after Delivery.* By LUMLEY EARLE, M.D. *London;* 1865.

WE have before remarked upon the very large amount of attention given of late years to the subject of obstetric medicine and surgery, and the numerous works published on that department of our profession. New books continue to be sent to us, each containing something valuable, if not novel, although we might almost suppose the subject exhausted. The first of those enumerated above is known to the profession, and is a very useful and readable book. The first section gives an account of the causes, and principles to be observed in the management of powerless and obstructed labours; the second part comprises the history of 153 labours, presenting various degrees and kinds of difficulty; and the third consists of the statistics and analysis of 13,783 deliveries attended under the direction of the author from 1842 to 1864, chiefly in the Royal Maternity Charity. It would be impossible to render the author's views of the various matters treated of in a more succinct and clear manner than he has done, and the best recommendation we can give is to read the work itself. Its perusal will not give much trouble, and the time devoted to it will be amply repaid.

Dr. Playfair's little work is a valuable addition to obstetric literature, and will be read, we have no doubt, by all students. It is in reality a handbook, and contains a clearly written account of the history and management of the various operations required during childbirth. These are described as briefly as possible, compatible with distinctness; and to each chapter is added a summary, which helps to impress it upon the minds of those who read the work. Indeed, it would be useful for students to commit these summaries to memory, for they are in most instances correct, and to be depended upon in practice. In the chapter on the induction of premature labour the method of operating by separating the membranes from the uterine parietes by a gum-elastic catheter or catgut bougie is spoken of as excellent, easy, and painless in application, and safe both to the mother and child. This plan was recommended by Dr. Copeman, of Norwich, in a work published in 1856, entitled 'Records of Obstetric Consultation Practice,' where he says the operation is easy to accomplish, and not painful to the patient, and when labour comes on it cannot be doubted that the fact of the membranes being entire during the first stage materially facilitates the progress of delivery, and contributes in no slight degree to the safety and preservation of the child. The vectis is an instrument not much used at the present day in London, and in the works of obstetric authors is generally either not mentioned at all, or spoken of as an instrument of very little importance. Dr. Playfair speaks of it more at large, but does not describe its real uses and advantages as if he were familiar with its employment. He also, in our opinion, exaggerates its dangers, and in comparing it with a single blade of the forceps deprives it of almost all its value. In the work above referred to, Dr. Copeman gives a plate of a properly formed vectis, and describes its curves and dimensions. It is essentially a *tractor*, and a rectifier of position, and ought never to be used as a lever of the first order; and it would seem from the cases recorded that it is not only adapted to those in which the short forceps are available, but also to such as would otherwise require the long forceps, and even to some in which no forceps could be used.

The last work named at the head of this article is on a subject that very fitly follows the consideration of obstetric operations—namely, flooding after delivery. It is a small neat volume of 238 pages, written by Lumley Earle, M.D., of Birmingham, and although it contains little or nothing original, the author's observations are founded on the result of considerable experience, and deserve attention, particularly those which refer to the *preventive* treatment of flooding after delivery. The various causes of flooding after labour have been considered separately, with their diagnoses and special treatment, and in most instances illustrated by cases. It will no doubt prove a useful book to students and young practitioners, and the author is probably not far wrong in his assertion that, if they desire to reduce the frequency of this dangerous complication to a minimum in their practice, they will succeed by following out the preventive treatment he has so fully described and recommended. The book contains many good practical remarks, but it will be better to read it than to analyse minutely its contents. We, however, quote the following observations as to the propriety of introducing the hand into the uterus in cases of flooding: "In a very bad case, when the patient is exceedingly low, it may be a question as to whether the hand should be passed into the uterus before the patient has somewhat rallied. The cases are quite exceptional in which it would be well to wait. If a great loss of blood is going on, we cannot expect that the patient's condition will improve, but just the reverse, and if the hand is to be passed at all, the sooner it is done the better. During the operation, in these cases of great depression, brandy must be poured down the patient's throat undiluted with water. The introduction of the hand, and especially whatever it has to do in the uterus, should be done gently and deliberately."

ART. IV.—*Observations on the Laws referring to Child-murder and Criminal Abortion, with suggestions for their Amendment.* By GEORGE GREAVES, Consulting Surgeon, Chorlton Union Hospital, &c. *Manchester*, 1864. Pp. 26.[1]

THE object of this paper, which is to bring the law into greater accordance with the principles of morality, of equity, and of science, "a terror to evil-doers"—which it hardly is at present —is assuredly excellent, and deserving of all praise.

The law of England, it would appear now, is that an unborn infant is not a living being, and that it is no breach of the laws of morality to deprive it of the life which it apparently (really) but not legally possesses.

The author, by reference to history, shows that from the beginning of the Christian era there has been a gradual deterioration of public sentiment, and of legal opinion and practice in reference to the crimes in question, until the present century, when the teaching of our law has become all but identical with that of pagan philosophers and law-makers; with, as there is too much reason to believe, great increase in criminal destruction of infant-life.

He explains, and, as we think, in a satisfactory manner, the circumstances which have led to this deleterious change, the chief of which are an erroneous physiology and a false humanity.

The reform he proposes and explains has for its basis—

[1] Reprinted from the 'Transactions of the Manchester Statistical Society.'

"The recognition of the fact that, from the commencement of pregnancy, the embryo, or fœtus, is a distinct living being. The declaration by Act of Parliament of a scientific truth is probably unusual; but it might be done implicitly.[1] The statute might declare that, if any drug should be administered to any woman being, or believed to be, great or quick with child, with the intent to cause the death of the said child, or to cause its premature birth, by which its life would be endangered; or to prevent its being born alive; or if any such woman should, with like intent, take any drug; or if, with like intent, any instrument should be used, either by the woman herself, or by any other person; or if, with like intent, any other thing should be done, either by the woman herself, or by any other person or persons, he, she, or they should be declared to be guilty of an attempt to commit murder, which is already a felony, involving the highest degree of secondary punishment.

"By another statute it might be ordered that, if a woman, during the whole period of her pregnancy, should conceal her condition, and should not when in labour call for help, and her child should be born dead, or die shortly after its birth, she should be guilty of felony; or if she should, at the last moment, call for help, but no efficient help should be at hand, and the child in consequence die, she should be guilty of felony, but the punishment might be mitigated.

"A third statute might declare that if a child recently born were found dead, with wounds or other marks of violent injury upon its body, it should not be necessary, in order to substantiate a charge of murder, to prove that the child was born alive, but only that it was living when the said wounds or injuries were inflicted, and that they were the probable cause of its death, and that they were such as could not naturally or accidentally be caused in the act of birth; or if it should not be proved that the child's death had been wilfully caused, but had been produced by criminal negligence, it might be lawful for the jury to find the woman or her accomplices, if she had any, guilty of manslaughter; or a woman acquitted on the charge of child-murder, might, if it should appear so in evidence, be found guilty of concealment of pregnancy."

He well says, in conclusion, that "The apathy, now almost universally evinced to the wide-spread destruction of infantile and fœtal life [proved by the vast number of acquittals], is degrading to our character as a Christian nation." Let us hope that his appeal will not be made in vain, and that since the imperfection of the law has been acknowledged by so high a legal authority as Mr. Stephens, measures will be taken to remove its great and glaring defects.

[1] It is done we may remark in the New Salmon Act.

ART. V.—*Address to the Sub-section of Physiology of the British Association. Birmingham*, 1865. Delivered by the President, HENRY W. ACLAND, M.D., LL.D., F.R.S.

THIS able, well-reasoned, and thoughtful address which we had the pleasure to hear, and to hear applauded, was deserving of an ampler audience than was present at its delivery.

As pointing out in well and forcibly expressed words the ends of biological science, its uses in connection with medicine, the hindrances which it has to encounter, partly from the intrinsic difficulty of the subject, partly from the prejudices of mankind —the author has performed a task fully entitling him to our thanks. With this feeling we venture to express the hope, that in its published form the address will have a wide circulation amongst medical men. Coming from an Oxford professor we consider it, in the liberal and enlarged views it takes of science, as of good omen in relation to the future of that University. It was at Oxford that physiology was first successfully cultivated in this country. It was there in troublesome times that the Royal Society, in its infancy, found a resting-place. Its new museum, we would fain hope, is a sign that the natural sciences are again to be duly appreciated and cultivated there, and that the statues of the eminent men of science which impart an interest to its enclosure, have not been placed there in vain.

ART. VI.—*On Hospital Dietaries.* By JOHN BEDDOE, B.A., M.D., &c. *Dublin*, 1865. Pp. 28.

WITH the advances of science, it is interesting to see how common subjects connected with our ordinary wants are attracting attention, and are becoming matters of minute investigation. Dietaries amongst these have a prominent place, and certainly very deservedly, health and life depending on sustaining food duly administered, and death and disease as certainly following its total abstraction, or its abuse, whether in great and continued excess or in great and continued deficiency.

Of all dietaries, the most difficult to form on sound principles are hospital dietaries, the requirements from the description of those to be fed, patients labouring under various diseases and in different stages, being so various and perplexing.

Dr. Beddoe, in this paper, enters on the subject with a just appreciation of its many difficulties, and discusses it with much ability. Three considerations, he justly remarks, must be had regard to in forming diet-scales—the benefit of the patients, economy and facility of administration.

In relation to these requirements, he passes in review the dietaries of some of the principal hospitals, lunatic asylums, workhouses, and prisons of the United Kingdom, including in a tabulated form the particulars of each. The information thus afforded imparts a special value to his pamphlet, enhanced as it is by a standard table of the composition of food and by many judicious remarks. In treating of the value of diet, of the proportion as to quantity and kind most appropriate in the formation of hospital dietaries, he well observes—

"The greatest difficulties arise from the as yet rudimentary state of physiology and animal chemistry, from our imperfect knowledge of the details of the processes of digestion and sanguification, and still more of those of destructive metamorphosis. How little (he asks) do we as yet positively know, however much we may conjecture, of the forces which regulate the amount of destructive changes that go on in the human body; of the sources of urea, uric acid and kreatine, and the circumstances which determine their amount, and which define the quantities of nutriment required to trim the balances of bodily weight, of muscular and nervous power?"

Adding—

"In our present ignorance or uncertainty about these things, it is difficult, if not impossible, to explain the enormous discrepancies which occur in the consumption of food by different individuals."

This quotation we make to show the spirit in which the subject is discussed. Were we to offer comments, more space would be required than we have at our disposal, indeed, even more than the author has taken would be needed to bring it fairly and fully before our readers.

ART. VII.—*Handbook for Yellow Fever: describing its Pathology and Treatment, as observed in uninterrupted practice during half a century. To which is adjoined a brief history of Pestilential Cholera and a method of cure, as detailed in a series of official Reports made during the prevalence of the Epidemic in the Island of Trinidad in* 1854. By THOMAS ANDERSON, M.D., &c., &c. *London,* 1866. Pp. 80.

THE author of this little work seems, from the concluding part of his Preface, to have anticipated that it will not be highly appreciated by his *confrères*, or, as he calls them, "medicos."

From a title of so much promise, we had expected to have derived from the perusal of its contents a good deal of useful information, but we have been disappointed. Dr. Anderson's experience both of yellow fever and of cholera has been more limited than might be inferred from his announcement of an uninterrupted practice of half a century.

According to him, the treatment which he employed in both diseases was eminently successful. In yellow fever it consisted chiefly in the administration of an ipecacuanha-emetic in the first instance, followed by five-grain doses of sulphate of quinine repeated every third hour during the first twenty-four hours, and succeeded by saline medicines, such as the acetate of ammonia, with a cautious allowance of wine and porter and a mild farinaceous diet. He deprecates blood-letting in the disease, and large doses of calomel and quinine.

In the treatment of cholera he trusted chiefly to ammonia, indeed, he considers the volatile alkali as much a specific in this disease as quinine is in ague.

We abstain from expressing any opinion respecting either of these modes of encountering diseases such as yellow fever and cholera—diseases which have hitherto baffled, may we not say? the universal skill of the profession, every vaunted mode proving on extended and fair trial, a failure. We need hardly remark that it is only by a careful comparison of different modes of treatment of the same epidemic disease that any satisfactory conclusion can be arrived at relative to superior efficacy; and such a comparison has not been attempted by our author: he does, indeed, state numerically his own success in yellow fever, that surprisingly great—in one epidemic having lost 1 only of 43, but he makes no mention of the results obtained by other practitioners.

The book, small as it is, owes its volume in no small part to official reports, testimonials, and notes; one note we quote for the amusement of our readers, yet given seriously by Dr. Anderson in support of his specific for cholera, but with too little regard for grammatical construction—

"It is curious, with respect to cholera and ammonia, that our Chinese residents seem to be aware of its prophylactic powers; certain it is that few of them died during the epidemic, in consequence of having been previously habitué. The ravages of smallpox among a tribe of Indians show the effect of a new disease on a primitive race of people, sometimes all but exterminating it, and I was informed that from experience derived from their own country, they were in the habit of setting aside their urine until it decomposed and formed ammonia, and then drank it. Sleeping among bags of onions, I have had occasion to observe, from the ammoniacal exhalations evolved, is also a preventive."

ART. VIII.—1. *Report on the Sanitary condition of the City of Edinburgh, with relative Appendices, &c.* By HENRY D. LITTLEJOHN, M.D., F.R.C.S.E., Medical Officer of Health for the City. *Edinburgh*, 1865. Pp. 72.

2. *On the Cleansing Operations of Edinburgh, as compared with other Towns.* By H. D. LITTLEJOHN, M.D., &c. *Edinburgh*, 1865. Pp. 12.

3. *Report on the Causes of Death among the Assured in the Scottish Equitable Life Assurance Society from* 1831 *to* 1864, *being a period of thirty-three years.* Submitted to the Board of Directors by WILLIAM ROBERTSON, M.D., F.R.S.E. 1865. Pp. 47.

4. *On the Scientific Investigation of Disease in Animals and Man.* By a Fellow of the Royal Society and of the Royal College of Physicians. *London*, 1865. Pp. 15.

THE publications the titles of which we have placed at the head of this article we have brought together, as they all bear on the same subject—the health of the people and its disturbing causes.

In Dr. Littlejohn's very able and judicious report we have an example of a town which has never been regarded as unhealthy, and yet, as appears from his statements, is in an unsound sanitary state, always endangering the public health. The circumstances chiefly productive of this perilous state, such as are of a remediable kind, are not peculiar to Edinburgh, they are nearly the same as prevail in all our large towns, more or less, and even in most of our villages. We shall notice some of those which are most notable and serious in their consequences.

Crowded dwellings may well stand at the head of them. Though Edinburgh, taken as a whole, little exceeds London in density of population, yet parts of it, parts of the Old Town, with its lofty buildings, consisting of many flats—"lands"—some even of as many as seven, each room occupied by a family, afford striking instances of over-crowding, with all the ordinary accompaniments of poverty, filth, and vice. It is in these quarters that the proclivity to disease is most apparent, that epidemics commonly originate and from whence, when of a contagious character, spread, as from a focus, invading often the dwellings of the well-to-do inhabitants and wealthy, occasionally not sparing even the best parts of the New Town, so distinguished for its spacious streets and squares, for beauty of construction as well as site. Where, moreover, there is this undue density of population, there, as might be expected, the tendency to resist morbid action generally is least, the death-rate is highest, and the people altogether are in a low, degraded, and wretched condition.

Next to over-crowding, as most deserving of notice, may be mentioned a neglected or imperfect drainage, with inadequate means of sewage—unsanitary conditions which pervade more or less the city generally, and even some of the best parts of the New Town. On this subject the author gives some interesting particulars, showing how the system of water-closets, unless they are well constructed and rendered safe by trapping against the ascent of noxious gases, become insidious sources of disease. For this reason he very properly deprecates their introduction, wherever the necessary precautions cannot be taken and enforced to prevent their becoming, as in many instances it would appear they are, most noxious and disgusting nuisances.

Cesspools are another sanitary evil against which the author raises his voice. The details he gives respecting them are very instructing; when not well made and rendered water-tight—and in too many instances their construction is shown to be altogether defective—their contents, by exudation and percolation, poison, as it were, the ground adjoining and taint the air by their exhalations.

Fortunately for Edinburgh, in relation to salubrity, it has a good supply of water of excellent quality, not from wells and pumps, but brought from a distance, and consequently exempt from the deterioration to which it would be liable otherwise—from defective drainage and sewerage. Another circumstance tending to check the evils arising from the sources already adverted to is the excellent plan of cleansing the streets, of which Dr. Littlejohn has given a particular account in a paper read before the Social Science Association in 1863, the title of which is given in our heading; a plan which, whilst it secures the object intended—the speedy removal of dirt and all that is offensive from the streets—is, by the sale of what is daily or twice a day collected by an efficient set of scavengers under the superintendence of an inspector, rendered a source of revenue to the town, and of benefit to the country. The manure thus obtained is readily purchased by the farmers of the adjoining districts, and has yielded an income of £7000 per annum.

We have thus noticed a few of the circumstances exercising an influence on the health of Edinburgh. Very many more are pointed out, and amply and judiciously discussed by Dr. Littlejohn in his report, which we do not think it necessary to particularise; yet, all of these are more or less important, and teaching by example—that best mode of teaching—are deserving the attention of all persons interested in sanitary measures, and have an influential voice in carring them into effect.

Dr. Littlejohn, besides describing the unsanitary conditions which Edinburgh labours under, points out plans by which they may in a great measure be corrected. He calculates that were the suggestions which he makes acted on, the highest death-rate of the most unhealthy districts of the city should be reduced from 37·1 (the highest of all) to 25 per 1000; and of the city generally from 25·4, which it is at present, to 23·2—a reduction equivalent to the saving of 312 lives annually out of a population of 170,444, as recorded in the census of 1861. But this saving of life is probably much underrated, taking into account that were the causes of disease as much as possible diminished by sanitary improvements, epidemics would seldomer occur, and if occurring, would be more limited in their spread and less fatal.

Appended to the report, and enhancing its value, are copious statistical Tables and an ample Index, and prefixed is a Plan of the city divided into districts.

Dr. W. Robertson's report will prove interesting chiefly to professional actuaries. It is drawn up apparently with all the caution that the subject requires. Incidentally it is instructive in certain diseases, thus rendering it deserving of the attention of medical men. For instance—to give an example—it has been asserted by a high medical authority, Dr. Christison, that the tubercular and cancerous dyscrasiæ show a marked degree of equivalence; Dr. Robertson, testing this view by his experience, finds it unsupported, inasmuch as in fifty-four cases of cancer, all but four were "fair," and for the most part "highly eligible" lives untainted with hereditary disease of any kind.

Dr. Robertson makes some good remarks on the nomenclature employed in the classification of diseases commonly in use in "life tables;" these, as he shows, affording scope for great improvement provided that they are made more in accordance with our advance in pathological knowledge.

The paper 'On the Scientific Investigation of Diseases in Animals and Man' is well-timed, and is worthy of a Fellow of the Royal Society and of the Royal College of Physicians. The writer takes advantage of his anonymous authorship to tell us some important truths after the Socratic manner—of our ignorance, for instance, of the nature of disease and of our guess-work treatments of disease—of the necessity for the elucidation of the one and a better method for the management of the other, of careful scientific inquiry, conducted solely with a regard to truth and from a love of knowledge; and this, even though uncertain of successful results or of any certain reward, yet not without the cheering hope of the former, and the security, which is certain—that honest labour in scientific research, from the pleasure accompanying it and its beneficial influence on the mind of the individual inquirer is always compensating. He adverts to the many problems that stand in need of such research, and suggests how worthy it would be of our Government were they whilst expending fabulous sums of the public revenue in an endeavour to improve weapons of destruction, to allow a few thousand pounds to be applied in aid of investigations for the benefit of man and the preservation of health and life.

In Dr. Littlejohn's report many subjects are intimated in which such careful scientific inquiry might prove of great service —such as the state of the water of Leith and that of the irrigated meadows in the neighbourhood of Edinburgh. The former from an unpleasant smell emanating from it supposed to be very unwholesome, is having some £40,000 expended on its improvements, though on careful inquiry the mortality of the districts nearest its banks, compared with that of the other districts of the city, is lowest of all; the latter, on the contrary, left permanently neglected, in the uncertainty whether they are noxious or not, the prevailing belief being that the effluvia arising from them exercise in some manner an injurious effect on those parts of the town nearest to them.

The words in which this able writer concludes his paper are well deserving of the attention of the public—

> "There is nothing more certain than that very much more respecting the nature of disease will be discovered than seems possible at the present time. The only question for the public to determine is, whether such scientific investigations shall be urged on a rate commensurate with the enormous power of means and work at disposal in this country, or left entirely to the very few who can pursue them unaided. The investigation may be encouraged by our government, or we may permit the less wealthy governments of the smaller Continental states to employ the scientific labour of which we shall certainly, as well as they, receive the fruits."

ART. IX.—*A Practical Treatise on Rupture: its Causes, Management, and Cure, and the various mechanical contrivances employed for its relief.* By T. P. SALT, Birmingham, Anatomical, Surgical, and Orthopædic Mechanician, &c., &c. *London:* 2nd Ed. 1865. Pp. 112.

THIS book is an instance of a class of publications which are daily becoming more common, and which require to be looked upon with much caution; we mean books professedly treating of medical subjects, but which are written by tradesmen for circulation amongst their customers and the general public, with the object of selling some patented or other invention. The nature of such works requires that they should be treated as advertisements, and their statements trusted only so far as they are supported by proof. They are, however, as we are free to confess, open and legitimate methods of advertisement to which, in this age of advertising, it would be absurd to object. The case is very different from that of a medical man who should patent some invention and then write a treatise on some medical subject with a view of selling his patent article. Yet such instances, we grieve to say, are not far to seek. Mr. Salt's book is, at any rate, fair and above board. Its object is to recommend "Salt's Patent Orthonemic Truss." Of the merits of this instrument we will express no opinion, inasmuch as we have never tested it in practice, and therefore should have no right to decry it; while, if we were to express any favorable impression of its principle or construction, we should be straightway added to the 112 testimonials with which the present work terminates, and swell, in a third edition, the gentle gale which is to puff Mr. Salt's invention into general use. Besides its recommendation of the "Orthonemic Truss," the only point which we see calling for remark in the volume is, that Mr. Salt has been led by his experience as a bandagist to the conclusion that a double truss is always necessary in every case of single rupture; and, therefore, one of the features in which his apparatus differs from those in common use is, that it commands both rings. The following observation of Mr. Salt is worth bearing in mind—

"It is very common for persons with a single rupture, for which they have worn a single truss, to become, at some future time, the subjects of double rupture; but I have never known a patient with single rupture who, from the first, has worn a double truss, becoming the subject of double rupture, and I have no doubt my own experience in this matter is borne out by that of others." (P. 53.)

A few other matters are treated of in this volume (as prolapsus uteri, and ani), but with the same view, viz., to recommend particular modifications of the apparatus in common use. Practical experience can alone decide whether Mr. Salt's inventions have any real superiority.

ART. X.—*On the Treatment of Enlarged Tonsils, at any period of life, without the operation of Excision.* By WILLIAM J. SMITH, M.B., London. Surgeon to the Islington Dispensary, &c. *London:* Hardwicke, 1865. Pp. 55.

THIS is the first of a series of pamphlets on the subject of the anatomy, physiology, and diseases of the tonsils, which Dr. Smith proposes to publish. The present part treats of the common chronic enlargement of the tonsil. It appears both strange and inconvenient that Dr. Smith should have commenced with the pathology of the gland, before publishing his views on its physiology; for in the present part he has frequent occasion to refer to our ignorance of its functions, and to certain views which his own researches have led him to form on the subject; but as to the nature of these views he gives us no hint, and we are therefore often at a loss to assign their due importance to the pathological deductions founded on them. Apart from these views, whatever they may be, there is only one matter in Dr. Smith's present publication that appears to differ from the doctrines usually taught on the subject of enlarged tonsils—we mean, his preference for extirpating the superfluous portion of the enlarged gland, when necessary, with potassa fusa, instead of with the knife or guillotine. If this suggestion is found to be practically successful, it will be a considerable advance in the treatment of this disease in childhood. Simple as the operation of removing the tonsil looks, and is, when the patient is willing and steady, few things are more hopeless than the attempt becomes when the patient (an obstinate and terrified child) is bent on resistance. In such cases the use of chloroform becomes necessary, if the operation is to be done at all; but no one likes to use chloroform, if it can be avoided, for operations in the throat, where any considerable bleeding is possible. Hence, in such cases, the disease is very often left to take its course. Happily the course of the disease is generally to ultimate recovery, but there are many cases in which the removal of the projecting portions of the glands would materially improve the patient's condition, and in which the surgeon would remove them if he could do so without difficulty or danger, though he may not think it worth much risk or trouble. In such cases, Dr. Smith's plan may be found advantageous. He used at first to apply the caustic simply by means of a pair of forceps, but has since employed an instrument, of which he gives a figure, and by means of which the potassa fusa may be quickly and effectually brought into contact with the whole surface of the gland. There is no danger, according to him, of the melted caustic running into the larynx, nor is the pain and soreness so great as might be expected. The number of applications required appears usually to be about seven or eight, and the process may be repeated about two or three times a week. Dr. Smith allows that it is impossible to avoid the uvula being "acted on;" and the whole description seems to imply that the treatment is a far more severe business than the simple removal of the projecting portion with the knife or guillotine. As far as our own experience (a tolerably extensive one) has gone, this latter operation is safe, both in children and adults, if the patient is moderately steady, and the usual precautions are observed; and we should certainly give it the preference over the destruction of the same piece of tissue by repeated processes of cauterization and sloughing, however safe and effectual such processes might be. But for the intractable cases spoken of above, or for adults who might be cut with safety, but will not give their consent, Dr. Smith's plan may prove very useful, and we think a fair trial should be given to it.

Dr. Smith speaks disparagingly of the action of the guillotine in the removal of the tonsil, and no doubt when the latter is much buried behind the anterior pillar of the fauces, it may be difficult to make use of that instrument; but when the ring can be passed fairly over the enlarged gland, the sliding French guillotine, with a double harpoon that rises as it passes onward, appears to us a most efficient instrument, and we can testify to having used it very often with great success and comfort.

Dr. Smith also calls attention to the fact, that the enlarged tonsils sometimes entirely block up the posterior nares, and that if chloroform is given in such cases, and the jaws become spasmodically closed, asphyxia may be produced.

We must not extend this notice by following Dr. Smith through the other parts of his subject,—as the cases in which removal of the tonsils is indicated, the relation between enlarged tonsils and deafness, &c. They appear to us carefully and well handled, and the publication altogether very creditable to its author.

Art. XI.—*Outlines of Elementary Botany.* By Alexander Silver, M.A., M.D., &c. &c. *London,* Renshaw, royal 32mo, pp. 380.

In an elementary book of this kind originality is not to be looked for. All that we can in justice expect from the author, is a conscientious and careful selection from the best works and memoirs on the subject, so that the student may be put in possession of the salient points of the science. Dr. Silver seems fairly to have succeeded in this object, and his little book is likely to be very useful to the medical student. The introduction of the list of officinal roots, leaves, &c., under the chapters devoted to those organs, is a useful and valuable feature, as it will serve to give increased interest in the subject to the pupil. The account of the natural orders is given in a very concise form, and one which will, we fear not, be very attractive to the reader. A better plan would have been to have given merely a few of the more prominent and important orders, and treated them at greater length; as it is, very many of the families mentioned by Dr. Silver might be better omitted in such a case as this.

We miss, also, any instructions to the pupil in the art of describing plants accurately and tersely, which we the more regret, as it is a point now much insisted on by examiners, and deservedly so, for no plan is better adapted to promote habits of careful observation, or to prevent the evils of cramming. A man well practised in this department is not likely to overlook important symptoms of disease, or to assign undue value to phenomena which though prominent are not necessarily of primary consequence. We do not doubt that Dr. Silver's book will be a favourite with medical students, and therefore hope to see in the future editions some such improvements as we have suggested.

ART. XII.—*Chemistry for Students.* By A. W. WILLIAMSON, F.R.S. *Oxford:* at the Clarendon Press, 1865, pp. xvi, 387.

WE have read this book with care, and are obliged to confess that we are in some measure disappointed with it. It commences quite abruptly with an account of oxygen, and we are sure that the descriptive details will prove unintelligible to the beginner. Symbols and atomic weights are given without a word of explanation, while allusions to such substances as manganic peroxide and pyrogallic acid occur long before the reader is in a position to form any idea of what these expressions mean. But in justice to the author we are bound to say, that the explanations, when given, are clear and to the point, and that the most "interesting and useful facts" pertaining to the science of chemistry, and the most "important ideas" derived from these facts, are succinctly recorded.

The range of subjects treated of by Professor Williamson is well seen in the table of contents prefixed to the volume. Advantage is taken of the descriptions of the non-metals and metals, to interpolate here and there a few lucid remarks on the fundamental laws of chemistry and chemical physics. Some well devised and appropriate problems are appended to the earlier chapters of the work. About one third of the book is assigned to organic chemistry.

Allusion was made, at the beginning of this notice, to what we consider the chief defect of the 'Chemistry for Students.'

The want of a suitable introductory explanation is a serious fault, which it may be rather difficult to remedy, but there are several minor defects which may be removed with the greatest ease. The woodcuts, for instance, are quite unworthy of the Clarendon Press (see the figure in page 50). Shading and high finish are not requisite, but the bad drawing, the awkwardness, and the disproportion shown in several of the illustrations, would not be tolerated even in an instrument maker's catalogue.

The other easily remediable defects in the book might have been avoided by more careful correction of the proofs. Ordinary typographical errors, for example, are far too numerous. On page 40, the formula HTO_4 occurs twice over, instead of H_2SO_4; on page 103, we find the expression, $Ba(ClO_3)^2$, instead of $(Ba\,ClO_3)^2$, and there are many other similar misprints. Then, too, vagaries and inconsistencies in the spelling of technical and scientific words are constantly recurring. We read of centi*meters* and millime*ters*, yet we find also the words lit*res* and met*res*. On page 45 we are introduced to the cryophor*o*us, and while all the other chlorates are written ClO_3 M, barytic chlorate is $BaClO_3$.

ART. XIII.—*The Surgery of the Rectum: being the Lettsomian Lectures on Surgery, delivered before the Medical Society of London*, 1865. By HENRY SMITH, F.R.C.S., &c. *Churchill*, 1865. Pp. 127.

THIS is, we believe, the latest publication by Mr. H. Smith upon a subject on which he is constantly before the public; and therefore it is only necessary to direct our readers' attention to what is novel in the work before us, as contrasted with Mr. Smith's other works, which are no doubt familiar to most surgeons. The chief point in the present lectures is the warm recommendation of a new method of operation for hæmorrhoids, viz., to remove them by means of the clamp and cautery, instead of by the ligature. The plan is as follows: The piles are successively seized and tightly compressed by means of a clamp, which secures temporarily all the vessels at the base of the tumour. The latter is then shaved off, leaving the cut surface of its base between the blades of the clamp. Then the cautery is passed over this cut surface till all the vessels are believed to be thoroughly seared, when the clamp is relaxed. After all the piles are thus removed the parts are returned within the anus, and the operation is completed. The advantages which this method is said to possess over the ligature are—1, that it is safer; 2, that it is quicker; 3, that it is more free from pain and risk of long-continued ulceration after the operation.

Before inquiring what may be the truth of these alleged advantages, the previous inquiry must, of course, be whether the operation can be safely performed. Every one knows that fatal disasters occasionally followed the immediate excision of piles, as it used to be practised, in consequence of hæmorrhage after the return of the parts within the bowel. Is it not possible that the same thing may occur in this new operation? That is to say, may not a vessel have escaped the action of the cautery and bleed after the parts are put back? Mr. Smith is alive to this, and has modified the instruments used by his predecessors, with a view to meet the danger. We say his predecessors, for Mr. Smith makes no claim to originality in this method of treatment, which, curiously enough, has run a course exactly like the nitric acid treatment of piles and prolapsus, having been invented in Dublin, introduced into London practice by Mr. H. Lee, and then found its most enthusiastic supporter in Mr. H. Smith. Mr. Smith, then, uses a sort of blunt scissors, furnished with a screw, instead of an ordinary clamp, whereby the base of the pile is securely held so that it cannot slip away before it has been thoroughly cauterised; and the grasp can be gradually relaxed in order that if any vessel bleeds the screw can be tightened and the cautery reapplied. Mr. Smith used to employ nitric acid as the cautery, but has now renounced its use in favour of the hot iron, which he says is not painful, and which is certainly more energetic in its action. We must presume that Mr. Smith has discarded the use of nitric acid from some inconvenience which he has found to attach to it, but nothing of the sort appears from his report of cases, for out of fifteen cases, treated with nitric acid, which he details in his Appendix, only one is said to have had any hæmorrhage after the operation, and this is distinctly said by Mr. Smith to have been his fault and not the fault of the method; since he attempted to secure two tumours in one clamp, and one of the tumours slipped away. In one other case there was so much bleeding at the time of the operation that Mr. Smith had to include the bleeding vessel in a ligature (Case 7): but this also occurred in one of the cases with the actual cautery (Case 32). We are therefore left to conclude from Mr. Smith's practice that nitric acid is not an effectual agent to suppress hæmorrhage in these cases, though from the terms in which the cases are reported we should have been led to believe the reverse.

Let us grant, however, for the sake of argument, that the actual cautery can be used with the same security against hæmorrhage, and with as little immediate pain as the ligature, and we still have to inquire whether it has any real advantage. It is evident that this must be a matter of experience, and can best be settled by the evidence of those whose practice lies mainly in this special department of surgery. Mr. Smith's assertions in the work before us have (as our readers perhaps know) been sharply criticised by Mr. James Lane in the columns of the 'Lancet.' It is hardly our province to pronounce a dogmatic judgment on a question on which men of so great experience hold opposite opinions; but we must be allowed to say that Mr. Smith has ventured to speak of this new plan in terms utterly unsupported by any arguments or facts which he has to adduce. Let us discuss the three advantages above referred to in their order: (1) That the method by clamp and cautery is safer—Mr. Smith says that the ligature is followed occasionally by tetanus and by pyæmia. This is no doubt true, but either occurence is a very rare one; and, perhaps, death after ligature of piles is as rare as after any surgical operation, and occurs mainly in persons suffering from some organic affection and who are liable to fatal consequences from the slightest causes. After the operation with the cautery Mr. Smith says that "it is not possible that either tetanus or pyæmia can occur"—a very unfortunate expression certainly, and which is handled with unsparing logic by Mr. Lane, in the paper above alluded to, who asks whether burns are remarkable for their immunity from tetanus, or whether absorption of matter never follows the separation of a slough. The fact is that death after ligature of piles being an excessively rare event, every surgeon could bring forward a long string of cases in which that operation had been practised without any unpleasant symptoms. Thus Mr. Lane tells us that more than 800 cases have been operated on at St. Mark's Hospital by Mr. Gowlland and himself with uniform success, except that four cases of fatal tetanus occurred at about the same time—a fact obviously due to some inscrutable temporary cause. Now all the evidence which Mr. Smith adduces here to show the safety of the new method consists merely of 15 cases in which the clamp was used with nitric acid, and 20 with the actual cautery. But so small a number cannot prove anything, especially when we notice that the operation with nitric acid has been abandoned clearly because it was unsafe.

2. It is said that the new method is quicker, so that the patient can safely go about in four or five days after the operation. In one case Mr. Smith operated thus on an out-patient at the hospital, and allowed him to walk home after the operation, but he does not recommend this as an example to be followed.

With regard to the reality of this advantage over the ligature, Mr. James Lane says that the method recommended by Mr. Smith has been tried at St. Mark's Hospital in a considerable number of cases, and that the stay of the patients in the hospital has been on an average as long as after the ligature. It must be recollected that the application of the cautery will cause the part to which it is applied (viz., the base of the tumour) to slough), and that on the separation of this slough an ulcerated surface of the same extent will be exposed. After the ordinary operation the ligature has to cut through the same extent of tissue (*i. e.* the base of the tumour), and on its separation the same extent of ulcerated surface is left exposed, so that theoretically we should expect that the only difference in rapidity between the two processes would have been the difference (if any) between the separation of a slough and the coming away of a tight ligature, the amount of tissue in each case being the same; or if the device introduced by Mr. Salmon, of cutting away the pile from its connection with the skin of the anus, be adopted the amount of tissue to be divided by the ligature will be the smaller. Still, this is a matter purely of experience; and most of Mr. Smith's cases are, no doubt, somewhat less protracted than is usual after ligature. But the difference of three or four days in the length of treatment is after all in most cases quite unimportant.

3. The third advantage claimed by Mr. Smith for the new method is its comparative freedom from pain after the operation, and its presumed immunity from the risk of the abiding ulceration which sometimes (though very rarely) persists after the ligature. With respect to the latter point we have really no means of judging until our experience of the new method shall be more nearly equal to that of the old, since the event is in any case a rare one. But it really does seem *à priori* probable that the after-pain, which is often very distressing from the ligature, would be less after the cautery.

The reader will have gathered from these observations what is our opinion with respect to this new treatment, viz., that its advantages are as yet unproved, and its immunity from danger is also unproved. It is not always in the first few cases of a new operation that its risks become apparent. Mr. H. Smith will not have forgotten that Mr. Holt produced an array of cases far longer than his own to show that the operation of bursting a stricture was free from any serious danger, and that it fell to his own lot to publish a case in which the operation proved fatal to a man apparently in sound health. In some of his own cases there is mention of hæmorrhage after the cautery, which, though Mr. Smith tries to account for it otherwise, may possibly be found on further experience to be necessarily an occasional and, perhaps, sometimes a very serious complication. Nor can we allow the absolute immunity which he asserts from the risk of tetanus and pyæmia. Still a method which has satisfied Mr. H. Lee and Mr. H. Smith of its superiority to an operation so very successful as the ligature of hæmorrhoids must have a good deal to recommend it, and may at any rate be justifiably employed by those who are in any way dissatisfied with the old operation.

ART. XIV.—*Clinica Medica di Siena diretta dal* Professore PIETRO BURRESI, *Diabete.* Estratto dallo Sperimentale dei mesi de Marzo, Aprile, e Ottobre, 1864. Pp. 50.

Medical Clinique of Siena, under the direction of Professor PIETRO BURRESI, *on Diabetes.* Republished from the 'Sperimentale' of the months of March, April, and October, 1864.

THE present treatise is a further step towards enforcing the truth, that we have no sound basis for current ideas as to the pathology of diabetes. A common error selects one or more prominent symptoms as representative of the disease, but we are undeceived by the entire want of relation and proportion to the intensity of the malady during its progress, discoverable, under a severer class of tests, in any single symptom; and the effect of intercurrent diseases, such as diarrhœa, and especially of softened tubercle, proves that the secret of the disorder lies deep in the organism.

So far is Signor Burresi from regarding diabetes as consisting exclusively in a secretion of sugar, that he selects defective assimilation as its principal nosological feature, or rather, he leaves its nosological position undeclared, calling it, as did the ancient physicians, an *idiopathia,* the existence of which is as certain as its nature is unknown, while its diagnosis from polyuria is clear, necessary, substantive, and radically definable. The nature of the food rather than the amount of fluid ingesta modifies the amount of urine secreted, and the low temperature of the diabetic seems to forbid the idea of an increased metamorphosis in the tissues.

In the absence of direct remedies, Signor Burresi finds a feeble resource in the use of a saccharine diet, as represented by dates, figs, grapes, mustard, and glucose, superadded to a mixed repast of common character, such as ordinary hospital fare. This saccharine diet possesses the advantage of being well endured by the patient, and of being a welcome change after the use of an exclusively animal regimen—he has even maintained its use for three months' duration; the benefit derived from it is attended with much uncertainty, but in some cases the relief is both speedy and continuous; such a release from the monotony of sheer animal food seems to the author highly desirable.

Both the author of the treatise and Professor Capezzuoli have employed themselves in the view of the doctrines of Bouchardat, and they find that the amount of sugar secreted does not depend with any exactness on the quality of the food, any more than it does on the indulgence of thirst; animal diet only seldom produces a slight diminution in the relative amount of sugar to a given quantity of urine, and even the saccharine diet (as above defined) may have the same effect. They do not deny that by modifying the amount of urine, diet is often very influential in increasing or diminishing the absolute amount of sugar secreted. In such cases they regard the operation of diet, with regard to the sugar, as indirect. We confine ourselves to a bare indication of the scope of this work, the impossibility which presents itself to us of entering into the nice details of which it consists, compels us to rest satisfied with the little information we now tender to our readers. Though founded on the observation of but six cases, it is a good example of minute and philosophical inquiry, no consideration being neglected which can be brought to bear on the disease.

ART. XV.—*Inconsistencies of the English Census of* 1861 *with the Registrar-General's Reports, and Deficiencies in the Local Registry of Births.* By WILLIAM LUCAS SARGANT, 1865. Pp. 51.

THE following conclusions, the results of the author's calculations, are so important, if any wise correct, that we are induced to quote them; not indeed with the intention of engaging in any critical examination of them, but solely for the purpose of calling the attention to them of those of our readers who take an interest in vital statistics. We have only to add, that the paper containing them, with the author's explanatory remarks and suggestions, is published in the 'Journal of the Statistical Society of London,' for March, 1865.

The conclusions in question, which he calls the principal ones he has arrived at, are the following:—

"1. That the census of 1861 is not to be implicitly trusted, but requires further investigation.

"2. That the male infants below 1 year old are underrated by 36,546, or 12 per cent.; and the female infants by 30,831, or 10½ per cent. That in the second year of life the deficiencies are 11½ and 11 per cent.; in the third year 2 and 1 per cent; and in the first five years taken together, 6½ and 6 per cent.

"3. That this difference of error between male and female infants is probably owing to the better registration of male births, and not to a worse enumeration of males in the census.

"4. That the males and females together, of all ages *under* 20, are apparently underrated by 510,446; but that some considerable deductions have to be made from this number.

"5. That the males and females together of *all ages* are probably underrated by more than half-a-million.

"6. That the deficiency in the census is far greater in some districts than in others.

"7. That the registration of births is very imperfect in some places, Liverpool and Hull appearing to be the worst, with London, Cheltenham, Plymouth, and Portsmouth, following in order of demerit.

"8. That we have but few materials for comparing the census of Scotland with calculations made from the registers of births; but that as far as we can judge, the Scottish census is as inaccurate as the English one."

ART. XVI.—*On the Probable Surgical Effects in Battle, in case of the employment of Projectiles of a more elongated form, such as the Whitworth Projectiles.* By Deputy Inspector-General THOMAS LONGMORE, Professor of Military Surgery, Army Medical School. Reprinted from 'Army Medical Report for 1863.' Pp. 7.

THIS short tract will well repay perusal, as containing some interesting and curious facts, as well as speculations, on the effect of bullets of different kinds employed in war. It is not, perhaps, always present to the mind of a civil surgeon, though obvious when once stated, that the effects of different kinds of arms have to be studied for military purposes under two points of view, viz., how they act on the individual who is first struck, and what effect they have on the body of troops of which that individual forms a part. The object of firing on a body of troops is, as Mr. Longmore justly observes, not to kill as many as possible, nor even chiefly to stop their instant power of motion (as when wild animals are hunted); but to inflict, upon as many of them as possible, such injuries as shall render them useless as soldiers, at least during the campaign. Consequently, projectiles are sought for, such as (1) have a high velocity and great range; (2) are not easily deflected from their course; (3) produce severe injuries in their course; and (4) can pass through successive files of men. The old spherical balls have at first a greater velocity than the new rifle bullets; but this is a matter of no practical importance, since the velocity is soon lost before attaining the distance at which firing would commence. The velocity, then, and the momentum of various projectiles of the forms now in use must first be studied, since it is evident that the individual struck will be in all probability more seriously injured if wounded by a projectile travelling with great momentum. We say in all probability, for the case of flesh-wounds must perhaps be excepted. If the bullet is to pass only through soft parts, it is possible that the swift course of a modern rifle-bullet will leave less laceration than the old musket-ball. But penetrating wounds of the head, chest, abdomen, and pelvis will be more common, and the bones will be more extensively splintered in all kinds of fractures. Hence, the individual struck will be more certainly disabled.

Secondly, with respect to deflection. A heavy conical bullet flying with great momentum and striking with its point will often perforate the body of the man whom it first strikes and pass into the man behind him, and even further if its path be sufficiently horizontal; while a round ball with a lower velocity must either be stopped or so turned aside that its course will probably no longer lie within the body of troops aimed at. This power of penetration and of resisting deflection will also be increased if the bullet be rendered harder in consistence. Consequently, pointed heavy projectiles, strengthened by admixture with some harder metal, are more dangerous to a body of men than any other projectile from small-arms.

Thirdly, with respect to the severity of the injuries produced by these elongated projectiles in their course, Mr. Longmore gives some particulars—novel to us, though possibly not so to military surgeons—showing how these bullets alter their rotation in consequence of the varying resistance they experience. Thus, a bullet which strikes, spinning as it does so on its long axis, may change its rotation so as to revolve while inside the body on one of its short axes, thus greatly increasing the area of its track, and, of course, the size of the wound it produces; and so, both the actual laceration and the chance of implication of important structures are increased. Some of Mr. Longmore's facts on this head must be very interesting, and would form, we should think, the material for an instructive monograph.

But, after all, the fourth consideration is that which is of most practical importance, viz., so to strike the opposing body of troops that successive files of men may fall under one bullet. This depends partly on the considerations we have dwelt on above, viz., that the momentum of the bullet shall be sufficient, that its form shall admit of easy penetration, and that it shall not be very susceptible of deflection. But it cannot be ensured unless its path be nearly parallel to the earth's surface—that is, unless the curve it describes be a very flat one. In the case of a ball describing a sharp curve, if it preserves its direction, it will probably either strike the ground or pass over the head of the man behind him who was first struck. The "lowness of the trajectory" is therefore an important element in the question which of two different kinds of projectile is the more dangerous in battle.

In all these respects Mr. Longmore says that the more elongated Whitworth is superior to the shorter Enfield bullet, and that of the Whitworth projectiles, the hexagono-conoïdal is superior to the cylindro-conoïdal, since the former is hardened by admixture with one tenth of tin; while the latter, like the Enfield, is made of lead only.

As far, then, as the powers of the bullet go, Mr. Longmore seems to argue that our troops would benefit by the substitution of this projectile for that supplied from the Government factory. Of course, this is but one element in the question, regarded from a military point of view; but it is an important one, and we cannot doubt that, when put so plainly before them by an officer of Mr. Longmore's reputation, it will receive from the military authorities the importance which it deserves.

Mr. Longmore's little pamphlet will amply repay the careful study of any one interested either in surgery or mechanics, and specially of those whose trade familiarises them with the terrible combination of the two, in which Mr. Longmore has rendered such good service.

ART. XVII.—*Lake Habitations and Pre-historic Remains in the Turbaries and Marl-beds of Northern and Central Italy.* By BARTOLOMEO GASTALDI, Professor of Mineralogy, &c., at Turin. Translated from the Italian, and edited by C. H. CHAMBERS, M.A., &c. *London,* 1866, pp. 128.

THIS publication, for which we are indebted to the Anthropological Society, will be acceptable to ethnologists in general, not so much for any new facts of importance which it contains, as for the proof it affords that there existed in Italy a people or peoples anterior to the Etruscan or Roman epochs,—races dwelling in pile-habitations similar to those so common in Switzerland and the north of Europe, and using implements, some of them of stone, but more of bronze, of much the same form and character as theirs.

It is curious to see how the rude arts with improved civilisation advancing southward, and how the north, at least in Europe, was indebted to the south for its improvement.

It is curious, too, to find that these archaic researches are quite new in Italy; and that in a country, supposed to be so well explored for its antiquities, relics of the remote prehistoric period, belonging to the stone-age of mankind, have only been discovered—not having been sought for before—within the last two or three years; and yet, that arrow-heads of flint had been long known to the peasants, by whom they are called *pietre del fulmine,* and are kept with the belief, on the homœopathic principle, that they protect the houses of those who possess them from lightning.

Most of our readers are probably acquainted with Sir John Lubbock's excellent work on the same pre-historic subject. In the latter part of it, where he treats of existing savage races, and in a very interesting manner, he describes the Tasmanians as ignorant of the mode of obtaining fire, *i. e. de novo,* so as to be under the necessity, after the manner of the Vestal virgins, of preventing its extinction; noticing them as a solitary instance of the kind. This as we learn from the best authority, Mr. Robinson,* is an error; he assures us, that like so many other rude races, they were in the habit of kindling fire by the friction of wood, and that he had often seen them practise it. As this unfortunate people may soon be extinct, should not the mistake be pointed out now, it may be perpetuated as a fact, and an important fact, by those naturalists who consider man as of Simian origin.

ART. XVIII.—*Des Fistules Génito-urinaires chez la Femme. On Genito-urinary Fistula in the Female.* Par IGNACIO RODRIGUEZ DA COSTA DUARTE, &c. &c. *Paris,* J. Baillière et Fils. 1865, pp. 96.

WE can speak on the whole in terms of praise of this pamphlet, which gives, in reasonable compass, a tolerably complete account of the anatomy and the causes of the various abnormal communications which may be formed between the uterus, or vagina, and the urinary tract, whether ureter, bladder, or urethra. The treatment is also fairly described, and illustrated by numerous cases, chiefly quoted from French and English sources. In this way the author has laid under contribution

* The same gentleman who enabled us to expose another mistake respecting the same people and the natives of Australia. See the January Number of the 'Review' for 1865, p. 91.

M. Jobert, M. Velpeau, Mr. Baker Brown, and Mr. James Lane. In quoting the latter author, he gives him the name of "Mr. James," a fresh example of the constant ambiguity which the invincible inability of foreigners to use English names correctly introduces into literature. On reading this case, it would be natural for any person not minutely acquainted with English surgery to refer it to the well-known Mr. James of Exeter. In other respects, however, this author is more careful than many foreign writers, for he always refers to his authorities distinctly, by volume and page, instead of adopting the vicious custom so constant in French works of merely saying, "Jobert says," or "we find in Velpeau," and leaving the reader who wishes to verify the reference, to hunt over the whole works of a voluminous author. To turn to the matter of the pamphlet. The reader will find an interesting account of the rare lesions consisting in a fistula between the ureter and uterus, and its diagnosis from the more common fistula between the bladder and uterus (pp. 21 *et seq.*). It was for this latter lesion that the operation attributed to "Mr. James" was practised, and Mr. Lane's operation is certainly one of the most interesting in this department of surgery. On the subject of vesico-vaginal fistula, complicated or not with injury to the uterus or urethra, our author is clear and sufficiently full in his directions, though not very minute. After describing the well-known proceedings of Jobert, Simon, Marion Sims, and the modification of the latter by Bozeman, he gives the preference, rightly as we think, to the original operation of Dr. Marion Sims. In very complicated cases, he would not hesitate to obliterate the vagina entirely; and he gives a description of such a case, in which the urethra being destroyed and the vagina injured in many parts and almost obliterated by cicatrisation, he proposed to establish a communication with the rectum, and to close the vulva, so leaving the bowel to act the part of a cloaca for the urine, menses, and fæces. The patient, however, refused. Finally, he quotes with approbation, Mr. Baker Brown's case, in which he closed the vulva altogether, in a case of combined vesico- and recto-vaginal fistula.

On the whole, this is a careful and creditable brochure, although destitute of any proof of originality, or even of extensive experience.

Art. XIX.—*The Treasury of Botany: a popular Dictionary of the Vegetable Kingdom.* Edited by John Lindley, Ph. D., F.R.S., and Thomas Moore, F.L.S. In two Parts. Pp. 1254. *London:* Longmans.

THE name of Dr. Lindley will be long held in grateful remembrance in this country as one of the most successful teachers of botany that we have ever had. Possessed of vast information himself, and untiringly and unceasingly working to increase the general store of knowledge in his department, he was not above imparting the results of his labours to others in a popular form. Popular science did not with him mean, as it too often does, shallow science. Unfortumately, he over-tasked a vigorous intellect and robust frame, and the present work will possess an interest for some, as being the last out of the great number with which Dr. Lindley's name is associated.

The plan of the 'Treasury of Botany' had, we are told, been perfected under his supervision, but he was unable to continue his editorial labours beyond letter C, and before the book passed from the printer's hands its original editor had passed away. Mr. Moore's labours have in consequence been anything but light, but we may fairly congratulate him on having brought to completion the most valuable work of popular reference on botanical subjects that has appeared in our language. That in a work of such magnitude, and one to which so many writers have contributed, errors should be found is no matter for wonder, but assuredly they do not materially affect the value of the book. The reader will find a popular and, so far as we have seen, accurate account of all the more important genera of flowering and flowerless plants, with notes on their uses and value for pharmaceutical, commercial, or æsthetic purposes. Lists of popular names (often more embarrassing than technical ones), a glossary, and a series of illustrations of botanical geography will be found in this compendium, which is abundantly illustrated both by steel plates and small but clear woodcuts. The names of the contributors afford a guarantee for the general accuracy of this compilation, which, including as it does matter not to be found in any of the ordinary cyclopœdias or text-books, will become a valuable work of reference not only for the general reader but also for the student.

ART. XX.—*Resection of the Shoulder-Joint: Remarks upon the cases of six Invalids, admitted during the year* 1864, *at the Royal Victoria Hospital, upon whom Resection of the Shoulder-Joint had been performed in New Zealand for Gunshot Injuries.* By Deputy Inspector-General T. LONGMORE, Professor of Military Surgery at the Army Medical School. Reprinted from 'Army Medical Reports,' vol. v, for 1865. Pp. 25.

This pamphlet has several features which entitle it to more attention than its unpretending appearance and small extent might perhaps otherwise attract. In the first place, and more especially, it contains Professor Longmore's matured judgment as to the great value of this excision in military practice. It is true, that in theory it is admitted that, in gunshot injuries requiring operation, excisions in the upper extremity are always to be considered the correct operation until contra-indications are proved, while in the lower extremity the reverse is the case. But the practice does not seem hitherto to have corresponded to this theory, at least in English military surgery, for, even in the Indian mutiny of 1857-8, it is noted in this pamphlet, that out of 935 wounded, who were invalided to England, there were nine cases of amputation at the shoulder, and only one of excision. Whether this great proportion of amputation is to be attributed to the circumstances of warfare indisposing surgeons to any operation requiring long attention and careful bandaging, or to the fact, that the superior officers had learnt their surgery before resections came into fashion, we know not; but Mr. Longmore does not disguise his conviction, that amputation at the shoulder is unnecessary in the great majority of those cases in which it has hitherto been practised in warfare. Hence we are glad to see that in the late (or present?) New Zealand war, a different practice seems to have been pursued, since six more or less successful cases of excision are here related, and another is known to have occurred, while no cases of amputation had been heard of. When the surgical history of the American civil war comes to be written (if, indeed, materials have been preserved for writing it), we shall no doubt have our knowledge of this matter greatly enhanced; and we doubt not that in many cases the limb has been preserved, even when all the movements of the arm have been entirely abolished by the extent of the injury of the humerus and the length of the splinters which it has been necessary to remove. In such cases, the arm hangs like a flail from the thorax; yet an apparatus may be contrived to support it, and then the patient enjoys the use of his hand, and his condition is incomparably better than that of the man who has lost the whole extremity. If we may trust to the conversation of men who have borne a part in this war, such operations have become common in the American army; and if the practice were recognised, it would of course greatly limit the application of the shoulder-joint amputation.

Mr. Longmore gives us here a short history of the introduction of this excision into military practice, and a well-reasoned defence of the method which he prefers for its performance, *i. e.* by a longitudinal incision in the course of the biceps tendon (which is to be preserved), in place of the flap from the deltoïd, which was the old method of operating. We have no hesitation in expressing our preference in this, as in every other excision, for a simple straight cut, whenever the morbid thickening, or the natural depth of the parts is not very great. It is also reasonable and probable (though we venture to think it is hardly yet proved), that the long tendon of the biceps may be useful when it can be preserved. At the same time, we cannot say that the two cases which Mr. Longmore gives as a contrast, in one of which the operation had been done by a simple incision, and in the other by a flap, support the inference which he founds on them, that the former operation is essentially superior; since, allowing that in the first operation the arm was less useful than in the other, the fact is evident from the history, that the injury was far more severe, and more bone was removed. In fact, it is impossible to prove such a point by the histories of isolated cases—nor is it necessary. The flap operation involves more injury and more bleeding than the other, therefore unless it is essentially necessary (which it can only be when the parts are at a great depth and a full view of the glenoid cavity is required) it should not be adopted.

The cases are fully detailed, and some good illustrations of the results of the operations are given.

We accept this pamphlet as a useful contribution to the history of this excellent operation, and a fresh encouragement to surgeons to preserve limbs which our predecessors would have sacrificed.

ART. XXI.—*Observations on the Inca and Yunga Nations, their Early Remains; and on Ancient Peruvian Skulls.* By ARCHIBALD SMITH, M.D. (late of Lima). *Edinburgh*, 1864, pp. 16.[1]

WE can do little more than recommend this paper to the attention of those of our readers who take an interest in ethnological research. The advanced stage of civilisation which the nations named above had attained at the time of their invasion and cruel conquest by the Spaniards is now well known; it is put in a strong light by the author, especially the superiority and the greater spirituality and purity of their religious views, compared with those of the majority of the aborigines of the American continents. The variety of colour of these people is particularly interesting, as showing how much the colour of the skin is modified by physical circumstances. The following

[1] From the 'Proceedings of the Society of Antiquaries of Scotland,' vol. v.

extract well expresses this and some of the peculiarities of the indigenous races of the coasts and of the Sierra of Peru :—

"As a whole, they are of short stature, with small compact hands and feet, but on the mountains, where the air is highly rarified, we find a proportional expansion of the lungs and depth of chest, a ruddy complexion, and remarkably firm, well-framed muscular limbs. In colour of skin they vary considerably, according to the elevation and climate, individual constitution, and other causes. I have seen in the valley of Huanuco an industrious agricultural Indian family, of the name of Avila, who were distinguished by a fair complexion and lightish hair; and I have been told of tribes on the eastern frontiers who are said to be nearly as white as Europeans. But making allowance for every subordinate divergence of colour, the prevailing tint of the skin is brownish, though in some instances it deepens into bronze, and in the humid, sultry forest land, verges to yellow.'

One peculiarity of their long black hair is that even in old age it rarely turns to grey. He describes their cranial conformation as characterised by—

"A naturally low and narrow forehead, as compared with the interparietal or lateral swell; a short longitudinal diameter; and, very commonly, a more or less depressed occiput."

He adds :—

"But that the Indian forehead is not always naturally low, a striking evidence is found in the portrait of the late Archdeacon of Cusco, the much honoured Dr. Justo Sahuaronra, who was the last of the Incas of Peru."

He assures us that—

"Intellectually, the educated Indian of Peru is allowed to be quite equal to the white Creole."

PART THIRD.

Original Communications.

Art. I.

On the Development of Striped Muscular Fibre in the Vertebrata: being part of a Graduation Thesis for which a Gold Medal was awarded by the Senatus Academicus of the Edinburgh University, August, 1863. By P. M. Braidwood, M.D., M.R.C.S.E., formerly President of the Royal Medical Society, &c.

Though the subject of voluntary or striped muscular fibre has been a much vexed question since an early period in the history of anatomy, the mode of development of muscular fibre has received comparatively little attention until within the last half century.

In no physiological inquiry probably can the generally received theories of the period be more distinctly traced out than in the various published observations on this subject. The earliest views are those of Valentin and Schwann, who considered muscular fibres to be formed by round nucleated cells, developed out of the "primitive jelly-like formative mass," arranging themselves linearly, then coalescing together, forming fibres ("secondary cells of muscle," Schwann), which become "uniformly transparent, defined, and cylindrical," having nuclei generally lying close together on their walls.[1] According to Schwann, the secondary cell elongates, and sarcous substance is deposited on its inner surface, rendering it solid. After the sixth month transverse striæ begin to be seen in the fibres.[2] Without referring specially to the observations published on this subject since that time, viz., Martin Barry's view of the development of muscle by the coalescence of blood-globules;[3] Lebert's by the development, first, of "organo-plastic" globules in a molecular blastema, then of "myogenic cells" which elongate, and become striated longitudinally and transversely, forming the

[1] Valentin, 'Entwickelungsgeschichte des Menschen mit vergleichender Rücksicht der Entwickelung der Saügethiere und Vögel.' 1835, pp. 267, 268.

[2] 'Microscopical Researches into the Accordance in the Structure and Growth of Animals and Plants,' translated from the German of Dr. Schwann. 1847, p. 141.

[3] Martin Barry, 'Philosophical Transactions of London.' 1840–41, p. 605.

ultimate fibres;[1] Remak's, by the elongation and coalescence of large primitive, transparent, round, nucleated cells, in which the nuclei become increased in number, and transverse striæ are formed;[2] let us briefly allude to Kölliker's researches. This distinguished histologist considers, like his predecessors, that muscle is developed from cells which elongate into fibres, and become striated. "The sarcolemma of a muscular fasciculus," according to him, "represents the sum of the membranes of the coalesced cells; the fibrillæ, being the altered contents of the cells, become solid; and the nuclei of the youngest fasciculi being the original cell-nuclei, whose descendants are represented in the nuclei of the older fibres which have multiplied by an endogenous process."[3]

With this brief summary of the earlier views on the subject of the development of muscle, we proceed to consider the more recent observations of Messrs. Savory and Lockhart Clarke.

The first part of the sequel contains a brief *résumé* of their observations—which I have for the most part confirmed—together with additional investigations of my own; while the latter part refers to two links in the chain of the development of muscle which have not been described by these observers, viz., the development of the sarcolemma and of the transverse striæ.

The first appearance, then, presented by muscle is as a transparent, granular blastema, in which certain round and oval bodies of a globular form and granular aspect, containing distinct nucleoli, are formed (Pl., fig. 1). These cytoblasts or nuclei, according to the focus by which they are examined, appear either like fat-globules —as transparent bodies, with a dark smooth border—or have a granular aspect, generally containing a nucleolus, granular contents, and presenting a clear space immediately within the delicate border which limits them, and between it and the granular contents (Pl., fig. 1, *a*). These cytoblasts vary very little in size in different animals, but are met with of different sizes in the same animal. In the frog-tadpole, fœtal calf, and chick, I have found them to measure on an average from $\frac{1}{4100}$th to the $\frac{1}{2050}$th of an inch in length, and from the $\frac{1}{4100}$th to the $\frac{1}{2730}$th of an inch in breadth. Such cytoblasts are present also in the early forms of contractile substance which exist among the lowest divisions of the animal kingdom, viz., the Protozoa and Radiata; for example, in sponges and polyps (Pl., figs. 8 and 9 and 2). In all their characters these

[1] Lebert, 'Annales d. Sc. Nat.,' "Mémoire sur la Formation des Organs de la Circulation et du Sang dans l'embryon du Poulet." Series III, 'Zoologie,' vol. ii, 1844.

[2] Remak, "Ueber die Entwickelung der Muskelprimitivbündel," in 'Froriep's Naue Notizen,' No. 768, 1845, p. 306.

[3] Kölliker, 'Manual of Human Histology,' translated by Busk and Huxley, pp. 272–3.

cytoblasts resemble those met with in the primitive muscle of the Vertebrata, except that they are for the most part larger.

The nucleoli of the cytoblasts appear as bright particles of a round form, with a smooth defined outline, and measuring on an average $\frac{1}{12300}$th of an inch in diameter. In addition to these nuclei there are seen, at this stage, in the blastema, numerous round, transparent, highly refractive particles, in every respect similar to the nucleoli of cytoblasts, and measuring from $\frac{1}{8200}$th of an inch in diameter to a size very much smaller. The cytoblasts possess each, most commonly, one or two, occasionally three, and very rarely four nucleoli. The particles resembling the nucleoli of the cytoblasts, and seen floating about in the blastema, are probably some of them the nucleoli of disintegrated cytoblasts, while others are those of cytoblasts yet to be formed.

With regard to the effects of reagents on these structures, as observed under the microscope, in the case of the muscles of a chick on the ninth day of incubation, *acetic acid* renders the wall of the cytoblasts very transparent, almost as if dissolved, and thus their contents become more distinct (Pl., fig. 1, *b*). The blastema at the same time appears more transparent. *Syrup* causes the cytoblasts to become contracted through exosmose, and thus to assume irregular forms. On the use of a *saline solution* the cytoblasts shrink up, and are evidently chemically affected. *Water* has no marked effect. On the addition of *sulphuric ether* the nucleoli and contents of the cytoblasts become very distinct, and refract light strongly. This effect is most marked in the case of cytoblasts imbedded in blastema. *Liquor potassæ* causes the contents of the cytoblasts to appear turbid. The cytoblasts themselves are diminished in size, and present irregular forms, seemingly from the collapsing of their walls after their contents have been dissolved by this reagent. The chemical nature, then, of these structures, as inferred from the above actions of reagents upon them, may be stated to be oleo-albuminous. Thus also are the cytoblasts proved to be surrounded by a very delicate cell-membrane, while their globularity is shown by the effect of changing the focus.

The next stage in the process of development of striped muscular fibre consists in the fibrillation of the primitive blastema along the sides of the cytoblasts, and the adhesion of the fibrillæ thus formed to their walls. Sometimes these fibrillæ enclose a single nucleus with conical processes of blastema, causing a resemblance to a fusiform nucleated cell. Most frequently, however, the fibrillæ enclose a series of cytoblasts, lying at variable distances from each other, and cemented together by blastema (Pl., fig. 2, *a*). Occasionally the cytoblasts forming such a series are seen to lie over each other in an imbricate form. Acetic acid renders these primitive muscular fibres and their nuclei more transparent. As seen in Pl., fig. 2, *a* and *b*, the cytoblasts lie with their *long axes parallel to the fibrillæ* which enclose them. On this point my observations differ from those of Mr. Savory, who says that "the nuclei, or cytoblasts, are not at first generally arranged in a single series; but two, three, or even more, lie side by side in apparent disorder."[1] As the primitive fibres increase in size, and become opaque in their interior from the accumulation of blastema, these nuclei, according to Mr. Savory, assume a much more regular position. "They fall into a single row (with remarkable regularity), and the surrounding substance at the same time grows clear, more transparent, and is arranged in the form of two bands bordering the fibre, and bounding the extremities of the nuclei so that they become distinctly visible. The nuclei have now become decidedly oval and closely packed together, side by side, so closely, indeed, that they appear as if compressed. Thus they form a single row in the centre of the fibre, *with their long axes lying transversely*, and their extremities bounded on either side by a thin, clear, pellucid border of apparently homogeneous substance. No structure can be discerned between the nuclei, owing to the close way in which they are pressed together. Sometimes, also, along the fibre an occasional nucleus lies obliquely instead of transversely."[2]

This fibrillation of the blastema presents an appearance closely resembling the contractile substance of the higher Radiata (Pl., fig. 9). The primitive muscular fibres are next seen to increase in breadth by the formation of lateral bands along their margins (Pl. I, fig. 2, *a*). These lateral bands are developed from the blastema in which the primitive fibres are imbedded, and possess cytoblasts like those of the original fibres. After becoming adherent to the muscular fibres they coalesce with them, while the fibres themselves elongate, and their nuclei separate from one another.

The next change observable in the process of development is that *some* of the cytoblasts in the fibres decay, *i. e.* become broken up into clusters of granules (Pl., fig. 2, *b*). Before this transformation occurs the cytoblasts are observed to increase in size, to become more circular, and their contents to be greatly multiplied and to become larger. Thereafter the granules, arranged formerly as clusters, are observed to separate from each other along the axes of the fibres, and to become by degrees obscured by the increased deposit of sarcous element on the surface of the fibres. In the fibres earliest developed these changes are seen to proceed more rapidly than in those of later formation.

The muscular fibres are observed next to become striated longitudinally, owing, doubtless, to their subdivision into fibrillæ (Pl.,

[1] Savory, 'Transactions of Philosophical Society, London.' 1855, pp. 245-6.
[2] Ibid.

fig. 3, *a*). This appearance is soon followed by that of faint transverse markings observed along the margins of the fibres (Pl. I, fig. 3, *a*). The faint longitudinal striation gives the muscular fibres of the Vertebrata an appearance very like that of the contractile tissue met with among the Mollusca and some of the Annulosa (Pl., fig. 10). In these lower classes of the animal kingdom, however, the fibres of the contractile tissue possess a glistening aspect, like that of ligamentous tissue.

"A faint indication of the appearance of the transverse striæ," says Mr. Savory, "may sometimes be observed in the lateral bands, almost as soon as these are fully formed; and as these bands approximate, the striæ become more plainly marked, and often contrast strongly with the intermediate and apparently homogeneous central portion of the fibre."[1] The longitudinal striation of the fibres, however, according to my own observation, is perfected *before* transverse striæ appear; but sometimes the transverse striæ are most distinctly marked because of the greater amount of blastema present in the centre of the fibre. The transverse striæ appear at first as scattered streaks along different parts of the margins of the fibres; they then gradually extend towards and meet in the centre of the fibre. At this early stage the transverse striæ are arranged more closely together than in adult muscle. This may be considered to be the last stage in the development of the primitive muscle-fibre or fasciculus, the next changes which it undergoes being an increase in breadth by the formation of lateral bands which ultimately coalesce with the fibre, a progressive subdivision of its substance into fibrillæ, the complete development of transverse striæ, and the formation of the sarcolemma or sheath of the fasciculus.

Examining, then, before proceeding further, a primitive muscular fibre as it now presents itself, undergoing the later stages of development at different points along its extent, it appears as a narrow cylindrical band, with more or less smooth margins. The cytoblasts which remain are seen at tolerably regular intervals along its axis, and generally equidistant from either margin. They are large, round, or somewhat oval in form, and lying with their long axes parallel to the margins of the fibre. Between them are seen clusters of granules, the remains of cytoblasts which have become disintegrated. Passing towards the opposite end of the fibre we see the longitudinal striation distinctly, as also indications of transverse striation along its margins, and large-sized granules aggregated or isolated in its centre.

The comparative anatomy of muscle furnishes us with no marked analogy in the case of the lower animals to the stage in the develop-

[1] Ibid., p. 249.

ment of striped muscular fibre in the Vertebrata, characterised by the earliest indication of transverse striation. The lowest class of animals in which I have observed transverse striation is the Annelidæ (Pl., fig. 11), but in the muscle of these animals it seems to be only superficial. For on examining the fasciculi we observe a delicate but distinct striation; but on dissecting these fasciculi into fibrils no indications of transverse striæ are to be found (Pl., fig. 12, *x*).

These fully formed muscular fibres continue to increase in size and breadth by the formation along their margins of successive bands of fresh sarcous element developed from the surrounding blastema under the agency of cytoblasts coalescing with the body of the fibre, and ultimately undergoing the changes before described (Pl., fig. 3, *b*.) Hence it is that fœtal fibres rarely possess the same diameter throughout their entire length. As the fibre acquires its more perfect form so its substance becomes condensed; it diminishes in breadth, but this is not accompanied by a corresponding separation of the nuclei, and the fibre becomes less transparent. Lastly, "as a rule, all fresh material which is added to the fibre before the original nuclei separate is," as Mr. Savory observes, "attracted independently of fresh nuclei; but that which is added after the original nuclei have separated or become disintegrated is by means of the additional nuclei which are attracted to the exterior of the fibre."[1]

The appearances above described as presented by striped muscular fibre during the various periods in its development have been observed by me in the case of the human embryo, of the calf, chick, frog-tadpole, and by Mr. Savory in the case of the fœtal pig. "In the human fœtus," says Mr. Lockhart Clarke, "from about half to three quarters of an inch in length, the first stage of development may be seen to commence by the formation of fine lateral bands or fibrillæ along one or both sides of one nucleus or more.[2]" The rest of the process of development takes place after the same manner in the human embryo as in the Vertebrata generally.

In the development, then, of striped muscular fibre in the Vertebrata, including man, the following steps may be traced:—In a molecular blastema oval or round bodies, termed cytoblasts or nuclei, are first formed; next follows the aggregation of these cytoblasts, and their investment by the surrounding blastema; their regular arrangement into linear series, bounded by lateral bands, and with their axes lying parallel to these bands; the elongation of the fibres thus formed, and the separation of their nuclei; the coalescence of the lateral bands with the fibres; the breaking up of the cytoblasts into clusters of granules along the axes of the fibres; the longitudinal striation of the fibres; and lastly, the appearance of transverse striæ

[1] Ibid., p. 252.
[2] Lockhart Clarke, 'Transactions Philosophical Society, London.' 1861.

along the margins of the fibres passing gradually towards their centres. These several changes are not observed to occur in the fibres in regular *consecutive* series; but often two or more are seen to take place *at the same* time in a single fibre. Moreover, the rate of development varies in different muscles as well as in the various fibres of the same muscle.

On the origin of the transverse striæ and the sarcolemma.

None of the above observations, it will be noticed, have attempted to account for the formation of transverse striæ in the primitive muscular fibre or elementary fasciculus, otherwise than by considering them to be foldings or creasings of sarcous element. This is, perhaps, owing to the authors of these observations having examined fœtal muscle in the Vertebrata in the fresh state alone, and without using coagulating agents. If, however, such muscle be soaked for various lengths of time in *chromic acid,* the transverse striation is rendered more distinct, and can be traced even into the fibrillæ of the fasciculi. From my observations on muscular fibre so treated are derived the following results:

Examining a portion of muscle from the back of a frog tadpole about one and a quarter inches long, which has been soaked for four or five days in a weak solution of chromic acid, and then dissected into fibrillæ and preserved in glycerine, it will be distinctly observed that a fibrilla (measuring, on an average, $\frac{1}{16400}$th of an inch in diameter) presents the appearance of two very narrow well-defined bands, enclosing between them a linear series of globular particles, and connected together by intermediate sarcous substance. (Pl., fig. 4, *b*.) A fibrilla thus resembles a row of beads; each fibrilla is, however, in truth, more or less cylindrical just as the fasciculi are. When looked at with one focus the sarcous substance (dark spaces) of the fibrilla appears to be the darker; but on changing the focus the globular particles ("clear spaces") seem to be the darker portions of the fibrilla. This is described by Dr. Dobie as occurring in the fibrillæ of adult muscle.[1] That these spherical light particles are globular is proved by the changes produced on altering the focus; for, with a distant focus, they appear as transparent spheres, and by gradually altering the focus various portions of each of them are seen (Pl., fig. 4, *c*), till with the nearest possible focus they appear quite dark (Pl., fig. 4, *a*). With the advancing age of the animal the intermediate sarcous substance becomes condensed and increased, till these "clear spaces" in a child's muscle appear as cubes, having four surfaces, which are globular. This, it seems to me, explains the appearance described by Dr. Dobie as occurring in

[1] Dobie's "Observations on the Minute Structure and Mode of Contraction of Voluntary Muscular Fibre," 'Annals of Natural History,' vol. iii, series 2nd, 1849, p. 113.

the striated muscular fibre of certain animals; thus (Pl., fig. 7), the dark line sometimes seen crossing a "clear space" is the remains of a "dark space" which was once of the same size as the others, but has gradually diminished in breadth. Likewise, the somewhat light line observed occasionally in the "dark spaces" is probably the remains of a "clear space" which has become by degrees filled up with sarcous substance, or been compressed by two neighbouring "dark spaces."

The "clear spaces" of the primitive fibrilla appear to me to be derived partly from the larger molecules of the blastema, and chiefly from the increase in size of the molecules into which the original cytoblasts, as we have before seen, are dissolved; for these molecules present all the characters of the "clear spaces" of the fibrillæ, and they increase in size probably by their inherent powers of growth and by the coalescence of two or more smaller molecules. The "clear spaces" of the primary fibrillæ are spherical in form, colourless, with a well-defined dark border, transparent, possessing a delicate wall, enclosing probably semifluid contents, and they refract light like oil-globules. In diameter they measure nearly $\frac{1}{16400}$th of an inch. The "dark spaces" of the primitive fibrillæ, on the other hand, appear also to be surrounded by a delicate wall, but their contents are more dense. That each fibrilla is surrounded by a membrane is very difficult to be demonstrated, but it may be reasonably supposed to exist from the following facts:—A fibrilla possesses a well-marked contour, indicated by parallel lines; moreover, in preparations of muscle preserved in alcohol, when the latter has evaporated, the fibrillæ are observed to be narrow and their "clear spaces" to be shrunken, as if from the collapsing of their walls, but they resume their natural appearance on the addition of alcohol; and lastly, the "dark spaces" of muscular fasciculi are seen to become approximated during contraction; and if they were unconnected by any bond of union, like a delicate membrane, it is to be expected that during the changes they constantly undergo in contraction and relaxation the dark particles would be very liable to be displaced.

Seeing, then, that what are commonly called the "clear spaces" of the fibrillæ are, in truth, not spaces, but particles possessing delicate walls, and probably containing fluid, the question then suggests itself—what is the function of these so-called "clear spaces?" That they may be considered as "reservoirs of pabulum," or stores of nutriment derived originally from the capillaries of the sarcolemma, and brought thus into more immediate connection with the proper sarcous substance, is rendered probable by the following considerations:—Most of the animal tissues, including nerve (as lately demonstrated by Professor Beale), are richly supplied with nuclei, from which they are believed to derive their immediate nutriment, and it is natural to suppose that muscular fasciculi are likewise provided with analogous structures. Moreover, no vessels have as yet been observed to enter through the sarcolemma into the substance of the fasciculi, whereas the constant tear and wear to which muscular fibre is subjected on account of its contractility is greater than that of any other tissue, leading to the supposition that the fasciculi have some inherent source of nutrition.

To refer now to the development of the sarcolemma. The sheath of a muscular fasciculus is of such extreme delicacy, even in adult muscle, that one cannot easily recognise it in that of the fœtus. Hence arise the diffident terms in which observers have couched their views as to the development of the sarcolemma. Of all the older observers, Remak alone gives an opinion on this point. Agreeing with Remak in his surmise regarding the origin of the sarcolemma from a delicate layer of sarcous substance raised above the surface of the fibrillæ, certain of my preparations, in which the fasciculi were slightly tinged with carmine or with asphalt, satisfied me as to the correctness of his theory, and showed in addition that the development is effected by the agency of cytoblasts (Pl., fig. 6). For as a primitive fasciculus advances in development, and as its lateral bands become by degrees one with it, the cytoblasts are seen (as observed also by Mr. Lockhart Clarke) to rise to the surface. When tinted as above mentioned the cytoblasts are distinctly seen to be nearer the surface than the transverse striæ; and that they are connected together by a delicate layer of sarcous substance, raised thus above the surface of the fibrillæ, is rendered very probable by the fact that the longitudinal striation of the fibre, as also the clusters of granules in its axis, and subsequently its transverse striæ, become more obscure, as if covered by a membrane.

From the preceding account of the development of striped muscular fibre in man and the other Vertebrata may be derived the following conclusions:

Muscle is originally developed as a molecular blastema of an oleo-albuminous character, in which nucleolated nuclei, or cytoblasts, with granular contents, are formed.

This blastema next undergoes a process of fibrillation; the cytoblasts—arranging themselves linearly, and with their long axes parallel to the fibrillæ between which they lie—are connected together by blastema.

As regards the progressive changes undergone by the cytoblasts, they become round, their contents increase in size, and at last they break up into clusters of granules.

The growth of the primitive muscular fibre, on the other hand, proceeds as follows:—It becomes more and more dense by the constant addition of portions of the surrounding blastema, lateral bands are formed under the agency of nuclei, and ultimately coalesce with the fibre; longitudinal striation next takes place in the fibre, and, somewhat later, transverse striæ are observed at different parts along its margins. In the fresh muscle this striation is seen by degrees to become more and more obscure from the increased blastema on the surface of the fibre (the sarcolemma).

The increase in breadth of a primitive fasciculus is accomplished by means of lateral bands with nuclei being formed on each margin of the fibre, and ultimately coalescing with it, as also by the gradual addition of portions of blastema. Increase in the bulk of adult muscle is due to the increase in length and breadth of its original component fasciculi, to a subdivision of the same without the agency of nuclei, and to an increase in the amount of the interfascicular connective tissue and fat.

The "clear spaces" of the primitive fibrillæ present the appearance of globular molecules arranged in linear series and connected together by sarcous substance. They probably are reservoirs of nutritive material, and serve to distribute the nutriment derived from the capillaries on the surface of the sarcolemma among the sarcous substance in its interior.

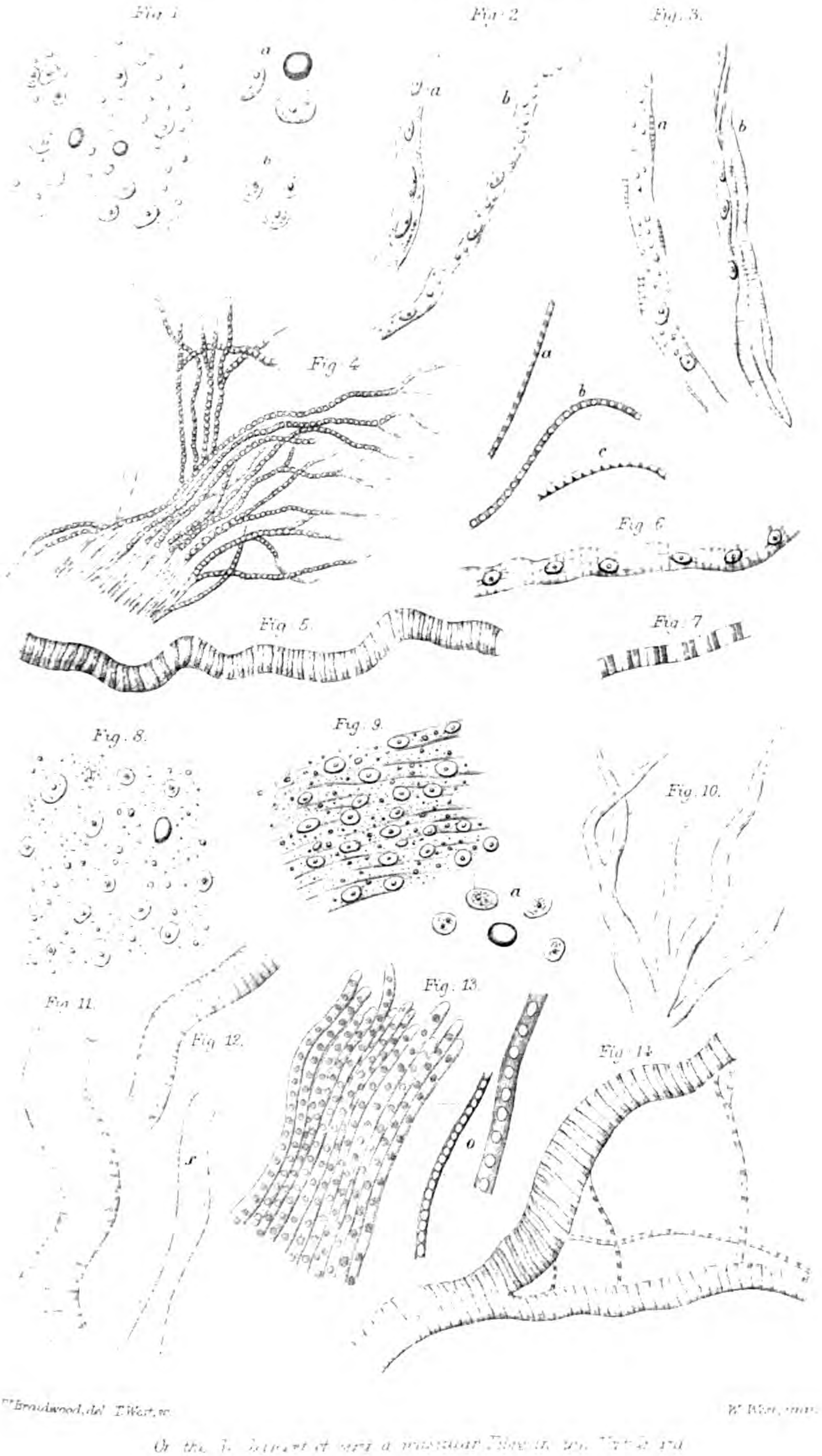
The sarcolemma seems to be formed originally as a thin layer of blastema, raised under the agency of cytoblasts above the surface of the primitive fasciculus. These cytoblasts become afterwards nuclei of the sarcolemma in the adult.

Lastly, a striking analogy may be observed between the appearances presented in the different stages of the embryonic striped muscular fibre of the Vertebrata and those of contractile substance in many of the lower divisions of the animal kingdom.

Description of Plate.

FIG.

1. From the back of a frog tadpole, one and one fifth of an inch long, shows the original molecular blastema of muscle with the cytoblasts forming in it. These cytoblasts (in the muscle of the frog tadpole, fœtal calf, and chick) measure, on an average, $\frac{1}{4100}$th to $\frac{1}{2050}$th of an inch in length, and $\frac{1}{4100}$th to $\frac{1}{2730}$th of an inch in breadth. The molecules vary in size from $\frac{1}{8200}$th of an inch in diameter to a size just perceptible by the eye. *a.* Isolated cytoblasts, illustrating some of the properties of those bodies. *b.* Cytoblasts, acted on by acetic acid, which causes them to appear as if their walls were dissolved.



W. Braidwood, del. T. West, sc.

W. West, imp.

On the [illegible] Muscular Fibre [illegible]

FIG.

2. *a*. Muscular fibre from the anterior extremity of a frog, showing the cytoblasts arranged linearly with their *long axes parallel* to that of the fibre. *b*. Muscular fibre from back of a human fœtus, three quarters of an inch long, illustrating the disintegration of the cytoblasts into clusters of granules, which gradually separate from one another.

3. *a*. Shows the longitudinal striation of a muscular fibre from a human fœtus—second to third month. It is $\frac{1}{1400}$th of an inch in diameter. In it, also, the transverse striæ are seen as delicate streaks, at intervals, along its margin. *b*. From a human fœtus of the fourth month (four and a half inches long), illustrating the increase in breadth of primitive muscular fasciculi by means of the formation and coalescence of lateral bands formed under the agency of cytoblasts. In the human fœtus at this stage the voluntary muscles are perfectly developed, distinct, and surrounded by delicate fasciæ separating them from one another.

4. A fasciculus dissected into fibrillæ, after having been soaked for sixty hours in weak chromic acid. From a frog tadpole, one and three tenths of an inch long. Shows the fibrillæ to consist of a linear series of spherical particles connected together by blastema, and bounded along the margins by the same. *a*. Fibrilla $\frac{1}{16400}$th of an inch in diameter. The "clear spaces," on altering the focus, seem darker than the intermediate sarcous substance ("dark spaces"). *b*. Represents the spherical form of the "clear spaces" in the primitive fibrillæ. *c*. Owing to the globularity of the light particles, with an intermediate focus, the sarcous substance ("dark spaces") has an injutting appearance.

5. Muscular fasciculus from biceps of a human fœtus of the ninth month. The transverse striæ ("dark spaces") are broad, strongly marked, and bounded by dark smooth margins. This appearance closely resembles that of the fasciculi in insects. (Compare with Pl., fig. 14.)

6. Fasciculus tinged with carmine, from the intercostals of a human fœtus of the ninth month. It shows the cytoblasts of the sarcolemma lying on the surface, and illustrates the development of that membrane.

7. Diagrammatic representation, illustrating Dr. Dobie's view of an appearance occasionally presented by the muscle of adult animals. Some of the "dark spaces" are crossed by a clear streak, while the "light spaces" have a dark line passing across them.

FIG.

8. The sarcode, or contractile substance, of the common fresh-water sponge (*Spongilla fluviatilis*), consisting of a molecular blastema in which cytoblasts are imbedded.
9. Vertical section through the contractile substance of a common polyp (*Cyanea aurata*), showing delicate fibrils enclosing cytoblasts. An appearance analogous to that of the second stage in the development of the striped muscle of the Vertebrata. *a.* Cytoblasts, same as in preceding fig.
10. Muscular fibres of limpet (patella). They appear to the naked eye as bluish-white glistening bands, like the ligamentous textures of the Vertebrata; and on examination in their interior is seen a delicate, well-defined, longitudinal striation.
11. Fibre from the muscle of leech (*Hirudo sanguisuga*), after Lebert ('Ann. d. Sc. Nat.,' iii, p. 216). Presents very delicate transverse striæ.
12. Muscle of worm (*Lumbricus terrestris*). Delicate striæ, as in the preceding figure, are seen; but on being dissected into fibrils (x) these striæ are no longer observed. This transverse striation is, therefore, probably sarcolemmatous, and consisting of foldings or creasings of that membrane.
13. Fasciculus and fibrillæ from muscle of wild bee (*Bombyx Hera*), after Lebert ('Ann. d. Sc. Nat.,' iii, p. 216). The fibrillæ (o) resemble those of primitive vertebrate fibrillæ, in which the "clear spaces" are spherical.
14. Muscular fasciculus from house-fly (*Musca domestica*). The transverse striæ are well marked, dark, and with sharply defined margins, like those in the fasciculi of the human fœtus at the ninth month.

Art. II.

Post-mortem appearances in cases of Sudden Death from Pulmonary Apoplexy. By Francis Ogston, M.D. University of Aberdeen.

The frequency and importance of the so-called pulmonary apoplexy as an immediate and direct cause of death in various diseased states of the body has been forced on the notice of the writer on several occasions, while engaged in the investigation of cases of sudden death occurring under unexplained or suspicious circumstances. Believing that the observations thus accumulated in the course of his medico-legal practice might be deserving of the attention of the profession, it is now proposed to place his cases on record, confining the narrative in each instance to the appearances in the bodies examined which were of a purely pathological character, or which had a bearing on the particular mode of death. One appearance of this last sort will, however, be omitted for obvious reasons, viz., the state of the surface, each of the bodies inspected exhibiting more or less of pallor of their non-dependent, with less than the usual lividity of their dependent, parts. It may also be premised that the only order adopted in the bringing forward of the cases has been to place the less severe of these first.

Case 1.—Alexander B—, æt. 64, found dead in bed. Interior of the air-passages dull red. Lower lobe of left lung apoplectic. Blood coagulated, and in sparing quantities in the left and abundant and fluid in the right cavities of the heart. Fatty degeneration of the walls of the heart. Liver, spleen, and kidneys bloodless; liver and kidneys fatty.

Case 2.—Alexander S—, æt. 60. Found dead in bed. Body emaciated. Brain pale. Close recent adhesions of the left lung to the chest and diaphragm. Left lung generally apoplectic. Right cavities of the heart distended with clotted blood; less blood, but of the same character, in its left cavities. Fatty degeneration of the heart. Nutmeg liver; waxy kidneys.

Case 3.—Christian F— or C—, æt. 64. Found dead. Interior of the trachea reddened. Recent adhesions of right lung to the chest and diaphragm. Whole of left lung apoplectic. Blood in the right cavities of the heart; its left cavities empty. Coffee-ground fluid in the stomach.

Case 4.—Mary K—, æt. 66, a bedridden pauper. Found dead in bed. Body much emaciated. Clotted blood in the mouth. Brain unusually bloodless. Fluid blood in the left and a bulky fibrinous clot in the right cavities of the heart. Nine fluid ounces of reddish serum in the left pleural cavity. Œdema of the right lung. Lower lobe of left lung apoplectic. Dilatation of the aorta. Liver hypertrophied and bloodless. Walls of the right kidney thinned, and its interior filled with clear serum. Fatty degeneration of the left kidney. Intestinal tract pale and empty.

Case 5.—Burnett C—. æt. 55. While confined by an illness of a few days' duration, and which had not been considered of a serious character, was found dead in bed. Small quantities of dark fluid blood in the mouth, trachea, and larger bronchi. Dark fluid blood in the right and in sparing quantity in the left cavities of the heart. Black vomit in the stomach. Peritoneal coat of the intestines highly injected. Lower lobes of both lungs apoplectic throughout. Rather more blood than usual within the head.

Case 6.—Mary C—, æt. 16. Found dead in bed. Had been previously in apparent health. Blood in the left nostril; pupils broadly dilated. Tongue protruded, and marked by the teeth. Cerebral sinuses turgid. Blood and frothy mucus in the mouth, throat, and trachea. Lengthy adhesions betwixt the left lung and the chest and diaphragm; upper lobe of the left and the lowest lobe of the right lung apoplectic; the remaining lobes of the lungs congested and œdematous. Right side of the heart distended with dark fluid blood; its left side comparatively empty. Liver and kidneys congested. Serous cysts in both ovaries. Mammæ and genitals undeveloped.

Case 7.—Alexander F—, æt. 61. Found dead. Pupils broadly dilated. Bloodlessness of the mass of the brain; a soft clot of blood in its lateral ventricles, with softening of their walls; an aneurismal dilatation of the left vertebral artery within the skull, of the size of a pea. Right heart distended with dark fluid blood. Blood, of the same character, in sparing quantity, in the left heart. Middle and lowest lobes of the right and lower lobe of the left lung apoplectic. Hypertrophy of the left ventricle of the heart, and ossification of its mitral and sigmoid valves. Dilatation of the aorta.

Case 8.—Daniel M—, æt. 55. Found dead in bed, to which he had retired a few minutes previously, apparently in health. Body somewhat emaciated; pupils dilated. Rounded ecchymosed patches on the inner face of the scalp. Contents of the skull paler than usual. Frothy mucus in the trachea. Right heart much distended with dark fluid blood. Dilatation of the aorta. Old adhesions in the surface of the right lung. Middle and inferior lobes of the right and lower lobe of the left lung apoplectic; upper lobe of the right lung œdematous. Fatty degeneration of the left kidney; a calcareous calculus in the right supra-renal capsule. Lower and intestinal tube anæmious.

CASE 9.—David C—, æt. 65. Found dead in a field. Bloodlessness of the cranial contents. Blood, in some quantity, in the throat and air-passages; a large fibrinous clot and dark fluid blood distending the right heart; a sparing quantity of blood in the left heart. Upper and inferior lobes of the right and the lower lobe of the left lung apoplectic. Heart much loaded with fat. Considerable dilatation of the aorta; a little bloody fluid in the stomach. Pallor and bloodlessness of the abdominal viscera generally.

CASE 10.—John G—, æt. 45. Found dead on the floor of his room. Blood at both nostrils. Shirt wet with urine. Sparing quantities of blood in the mouth, throat, and upper part of the trachea. Reddish frothy mucus at the bottom of the trachea. Blood, partly fluid, partly clotted, distending the right heart; a comparatively small quantity of blood in the left heart. Inferior lobes of both lungs apoplectic. Liver and kidneys congested.

CASE 11.—Jessie B—, D—, or R—, æt. 60. Found dead in bed. Previous complaint of sickness. Whole of the right and portions of both lobes of the left lungs apoplectic. Dark fluid blood copiously in right and sparingly in left cavities of the heart. Enlargement and fatty degeneration of the heart, and its right cavities dilated and attenuated. Contraction of the colon.

CASE 12.—Christina L— or P—, æt. 56. Found dead in a chair in her room. Pallor of the mouth, throat, gullet, and air-passages. Fluid blood and fibrinous clots in about equal quantities on each side of the heart. Both lungs closely fixed by pale adhesions to the chest. Extensive pneumonia of left lung; middle and lower lobes of right lung apoplectic. Liver and kidneys small and fatty.

CASE 13.—George K—, æt. 49. Found dead in bed. Pupils dilated; brain pinkish. Trachea reddened. Upper lobes of both lungs emphysematous; the remaining lobes of both lungs apoplectic. Right heart full of fluid blood, and its wall attenuated. Kidneys pale.

CASE 14.—Lawrence P—, æt. 24. Found dead in bed. Numerous puncta vasculosa in the brain. Uniform deep redness of the interior of the mouth, throat, œsophagus, and trachea. Fluid blood in the trachea. Right lung adherent to the chest. Left heart empty. Blood, partly fluid, partly clotted, in the right heart. The whole of the left and the lower lobes of the right lung apoplectic. Heart flaccid, pale, and fatty. Liver and kidneys fatty.

CASE 15.—John H—, æt. 75. Seen to drop down dead in the street. Pupils dilated. Fluid blood abundant in right and in sparing quantity in left heart. Walls of the heart generally attenuated. Old adhesions of right lung to the chest. Upper part of left lung emphysematous; the remainder of the organ apoplectic. Liver bloodless. Right kidney granular. Left kidney congested.

Case 16.—William K—, æt. 58. Found dead in bed. Less blood than usual within the head; a thin layer of blood in the mouth and air-passages. A little bloody serum in the pericardium. Fluid blood, in about equal quantities, on each side of the heart. Heart a good deal loaded with fat. Three ounces of fluid blood in the right cavity of the chest. Both lungs, except at their summits, apoplectic. Contents of the abdomen bloodless.

As bearing on the pathology of the disease, the four following cases may be subjoined, although in these the state of the lungs formed but a subsidiary element in the final results.

Case 17.—Catherine McK—, 65. Found dead at the foot of a stair, from the top of which it was believed that she had fallen while in drink. Thin layers of clotted blood under the scalp and dura mater over the upper back part of the head. Linear fractures traversing the occipital and the squamous plate of the left temporal bones. Muco-purulent fluid in the bronchi. Lower lobe of left lung apoplectic. Right cavities of the heart distended with dark fluid blood. Heart much enlarged; its walls fatty; its left side hypertrophied; its right attenuated; cretaceous matters in the mitral valve.

Case 18.—David S—, æt. 60. While in a state of mental depression swallowed a very large dose of arsenic. Assistance having been speedily procured, free vomiting was induced. When seen by the reporter, about twelve hours after the taking of the poison, his appearance did not seem to indicate immediate danger. About three hours subsequently to this, however, after it was thought that he had been for some time merely asleep, he was found dead in bed. Fatty degeneration of the arteries at the base of the brain. Blood, in about equal quantities, on each side of the heart, that on the right coagulated, that on the left fluid. Fatty degeneration of the heart, with warty fringes on its mitral and tricuspid valves. Enlargement of the ascending aorta, and thickening of its valves. Upper lobes of both lungs emphysematous. Lower lobe of left lung hepatized. Middle and inferior lobes of right lung apoplectic. Interior of the stomach intensely reddened. Liver and kidneys highly congested.

Case 19.—James H—, æt. 45. Had swallowed strychnia (quantity unknown), with a suicidal intention. Survived four hours, affected with continued convulsions of considerable severity. From two to three drachms of blood, partly fluid, partly clotted, in the left lateral ventricle of the brain. Blood in the mouth, throat, and trachea; in some quantity in the last situation. Left lung and inferior lobe of the right lung apoplectic. Fluid blood, in quantity, in the right and sparingly in the left heart. Slight injection of the duodenum.

CASE 20.—L—, a foreign seaman, æt. 48. Body found in water. In addition to the usual indications of death by drowning, fluid blood was encountered in the mouth; there were capillary ecchymoses on the inner face of the scalp, on the pericranium, and under the outer coats of the lungs, kidneys, and spleen; and the lower lobe of the left lung was apoplectic.

REMARKS.—From the narratives just concluded it will be seen, that the proportion of males to females in the cases, was as 15 to 5; or, omitting the last four, as 12 to 4. The average ages of the two sexes approximated pretty closely, giving 55·3 years for the males, and 54·2 for the females; or, leaving out the last four cases, 56·4 for the former, and 55·3 for the latter. One male alone was in the prime of life (24), while one female was only 16. Omitting these two exceptional cases, the average ages of the remainder stand at 53·6 years for the males, and 63·7 for the females, or, omitting likewise the last four cases, as 62·09 for the former, and 63·7 for the latter.

As, with the exception of case 18, the parties examined had not been seen prior to their deaths, their previous histories could only be partially gathered; in all, however, without exception, it was ascertained pretty accurately that these parties had either been of intemperate habits or of enfeebled constitutions, or both combined. The reported character of intemperance, however, it may be remarked, with one exception, applied only to the males.

The morbid condition of the head, in all but one of the cases (case 16), could, it is obvious, have had little or no bearing on the fatal event. Even in the solitary instance of serious injury, the fissures in the cranium and the consequent effusions of blood both outside and inside the skull, though ultimately likely to have destroyed life, were not of such a character as to have done so at once, apart from other accompaniments which had not occurred at the time of the woman's death. In no fewer than eight of the cases, the state of the brain and its coverings was set down in the notes of inspection as natural. In the remaining eleven cases (omitting case 17) the state of the head was as follows, viz.—

Pallor or bloodlessness of the brain in .	6 cases.
Turgidity of the cerebral sinuses in . .	1 case.
Brain pinkish in	1 „
„ showing numerous puncta vasculosa in	1 „
Blood in the lateral ventricles (with, in one of these, softening of the ventricular wall) in	2 cases.
Fatty degeneration of the arteries at the base of the brain in	1 case.
Capillary ecchymoses on the inner face of the scalp and interior of the pericranium in .	1 „
Aneurism of one of the vertebral arteries within the head in	1 „

The condition of the mouth, throat, and air passages exhibited some results different from what was perhaps to be anticipated. Thus, in the notes of inspection these are set down as natural in five of the cases, and in one as pallid. In these parts, however, blood was found in eight of the remaining cases as under—

In the mouth alone in	2 cases.
Simultaneously in the mouth and air passages in	2 „
„ in the nostrils, mouth, throat, and air passages in	2 „
Simultaneously in the throat and air passages in	2 „

In three cases the lining membrane of the trachea was uniformly reddened; in one of these the redness extending to the mouth, throat, and œsophagus, coincidently with blood in the trachea. The only other point noted regarding the air tubes was the presence in these of frothy mucus in two, and muco-purulent matters in one.

It is important to remark, before leaving this point of our subject, that though in eight of the cases blood was found after death in the mouth, throat, or air passages, careful inquiries brought out the fact that in only one of the instances (case 4) had the fatal event been preceded, recently at least, by hemoptysis; thus confirming and extending the statements of Laennec, Rokitansky, Stokes, and others, that there may be apoplectic effusion into the substance of the lungs without the spitting of blood. It may be noticed in addition, that only in one instance (case 19) was the blood in any quantity between the mouth and the lungs, while there was little more of it in any case than just enough to moisten the parts in which it was found.

The morbid condition of the lungs which characterised the whole of the above cases was that which has been simply and briefly put down in the narrations as apoplexy. As witnessed in these, it was at the same time observed to present both essential points of agreement and some obvious points of difference, when compared with the diseased state of the viscus to which the term was first applied by Laennec

('Diseases of the Chest,' Forbes's translation, 3rd ed., pp. 188-9), while in substantial agreement with Rokitansky's descriptions ('Pathological Anatomy,' Syd. Society's ed., vol. IV, pp. 65-6). This will be best seen by a statement of the appearances of the affected portions of the lungs encountered in the preceding cases. These portions of lung showed increased bulk, though not to any marked extent. The colour of their surface was darker and more uniform than that of the unaffected portions of the lungs. The morbid parts had lost their natural spongy feel, no longer crepitating on being cut or handled. On dividing them with the knife, their cut surfaces had the appearance of perfect smoothness and almost of polish. The colour of the interior of the diseased lungs, as thus exposed, was of a very dark red, approaching to black; and the different slices, when isolated, had the consistence of a very firm clot of blood. On submitting such slices of lung to firm compression, a moderate quantity of dark grumous blood could be made to exude, leaving their surface roughened or slightly granular. From the degree of firmness with which the effused blood had coagulated in the morbid parts, it was found impracticable to determine satisfactorily as to the effect of the effusion on the parenchyma of the lungs. It did not appear, however, that any distinct rent of their substance had taken place, to any extent at least; nor was the line of demarcation betwixt the effusions and the surrounding portions of the lung very definite or distinct, though the periphery of the diseased portions could be made out—if not by the eye, by the comparative facility with which the blood could be expressed from the parts furthest from the centre of the effusion, restoring to the lung at these parts a good deal of its natural sponginess.

The unaffected portions of the lungs in the cases narrated exhibited, it will have been seen, little or nothing bearing on the production of the apoplectic state of these organs. These admit of being summed up as under, viz.—

Adhesions (partial) to the chest in . .	4 cases.
Emphysema (partial) in	3 ,,
Effusions (on one side) into the pleural cavities, one of serum, one blood in . . .	2 ,,
Œdema (partial) in	2 ,,
Pneumonia (partial) in	1 case.
Hepatization (partial) in	1 ,,
Capillary ecchymoses in	1 ,,

While the apoplexy in eleven of the cases affected both lungs, to a greater or less extent, it showed a preference for the left lung nearly in the ratio of 2 to 1. Again, as regards the individual lobes of the lungs, those on the left side were apoplectic, as compared with the right, in more than double the same ratio. This morbid condition was far more frequent in the lower than in the upper lobes in both lungs. Thus we have apoplexy

Of the upper lobe of the right lung in	.	.	4 cases.
„ middle „	„ „ in	.	. 6 „
„ inferior „	„ „ in	.	. 11 „
„ upper „	left „ in	.	. 6 „
„ lower „	„ „ in	.	. 18 „

If the results of the above inspections are to be taken into account, the assumed connection between diseases of the heart and pulmonary apoplexy, in their relations of cause and effect, is not wholly borne out. Even if we include the morbid state of the aorta—we have the presence of disease of the central organ of the circulation in only thirteen of the twenty cases, though the proportion rises if we include cases 17 to 20 as special instances, independent altogether of ordinary causation, affording here a ratio of 11 to 5. Taking the whole twenty cases, however, as it does not much effect the result, we have—

Dilatation of the aorta in	5 cases.
Fatty degeneration of the heart in . . .	5 „
The heart loaded with fat in	3 „
General enlargement of the heart in . . .	2 „
Attenuation of the walls of the right heart in .	2 „
„ of the walls of the heart on both sides in	1 case.
Hypertrophy of the walls of the left heart in .	1 „
„ of the left ventricle in . . .	1 „
Dilatation and attenuation of the right heart in	1 „
Disease of the mitral valve in	3 cases.
„ „ tricuspid valves in . . .	1 case.
„ „ aortic „ in . . .	2 cases.

The results thus arrived at, as respects the state of the heart in its bearing on the causation of pulmonary apoplexy, certainly affords no support to the views of Rokitansky on this point, who, it will be remembered, lays it down that, "This form of apoplexy is very frequently found to be associated with active dilatation of the right side of the heart, and seems to bear the same pathogenetic relation to this cardiac affection as cerebral apoplexy bears to active dilatation of the left side of the heart" (op. cit. p. 67).

The states of the abdominal viscera, as disclosed by the inspections, do not call for lengthened observation.

In the arsenical poisoning (case 18) the phlogosed condition of the stomach was, of course, traceable to the swallowing of the poison. In the remaining nineteen cases, while no fewer than sixteen are set down in the notes of inspection as natural or healthy, the stomach in two of the others was found to contain coffee ground fluid, and the third fluid blood.

As regards the intestinal tube, it was found that while its peritoneal surface in one (case 5) was found inflamed, the duodenum injected in a second (case 18), the colon contracted in a third (case 11), and the whole canal pale and bloodless in three (cases 4, 8, and 9), in all the others it was set down as natural.

The liver, again, appears as—

Natural in	9 cases.
Bloodless in	5 ,,
Congested in	3 ,,
Fatty in	4 ,,

The spleen as

Natural in	16 ,,
Bloodless in	3 ,,
Ecchymosed in	1 case.

And the kidneys

Natural in	6 cases.
Fatty in	4 ,,
Waxy in	1 case.
Congested in	3 cases.
Bloodless in	3 ,,
One serous cyst, the other fatty in . . .	1 case.
One granular, the other congested in . .	1 ,,
Ecchymosed in	1 ,,

Finally, it will have been observed that, as far as could be gathered from this source, the distribution of the blood betwixt the two sides of the heart pointed to asphyxia as the mode of death in sixteen of the cases, and asthenia or syncope in the remaining four; or, if we leave out the four last, as mixed ones, we have the indication of death by asphyxia in fourteen, by syncope in only two of the cases. The cause of this exception to the general result it is not difficult to perceive; for, by turning to the narratives, we find that in the two deaths by syncope (cases 12 and 16) the heart in one showed the previous existence of fatty degeneration, and in the other not only fatty degeneration, but, in addition, general enlargement of the organ, and dilatation and attenuation of its right cavities. That, after eliminating this source of difference, the state of the circulation in the heart should have pointed to death by asphyxia in fourteen out of sixteen cases was what was to have been anticipated. The effusions of blood into the interior of the lungs could not have failed to interfere with, and from their extent speedily to arrest, the respiratory functions of these organs; nor is it difficult to perceive why it was that others of the *post-mortem* indications of asphyxia were but little marked, or sometimes even are wanting in some cases. To do this it is only necessary to take into account the quantity of blood withdrawn from the circulation and fixed in the lungs as a sufficient explanation of the bloodless state of the cutaneous surface generally, in all, and of some of the more important viscera encountered in so many of the inspections.

Art. III.

An Attempt to remove the Difficulties attending the application of Dr. Carpenter's Theory of the Function of the Sensori-motor Ganglia to the common form of Hemiplegia. By W. H. Broadbent, M.D., Assistant-Physician to St. Mary's Hospital, and Lecturer on Physiology at the School.

This theory, as is well known, is, that the thalamus is the organ of conscious sensibility, to which all impressions made on peripheral sensory nerve-fibres must be transmitted in order to be recognised as sensations, and the corpus striatum the organ or instrument of voluntary motion,—the downward starting point of volitional motor impulses, or it might be said of all cerebral motor impulses. These two ganglia are again associated according to the theory of Dr. Carpenter in sensori-motor action, impressions reaching the thalamus being passed on to the corpus striatum, and giving rise to automatic movements differing from those which have their centre in the cord, only in being accompanied by sensation.

The common form of hemiplegia is caused by hæmorrhage, or softening in one or both of these bodies on one side, and the difficulties presented are: that the thalamus and corpus striatum being considered respectively the organ of sensation and motion of the opposite half of the body, the motor paralysis is not general in that half, but affects only the limbs, tongue, and face, while sensation either escapes altogether or is only partially lost. These difficulties have hitherto prevented the theory from obtaining general acceptance.

As different interpretations of the results of experimental and microscopic investigations of the spinal cord and medulla oblongata have been given by different physiologists, it is necessary to state briefly the views I hold as to the function and mechanism of these parts.

The gray matter of the cord then I look upon as containing a series of sensory and motor nerve-nuclei connected together transversely, and fused longitudinally into a continuous chain. With the posterior and anterior nerve-roots they constitute an apparatus for automatic or reflex action. From the sensory nerve-nuclei communications pass upwards in the gray matter to the thalamus, these crossing in the cord; to the motor nuclei, fibres descend in the anterolateral white columns from the corpus striatum, decussating at the point of the medulla. The posterior white columns are longitudinal commissures between the superimposed nuclei for co-ordinated movements.

The medulla oblongata is simply a more highly specialised portion of the cord, but has in addition to sensory and motor nerve-nuclei certain accessory ganglia. The constituents are also re-arranged. The nerves given off from it having special local distribution and functions of great importance, the nuclei are large, and distinct one from another. The reflex actions also which have their centres here, involve the co-ordinated action of an extraordinary number of muscles, and are of the highest importance to life; the provision for this co-operation by means of commissural fibres must be of corresponding extent. But this localisation, or individualisation of nerve-nuclei, and linking together of sensory and motor nuclei confers upon them no new property. Voluntary motion does not originate in a motor nerve-nucleus of the medulla, and sensation is not recognized as such by a sensory nerve-nucleus. Each is connected with the corresponding sensory or motor ganglion, thalamus or corpus striatum, just as are the nerve-nuclei of the cord, the communicating fibres crossing the septum, and ascending in the opposite half of the medulla and pons.

These remarks may also be applied to the pons, so far as concerns nerve-nuclei and the longitudinal tracts of fibres.

The thalamus being thus looked upon as the seat of sensation, and the corpus striatum as the instrument of volitional (ideational) action; the pons, medulla, and cord, being considered merely as subsidiary mechanism, the questions arise in hemiplegia, caused by injury to one or both of these bodies:

1. Why is not sensation more frequently and profoundly affected?

2. Why is not the entire half of the body, head, and neck paralysed as to voluntary motion, instead of merely the limbs, and in a partial degree the face and tongue?

The first of these questions has recently been put forward prominently by Dr. Hughlings Jackson, in vol. ii of the 'London Hospital Reports,' as one of the discrepancies between what he terms "medical" physiology and "school" physiology. He exaggerates the discrepancy, however, for he says, "As a rule, there is no loss of sensation anywhere." In opposition to this assertion, I might quote any number of authorities, and I can state from my own observation that sensation is very frequently diminished, sometimes very greatly. I have tested it by pricking and pinching, by the compasses, and occasionally by hot substances, the sound side being always used as a point of comparison. This does away with the idea that a subjective complaint of "numbness" has been set down as a loss of sensation. Moreover, it is not the sensibility of the limbs merely, where the motor paralysis is most marked, which is affected, but also of the face, chest, and abdomen.[1]

It is a fact, however, that in common hemiplegia sensation is never (so far as I know) totally lost, and that it is often altogether unaffected when the motor paralysis is complete, and this requires explanation. First, how is it that sensation so frequently escapes when motor power is lost, while the converse never occurs?

A reason for this, almost in itself sufficient, is found in the relative situation of the two bodies. The corpus striatum, the motor ganglion, is in front of, and external to the thalamus, and may be extensively damaged without involving the thalamus or the fibres passing from it to the cord. The thalamus, on the other hand, lying behind the corpus striatum, and upon the fibres connecting it with the cord, can scarcely be seriously affected without injury to these fibres or the corpus striatum itself.

Again, the thalamus, according to hypothesis, standing with respect to the corpus striatum in the relation of a sensory to a motor nerve-nucleus, it would almost follow that severe injury to the former would paralyse the latter by inhibitory influence, even when the injury was confined to the thalamus, and did not reach the corpus striatum directly or indirectly.

It still remains to be explained, however, why the loss of sensation is not as complete in degree when the thalamus is the seat of softening or hæmorrhage, as the loss of motor power in the limbs when the corpus striatum is affected.

A parallel is furnished by disease of the spinal cord, motion being almost always first and most profoundly affected, and both are explained by Dr. Brown-Séquard's experiments on the cord. He found a remarkable difference in the results of section of the white motor columns, and of the gray matter along which the sensory impressions travel. The cutting across of a group of fibres in the motor tract was followed by a certain appreciable muscular paralysis, but considerable injury might be done to the gray matter before any loss of sensation became apparent, and while a single slender bridge of gray substance remained a considerable degree of sensibility persisted in the whole of that part of the body behind the seat of the injury. The entire sensory tract resembles in structure this gray matter of the cord, and the thalamus itself instead of presenting like the corpus striatum distinct gray matter with white fibres plunging into it, consists of an intimate admixture of cells and fibres. Without pretending to explain this diffused transmission of sensory impressions along the cord, we may fairly suppose it to prevail in the

[1] I ought to add that my observations have mostly been made on recent cases, Dr. Jackson's on old cases, and that sensation is recovered more rapidly than motion.

higher part of the sensory tract, and to be shared by the thalamus. If this be admitted, it is clear that only such an amount of destructive change, as should leave no fragment of this body in relation with the sensory tract, would produce complete anæsthesia. We should, in fact, expect that injury to the thalamus would manifest itself rather in inhibitory paralysis of the corpus striatum than in marked loss of sensibility.

I have left out of this consideration the special senses, recognising, however, that their exemption constitutes a grave difficulty. If their nuclei, say of the gustatory or auditory, are centres of sensation independently of the thalamus, there is no reason why that of the trigeminus should not have the same property, which would be fatal to the theory of the office of the thalamus. If on the other hand, these nuclei are supposed to stand in the same relation to the thalamus as the sensory nuclei of the cord, the exemption of the special senses requires special explanation. This, I believe, may be given, but it will better form the subject of a separate communication, and the difficulty must be left where it stands.

Many physiologists locate sensation in the medulla or pons. Some even suppose, that all the sensory nerve-nuclei of the cord are centres of sensation. These hypotheses would explain the escape of sensibility in hemiplegia, but the loss of sensation often met with would furnish a greater difficulty than the one escaped from. I cannot now go into the objections which might be urged against these views, but pass on to the question respecting motor paralysis.

The difficulty here, as has before been stated, is, that the limbs, and in a less degree the face and tongue, only are paralysed, and not the entire lateral half of the body.

In attempting an explanation of this, it becomes necessary to specify the muscles paralysed, and more particularly to determine the precise character of the facial paralysis in hemiplegia. The contrast between facial paralysis proper and facial hemiplegia has been frequently described. Dr. Todd accounted for the differences observed in the two instances, by supposing that in hemiplegia the motor division of the fifth was affected, the seventh escaping, but this view, I suppose, is not now held by any one who has given attention to diseases of the nervous systsm. The distortion of facial hemiplegia, as in facial paralysis, is generally recognised as being due to paralysis of the seventh nerve, but as to the exact character and extent of this paralysis there is still diversity of opinion. Dr. Hughling Jackson, in the paper before referred to, says, "the paralysis is simply of a small part of the face near the angle of the mouth, not of the portio dura, but of part of it." It is true, that the paralysis is most evident in the part of the face referred to, but no muscle supplied by the portio dura altogether escapes paralysis, while, on the other hand, the paralysis is complete in none of them. It is not necessary to describe the expressionless condition of the affected side of the face, or the partial obliteration of the muscular markings; but it is important to observe that this extends in a slight degree to the forehead, where the wrinkles in a recent and severe case when well marked, will be found slightly smoothed out, and the vertical furrow produced by the corrugator supercilii a little less deep. The orbicularis oculi which furnishes the most striking point of contrast between "facial paralysis" and "facial hemiplegia," itself gives evidence of impaired power. For a short time after a severe attack, it is obviously weakened, and this may be rendered more evident by bidding the patient close both eyes powerfully, or again, by asking him to wink the eyes alternately. The eye on the hemiplegic side cannot be closed alone, and the voluntary contraction of the orbicularis of this side will be seen to be less forcible. On the other hand, it is equally important to note, that in the region about the angle of the mouth where the paralysis is most marked it is not complete. The dragging over of the mouth to the sound side is not so great as in paralysis of the portio dura, and imperfect movements may be observed in the paralysed muscles. All the facial muscles, therefore, are partially paralysed, though in very different degrees, none completely so. I should place them in the following order. Least of all the small muscles of the alæ nasi, the orbicularis oculi, the occipito-frontalis, and corrugator supercilii; next, at a long interval, the orbicularis oris; most of all the straight muscles going to the lips and angle of the mouth. That the orbicularis oris is much less affected than these last mentioned, is evident from the perfect closure of the mouth which can be effected. From this fact also, the attempt to whistle, or blow, sometimes directed for the purpose of showing the paralysis, rather diminishes than increases the facial distortion.

The muscles supplied by the portio dura, however, are not the only ones in the face which give evidence of paralysis. The masticatory muscles, though they take no part in producing the distortion of the features, do not altogether escape. If the patient is told to close the jaw firmly, the masseter and temporal of the sound side may be felt to come first into action, and to contract the more powerfully; sometimes those of the affected side only act after two or three attempts.

In the tongue, as in the face, there is evident, but incomplete paralysis, the only manifestation of it being deviation when it is protruded, usually towards the paralysed side.

The abdominal muscles may be made to show a certain degree of paralysis much in the same way as the masticatory muscles. In respiration they act on the two sides with perfect equality, these movements being automatic; but in other actions it may be seen that the muscles of the affected side are weakened. Thus, though an hemiplegic patient is able to raise himself from the recumbent to the sitting posture with little or no help from his sound arm, by means of the recti abdominales, and the hand placed upon the abdomen feels the muscles of both sides contracting powerfully, still as in the case of the masseters, those of the affected side are somewhat later than the others, and do not act with the same energy.

The limbs are the only parts in which motion is altogether lost. In a severe case, I have found not only every muscle of the arm paralysed, but the scapula perfectly motionless. The pectorals, the latissimus dorsi, and trapezius, the latter, in movements of the head also, were flaccid, and apparently the more deeply seated muscles, the serratus, rhomboidei and levator, were equally paralysed. In the leg, the loss of motor power is rarely so complete as in the arm.

These are the muscles paralysed. It is, however, even more necessary to specify the muscles which escape paralysis, since it is this exemption and not the fact of paralysis which requires explanation. The muscles incompletely paralysed also, will again have to be alluded to, the partial exemption having to be accounted for as well as the complete escape.

The muscles, then, which give no indication whatever of paralysis are those of the eye, neck, back, and chest. The movements of the two eyes are as perfect as ever; there is no paralysis, therefore, of the third, fourth, or sixth nerves of the affected side. As to the neck, it is sometimes stated that the sterno-mastoid is partially paralysed, but I have never observed this, and in cases where the use of the arm and leg has been entirely lost, I have found the rotatory and the backward and forward movements of the head to be executed as well as ever. Inclination of the head towards one or other shoulder does not seem always as easy as usual, but it cannot be said that any muscles of the neck give appreciable indications of paralysis. No difference again can be detected in the movements of the two sides of the chest, or so far as I have been able to make out in the back. There are other sets of muscles which, without showing any obvious paralysis may be made to furnish evidence of impairment of independent volitional action. These are the levator and orbicularis of the eyelid, the masticatory, and abdominal muscles, as already described. Others, again, are manifestly paralysed, but not completely, as the facial and lingual muscles.

In all these instances, it is the persistence of purely volitional motor power which is spoken of, and not automatic action of any kind. The reflex respiratory movements of the chest and abdomen of course go on, but there remains also the power of taking a deep inspiration, or making a forcible expiration at will, which is not reflex but voluntary. Again, there is the act of rising into the sitting posture executed by the abdominal muscles. So also in the case of the eyelids, in addition to the automatic winking which is not arrested, as in paralysis of the portio dura, there is the voluntary closure of the lids, more or less firmly. The movements of the eyes have been spoken of as "sensori-motor," but it is evident that they are only so in the same sense as all motion is influenced by a guiding sensation, and are as strictly voluntary as any movements in the body.

If, then, the corpus striatum is to be regarded as the centre of all volitional motor impulses, these exemptions, partial and complete alike, require explanation. So far as I know, this has only been attempted in the case of the facial and thoracico-abdominal muscles, and the explanation has usually turned on the confusion of automatic with voluntary movements.

With respect to the thoracico-abdominal muscles, the explanation usually given is somewhat as follows: "That their habitual and constant action being in the reflex respiratory movements, they are to some extent withdrawn from the influence of the will, and are consequently not affected when voluntary motor power is lost." This which has a very plausible sound will be seen on examination to be absurd. It is in effect, equivalent to saying, that because these muscles are comparatively seldom called into action by the will, it is reasonable to expect that they will still be reached by volitional impulses when other muscles more constantly acted upon by the will are completely cut off from its influence. The persistence of the reflex respiratory movements is easily understood—the mechanism on which they depend is not damaged, but this does not explain the voluntary action still found possible in the muscles of the affected side.

Van der Kolk employs a similar process of reasoning in attempting to explain the exemption of these muscles, by reference to the relation of the lateral columns of the cord with the nucleus of the vagus. These columns have been shown by the experiments of Schiff, to serve for the motions of the trunk, and therefore for the respiratory movements. Van der Kolk finds that their fibres terminate for the most part in the nucleus of the vagus, this connection being part of the respiratory apparatus. He concludes, further, that in consequence of this relation, the function of the lateral columns does not depend directly on the will, though to a certain extent influenced by it, but that they are brought into action specially by a stimulus from the vagus. That is, because these columns form part of an automatic apparatus of which the nucleus of the vagus is the centre, when they are called into action by a totally distinct power, volitional, this also has its seat in this nucleus. His own researches have shown this idea to be untenable. Fibres are found to pass upwards from the vagus, through which the will is supposed to influence the respiratory movements, and *a fortiori*, these, or an independent set of fibres, must be required for such actions as sitting up. When these fibres are cut across, as they must be in hemiplegia, if they are connected with the corpus striatum, voluntary control over the trunk muscles ought to be lost, whether they pass directly into the lateral column or influence it indirectly through the nucleus of the vagus. The persistence of volitional motor power in the thoracico-abdominal muscles in hemiplegia, therefore, is not accounted for by the relation between the lateral columns of the cord, and the nuclei of the pneumograstrics.

The most recent attempt to account for the partial character of the facial paralysis in hemiplegia, is by Dr. Saunders, in a paper in the 'Lancet' for 1865, vol. ii, p. 478.

After showing that it is the seventh nerve which is paralysed, and not the fifth, as was stated by Dr. Todd, he points out that the facial muscles have three distinct modes of action, as respiratory muscles, reflex—as muscles of expression, emotional, and as voluntary muscles, strictly speaking. He supposes that for each of these different kinds of action, the trunk of the portio dura contains a distinct set of fibres connected at its origin with different excitor centres, volitional, emotional, and reflex respectively, and that destruction of the volitional centre, or of the fibres leading to it, may leave untouched the emotional and reflex centres and communications, permitting the muscles to be called into action through these. Even, if this hypothesis be accepted, it affords no explanation. According to it, purely volitional power should be completely lost, but this is not the case as has been pointed out. A considerable degree of it remains, even in muscles supplied by the portio dura, and, as has before been said, persistence of automatic action does not explain the possession of voluntary control.

In order to be more explicit, and render the insufficiency of this explanation more clear, the orbicularis oculi may be taken as an illustration. The habitual winking movements of the eyelids are automatic—reflex, as they would be termed by some, or sensori-motor, according to others. But we have also the power of keeping the eyes shut at will for any length of time, and of closing them with varying degrees of force, these being distinct exercises of voluntary power. Applying Dr. Saunders's explanation, some of the nerve-fibres supplying this muscle will be connected centrally with the nucleus of the fifth nerve for the automatic action; others pass to the corpus striatum, and convey the volitional influence. These latter being cut across on one side in hemiplegia, the winking should go on as usual, but the power of forcibly closing the eye of the affected side should be lost. This is not the case, a certain degree of weakness may be apparent, but both eyelids can still be closed at will, and held down with considerable force.

Dr. Hughlings Jackson also, as I gather from a note to the article before referred to, supposes the portio dura to break up within the medulla and to proceed to different parts of the nervous centres, not, however, precisely on the same grounds as Dr. Saunders, but on account of the wide distribution and varied functions of the nerve. If this were so, it would still fail to account for the persistence of voluntary motion in any of the muscles supplied by it, when the centre of volitional action was destroyed.

But it is an ascertained fact, that the facial nerve does not split up in the way here supposed, and the varied actions, automatic, emotional, and volitional, can be explained without any such hypothesis. The nerve passes entire to its nucleus, and it is by the communications of this nucleus with the different excitor centres, that the various kinds of movement are brought about, with the nucleus of the vagus for respiratory movements, with the fifth for sensori-motor, and with the corpus striatum for emotional and volitional actions. The same fibres in the nerve-trunk convey the impulse from the nucleus to the muscles, from whatever centre the impulse may originally have been derived.

Dr. Jackson has again pointed out the interesting fact, that the muscles of the sense-apparatus escape as well as the special senses themselves, but they are not the only ones exempt, and it is not pretended that any satisfactory explanation is thus obtained.

We come back then to this point, that if the corpus striatum is to be considered the organ of volitional action, an explanation is still required of the incomplete paralysis of the opposite side of the body, neck, and face, when this body is the seat of disease.

The key to this, I believe, is to be found in a comparison of the muscles paralysed with those exempt from paralysis, as to their habitual action. A striking difference is at once noted. Thus the arms (in which the paralysis is complete), are entirely independent in their movements, the one of the other, are altogether dissociated in their action, and habitually engaged in totally different motions. The muscles of the trunk, on the other hand (which escape paralysis), act in pairs, are almost always bilaterally combined in their action, and the two sides engaged in similar and associated movements. We move one arm, or one leg, while the other is quiet, or executing a totally different action. We find it impossible to expand one side of the chest without the other, or to move one eye without the other, and extremely difficult to throw into action the muscles of one side of the abdomen without the other, impossible, indeed, to do this forcibly.

The parts paralysed then, are such as have the power of acting independently of the corresponding part of the opposite side. The muscles which escape, are those which act only bilaterally, or in concert with the corresponding muscles of the opposite side.

But when muscles habitually act together, and rarely or never independently of each other, the nuclei of their nerves are usually connected by commissures.

The hypothesis suggested by these considerations is, *That where the muscles of the corresponding parts on opposite sides of the body constantly act in concert, and act independently, either not at all, or with difficulty, the nerve-nuclei of these muscles are so connected by commissural fibres as to be* pro tanto *a single nucleus. This combined nucleus will have a set of fibres from each corpus striatum, and will usually be called into action by both, but it will be capable of being excited by either singly, more or less completely according as the commissural connection between the two halves is more or less perfect.*

The existence of this transverse commissural communication between corresponding nuclei is not hypothetical, the fibres have been observed and described, and the association effected by them is considered necessary to harmonious bilateral action, but so far as I know the use here attributed to them, that is of conveying to one an impulse received by the other, has not been suggested.

According to this hypothesis then, if the centre of volitional action of one side is destroyed, or one channel of motor power is cut across, the other will transmit an impulse to the common centre, and this will be communicated to the nerves of the two sides, equally, if the fusion of the two nuclei is complete, and there will be no paralysis,—more or less imperfectly to the nerve of the affected side, if the transverse communication between it and its fellow is not so perfect, in which case there will be a corresponding degree of paralysis.

This will be better understood when illustrated by examples. The nuclei of the two third nerves, for instance, are situated close to the median line, high up in the floor of the fourth ventricle, and are so intimately connected together that they may be considered as one single centre, each half of which receives fibres from the corpus striatum of the opposite side. Supposing now the right corpus striatum be injured, voluntary impulses from the left will pass to the right nucleus only, but the two nuclei being fused into a single centre, this is called into action equally throughout, and the muscles of the left eye act as perfectly as those of the right. In the case of the portio dura on the other hand, the communication between the nuclei is imperfect. Here, then, the same injury having occurred, the left nucleus receiving no impulse from its own motor ganglion, receives only an imperfect impulse through the partial communication between it and its fellow supplied by the uninjured corpus striatum, and the muscles supplied by it are partially paralysed.

Applying now the hypothesis generally, it ought to be found, first, that the paralysis in any given set of muscles is exactly proportionate to the individuality of their action, and their independence of muscles of the other half of the body.

And secondly, that when any set of muscles having a certain degree of independent action partially escapes paralysis through association with muscles of the opposite side, associated movemeuts only are possible on the affected side, and not independent unilateral action of those muscles.

I think it may fairly be said, conversely, that if these tests are complied with, they furnish strong evidence of the truth of the hypothesis. Taking then the different parts of the body in succession, and employing these tests, we find, as I have before stated, that the limbs which alone have perfect unilateral independence of action, are alone liable to complete paralysis in the common form of hemiplegia.

On the other hand, the ocular muscles are never paralysed, and these have no unilateral independence. The two eyes always move together. It is impossible to move one and keep the other fixed, or to turn one up and the other down. Individuals are occasionally met with who can at will exhibit a convergent squint, but this apparent exception to the rule is really only an exaggeration of an associated movement. The centres from which the motor nerves of the ocular muscles of the two sides proceed are inseparably joined, and if they act at all must act together. We thus also see why strabismus in cerebral affections implies something more than disease of the central ganglia.

In the case of the thoracic muscles, again, we have the same point illustrated; no unilateral independence of action; no unilateral paralysis. Between these extreme cases of complete paralysis and perfect exemption, we have the instances of partial paralysis already enumerated, which afford opportunities for the application of both tests.

The eyelids habitually act together both in the habitual automatic winking, and in the strictly voluntary movements already mentioned, but independent action of one without the other is not impossible. The power of winking one eye is however an educational acquirement, some learn it easily, others with difficulty, others again never master it, and it is not uncommon to meet with persons who can wink one eye alone, but not the other. In hemiplegia accordingly, paralysis is not very apparent in the eyelid. For a time after the attack it is weakened on the affected side, but to render this evident it is often necessary to tell the patient to close the eyes firmly. In accordance therefore with the first test, we have, with little unilateral independence, slight paralysis.

The second test is applied by bidding the patient close the eyes alternately. He will be unable to do this on the paralysed side, and in making the attempt it will usually be seen that the orbicularis of the sound side contracts, against the wish of the patient, while that of the affected side remains comparatively passive.

A comparison between the orbicularis oris and the straight muscles, levator, zygomatic, buccinator, &c., again is an illustration of the fact, that the paralysis is proportionate to the independence. It is very easy to draw one angle of the mouth in any direction, the other remaining in position—difficult to compress the lips firmly on one side only. Accordingly, as has been stated, the orbicularis is paralysed to a less degree than the straight muscles.

As to the remaining muscles supplied by the portio dura, in those of the forehead and brow, and of the ala nasi, there is little independence of action and little paralysis. In those of the cheek and lips we have increased independence and marked paralysis. The movements of these parts are usually symmetrical, and a certain degree of association of the nuclei is presumed to exist, the paralysis consequently is not absolute; but the two sides move independently of each other with perfect ease from the earliest period of life, this association of the nuclei therefore is only partial, and not sufficient to prevent distortion of the features.

The second test does not give such decided results as in the case of the eyelids: while the paralysis is such as to give rise to marked facial distortion there is no power of independent motion in the affected side; but this soon returns, and may be observed when the eye of that side cannot be closed alone. This may be to some extent due to the power of fixing the muscles on the healthy side.

I have already described the very slight indications of paralysis which may be obtained from the masticatory muscles. This is in exact proportion to the small degree of independent action they possess. The muscles of the two sides always act in concert, and any one making the experiment will find it impossible to exercise any considerable force by the masseter of one side without bringing into action that of the other, even if a hard body be placed between the teeth on the side attempting to act alone.

Again, in the abdominal muscles, the degree of paralysis and the power of unilateral action correspond very nearly with what has been stated of the muscles of mastication. It will be found almost impossible to throw the muscles generally of one side into powerful action without those of the other side. In inclining the body to one side or the other, the rectus abdominis will act without its fellow, and it is in similar movements that impaired power may be shown in hemiplegia.

The tongue furnishes a very interesting illustration of paralysis of unilateral action and exemption of bilaterally combined movements. This organ is usually perfectly symmetrical as to its lateral halves. They are elongated, shortened, flattened, or thickened together; the only unilateral movements are those in which the tongue is carried bodily from side to side and the tip pointed right or left, or in which one margin is depressed and the other raised. The apparent exception to this rule, when one edge of the tongue is rolled up, is produced by pressure of that side against the teeth, and is a result of the lateral motion mentioned. Accordingly, in hemiplegia, there is no loss of symmetry in the tongue; it is still flattened and elongated as a whole; there is no impairment of motility in one half, but there is deviation of the tip when it is protruded.

The complicated arrangement of the muscles of the back makes it difficult to apply the tests to them. It is however certain, that they give little evidence of paralysis—many of their actions are automatic, balancing movements, and the two sides almost always act in concert. Even when the body is inclined to one side, the muscles of one side are contracting, to regulate the movement and prevent falling over, while those of the other are employed in curving the spine. In putting a hemiplegic patient through these movements, he readily loses his balance, and it is difficult to make out any difference between the two sides. In attempting to raise the hip he leans over to the opposite side.

The only real difficulty is met with in the neck. As has been already stated, the head is rotated from side to side, or inclined forwards or backwards as readily in a patient with hemiplegia as in a sound man. The inclination towards one or other shoulder does not seem quite so easy or so perfect. The forward and backward movements would be accounted for by the bilateral action of the muscles engaged, the recti antici, and the muscles of the back of the neck. The rotatory movements do not admit of so easy an explanation. But it is to be noted, that although in rotation no two corresponding muscles are engaged, yet there are associated in it muscles belonging to opposite sides of the body. Thus, in turning the head towards the right shoulder, the left sterno-mastoid anteriorly, and the right inferior oblique atlo-axoid posteriorly, are brought into action, and *vice versâ*. Looking now at the nervous supply of these muscles, it will be seen that it is quite possible for their nuclei to be associated, and indeed very probable that they are, the sterno-mastoid receiving a branch from the spinal accessory, some of the twigs of which come off from the cord at the same level as the second cervical, which supplies the inferior oblique. According to this, then, the left sterno-mastoid in hemiplegia of that side will receive an indirect impulse through the nucleus of the second right cervical, and so escape paralysis. The association of nerves of opposite sides is not without parallel; as, for example, in the instance of the third and sixth; and the supposition of a relation between the nuclei of the second cervical and spinal accessory, would furnish an additional reason for the scattered origin of the latter.

As to the lateral movements of the neck they resemble the corresponding movements in the back, both sides acting, one limiting, the other effecting the motion.

I think I may say, that the facts here given furnish a considerable balance of evidence in favour of the hypothesis I have advanced. Its simplicity and its general applicability are of themselves recommendations, and I venture to hope that further observation will clear away the few difficulties which remain, and place it, and with it Dr. Carpenter's theory of the function of the corpus striatum, on a firm basis.

Some interesting corollaries might be pointed out, and the association of nerve-nuclei will be found to bear on other cases than those of hemiplegia, but any discussion of these would be premature.

Art. IV.

On the Connexion between Loss of Speech and Paralysis of the Right Side. By W. Moxon, M.D., Pathologist and Curator of Museum to Guy's Hospital.

It is, I think, not over venturesome to say, that no observations have for many years excited in the medical world more intense and general interest than those of M. Broca, upon the coincidence of loss of speech with paralysis of the right side, which have been brought before the profession in England by Dr. H. Jackson, in a comprehensive and able record of cases.

Whatever may prove to be the explanation which these observations will ultimately receive, they cannot fail to take an important place not only in the pathology of the brain, but also in the inductive part of the science of mental phenomena.

So intimately is language associated with the highest operations of the mind, that logic—the science of verbal propositions—has always either taken its place amongst the purely mental sciences, or has been held as an adjunct but little subordinate in rank.

On the other hand, the power of voluntary motion in the limbs, though of course in connection with the intelligence, being shared equally with the most lowly animated beings, is accepted as almost without the pale of mental science.

Yet in the cases under consideration, there is a very singular coincidence of a loss of the latter simply physical power, with a like loss of the intellectual power of using language. These two conditions being equally the effect of a local and mechanical lesion in the brain, whilst the sufferer shows unmistakeable evidence of his comprehending the spoken language of others.

The coexistence of this capacity of apprehending what is said with an entire inability to utter the very same words which are readily and fully understood, is very surprising to observe, yet there can be no doubt of its occurrence.

But the natural conclusion from the connection of a unilateral disease of the brain with loss of language, that language itself has its organ on one side only of the brain, is not a less astonishing result. For no animal, vertebrate or invertebrate, is an exception to the law, that all "organs of relation,"—all organs whose function is to communicate with or move in relation to the world outside the creature's body,—are bilaterally symmetrical. Even univalve mollusks, unsymmetrical as their bodies are commonly said to be, always have the eyes, tentacles, auditory capsules, "foot," jaws, and the nervous centres connected with all these bilaterally symmetrical.[1]

Knowing this, which is indeed perhaps the most general truth in all the science of animal construction, it is very surprising to meet with facts which seem to show that the highest expression of communicating organs, the organs of human speech, are one-sided and not symmetrical.

That a law so universal should, if I may so speak, suffer infraction by its highest subject, is so contrary to the course of nature, that most people, I believe, will expect some secondary explanation to be found, which shall re-establish harmony between the ground-plan of construction of the human organ of speech and that of animal communicating organs generally.

My object is to advance and to endeavour to support an hypothesis, which if valid will effect this last object, of reconciling the apparent unilateral development of the human language-organ, with the bilateral form of communicating organs generally, and to suggest probabilities which may throw some feeble light on the relation of mind, to the organs by which language is apprehended and expressed.

I would first notice, how very difficult it is to make one's hands execute opposite motions at the same time; let any one, as Dr. Carpenter says, try to make one hand revolve in one direction, whilst the other revolves in the opposite direction, and he will experience this difficulty.

Now let any one try to make them both revolve at the same time in the same direction, and it will be evidently the most natural thing in the world; in fact, his attempts at the first little experiment will generally resolve themselves into performances of the second.

This difficulty is not only felt in opposite motions, but also in

[1] I need hardly say that radiate creatures are no exception to this law, since radial symmetry is not contradictory of bilateral symmetry, it is that, and more it is bilateral symmetry about any diameter.

different motions of the hands. In learning to play upon the piano, a very long time is taken before the learner becomes able to give his attention to the hands separately, so as to make them perform different parts simultaneously, whereas the fingers of an unskilled person will move together upon the keys of the instrument freely enough so long as the movements correspond. They seem to have a will of their own to go together, but the difficulty of making them move differently at the same time is so great, that some aspirants fail to overcome it, others succeed very imperfectly, and it generally remains a sufficient difficulty to oblige the performer to play over a new piece a few times, to accustom the hands to work together perfectly their several parts.

The plain inference from these every day facts is,—(1) that the attention to one hand only is sufficient somehow to guide the other hand in the same motions at the same time; and (2), that it is very difficult, almost impossible, to give simultaneous attention to the two hands.

In short, that we have but one attention for two limbs.

To view it in another way: the difference of facility with like and opposite simultaneous motions, is a measure of the degree of attention saved to the hand which follows its fellow. For either the attention is equally divided between them, or it goes chiefly to one. The first of these alternatives is not satisfactory, for the whole attention in the case of simultaneous action is less than in diverse, the whole attention being in the latter case beyond our power; whilst in the former it is easily within it. If the attention be equally divided between the hands, then in the case of similar movement, each particular hand will receive a less degree of attention than the same hand receives in diverse motions. But there is no reason if each hand is equally the subject of attention why it should not require as much attention in moving one way as the other.

So that the sufficiency of a lower amount of attention in similar motions must be due to the one hand following the other in these motions, wholly or partially without attention, the will acting immediately on the first and on the second through the medium of the first.

Now as education is based on the memory of former acts, and as memory resides where attention was in action—memory being only the mark left in the brain by former acts of attention, being that which persists of former attention—education of similar simultaneous motions will grow up on one side, that on which the attention was operating; no education will grow on the side which habitually follows; attention never having been there, and therefore of course leaving no mark to constitute education.

The accumulation of skill or memory on one side would suffice for the two in consensual motion, whilst the accumulation of skill by attention alternately to either side would be unavailable for simultaneous motions, on account of the impossibility of using both stores by the attention at the same time. So that in regard of simultaneous motions of the two hands, there would be no reason for educating both sides, for it is more easy to educate one, and just as in actions which are performed by only one hand, one, the right usually, is alone generally educated for the performance of these actions, so in simultaneous motions, one being sufficient would be chosen, and the other habitually follow.

I will further endeavour to state my view by an easily conceivable but *imaginary* case: suppose that music were always played by the same motions of the two hands, much of the present difficulty of learning its performance would not arise, and the pupil would acquire skill in a short time. Suppose a pupil who had been educated in this easy consensual way, were to suffer such an injury that the combining power of the right hand should be destroyed; then I anticipate that pupil would no longer be able to execute with the left hand the same complicated and rapid motions as heretofore, because in all the course of instruction and practice the left was relying, if I may so speak, on the guidance of the right. The left hand would, I presume, be still able to go through ordinary simple motions, such as an untrained hand could execute, and perhaps something more, because the two hands *are separate*, and doubtless in even consensual practice, attention would occasionally pass over to the other hand. I must here state again, that I refer only to such an *imaginary* example, and not at all to the performance of music in the ordinary way, where the two hands are trained independently, and the damage of one would scarcely interfere with the other.

Now in this I am aware that I am not saying what is self-evident; and some, especially pianists, may differ from the opinion I have advanced in the above supposed case. For the two hands are accustomed to *alternate* with independence, and it is difficult to divest one's mind of the sense of freedom and independence which is set up by the common experience of this *alternate* freedom. Hence, some may not be satisfied that there would be such a loss of freedom with the left hand as I assume in my supposed case of consensual education. But we have before seen, that this alternate freedom goes with a wonderful incapacity of simultaneous freedom. And I think it will be allowed, that to the extent in which the left hand was helped by the guidance of the right, to the same extent it will be impeded by the deprival of that guidance. And we have before seen, that in simultaneous motions one hand depends for its guidance very largely on the attention directed to the other.

If this be true of the hands which are quite separate and accustomed to act alternately with entire independence, what would be the difference if the two were united, and never had been, under any circumstances, accustomed to other than similar and simultaneous motions; they being all the while quite as distinct in their construction and nervous supply as now.

Would not the result be, that one hand would always lead the other, from the utter want of any temptation to move them separately, the attention to one consensually guiding the other? Would not the very consciousness of possible independent motion of either side be lost, or rather, never be obtained?

Now the organs of speech are just in the same condition as the hands would be if united. There are just as much two tongues as two hands, save for this union of the tongues of the two sides down the middle, where they come in contact. In serpents the two tongues separate near their ends, and the tongue becomes forked, and in some parasitic crustacea (Lerneidæ), the ends of limbs are joined together in the middle line, and made to form a sucker. The tongue of either side is as distinct from its fellow tongue as the hand of either side from its fellow; each has its own entirely independent nervous and muscular construction. The only difference is, that the tongues are joined down the line where they meet, whilst the hands and arms are not so. But this difference is merely accidental, compared with the great fact of their distinctness.

In short, there are a right and a left tongue, distinct but not separate. We have one attention and two tongues, just as we have one attention and two hands.

We have only to perceive that the right tongue tends to guide the left, as the right hand tends to guide the left, and to make a fair allowance in comparing them with the hands which feel so independent, for the fact that they are joined and never moved independently, so that none but a consensual motion ever occurs; and we shall, I think, see how attention, and therefore memory, and therefore education, may never reach the left tongue at all, or at least, that if simple attention does go to either side indifferently, education would only grow up on one side on the principle before stated, in that it is in short much less trouble to educate one than both, and both educations never could act together, whilst the tongues always must move together. So that all that store of recollections of movement-associations which constitutes the power of speech, will be localized in the left brain, which corresponds to the right tongue, there being no remembered results of attention, that is, no education on the other side where attention never operated. Yet, in the right brain there will be all the organs, which if educated would become the seats of speech-power; so that the ground-plan symmetry of the organs of speech is preserved.

The view here advanced has a very close analogy with that many years ago put forward on other grounds by Valentin, respecting the motions of the eyes. That physiologist showed it to be at least probable, that in the common motions of the eyes, which are equally partaken by both, the will, or as I have used the word attention hitherto, I may as well say the attention, acts really upon one of them only whilst the other follows consensually, in virtue of a natural arrangement which he explains in detail. His views were very generally adopted, and are, I believe, still current.

That the attention to the motions of the eyes must lie on both sides of the brain, follows of course, from the fact that each eye leads towards its own side, so that the initiative is taken by each upon occasion. But it curiously happens, that in the motions of speech all the movements take place in the middle plane, in all languages the two tongues acting just equally, and the motion being as well originated by the one side as the other, and therefore possibly by one side alone. This at least gives a negative corroboration to the theory I am endeavouring to support. What I have here advanced must, I think, be allowed to have shown that important grounds lead to the belief, that the educated associations of movements which constitute spoken words, take place only on one side of the brain; this education constitutes the power of speech, as contradistinguished from the power of apprehending language, and the power of conceiving thoughts.

I admit that up to this point the proof is not complete, but only amounts to a high probability. It awaits a test such as should show that damage to one side of the brain is always associated with loss of speech, whilst damage to the other is equally always unassociated therewith.

This test is constantly repeated in the cases which I adverted to in the commencement of my paper. There is now a vast accumulation of recorded experience, which is daily increasing, and which is practically without exception, in testifying that paralysing injury to the right side of the body (situated on the left side of the brain), causes loss of speech, whilst paralysing injury to the left side of the body (situated on the right side of the brain), does not cause loss of speech.

There must be some cause for this, and I advance the foregoing facts and reflections, in the confident belief that they will satisfy those who entertain the subject, that the organ of so much of the process of speech as is above mere mechanical motion and below the mental conception of the thought, that is, of the part due to those associations of lip and tongue movements whose growth is dependent on education, and whose operation must precede the mechanical motions of speech, is *developed* by education on the left side of the brain, whilst on the right side corresponding parts remain in an undeveloped condition.

It has been shown above, that in proportion as movements are educated is their performance restricted to one side. In the case of manual dexterity this is matter of universal experience, except—(1)

where the two have to perform different motions simultaneously; or (2), the rare cases of ambidexterity, in both which cases the double skill is acquired by attention alternately to the hands to each for the time alone. It has been shown, that as the two tongues which compose the apparently single member are joined, and their actions always simultaneous, such alternating attention would, from the consent of the two sides in simultaneous motions, never be wanted, except when in lateral motions, the tongue of either side may have to take the initiative in movements towards its own side; and further, that the movements performed in acts of speech are always in the middle plane, and such that the initiative of either side indifferently would, if the other move consensually suffice for all movements of speech.

I now wish to draw attention to the lengthy education which is passed through by the child before the power of speech is acquired. Long after a child understands the language of others, it is quite unable to utter words itself. The motions of letter-pronunciation are casually made in accidental actions of the tongue and lips, but the attention of the child has to be long bent to the speech-movements of others, and to its own efforts, before it learns to associate the successive movements of a word's pronunciation. The actions of the tongue and lips have to be welded into unity, so that the succession of movements comes to be regarded as a unit by the attention, the coalescence of the several movements being due to, and dependent on, the remembrance of those past acts of attention by whose operation they were gradually and slowly combined. Now if we contrast the pains, difficulty, and slowness of the original acquirement of speech, with the velocity and ease with which in common conversation we pass not from word to word only, but from sentence to sentence, (almost) without a thought to the movements which must each be executed by the tongue and lips, in order to their utterance, we may gain some idea of the inconceivable pitch of education which is given to those supra-motory departments, if I may so call them, of the brain, which hold ready for use the memorial forms of outgoing words.

We cannot perhaps conceive in what shape these *ideas of associated movements* persist, but it is quite certain that they do persist, and that in such perfection, that in ordinary speech the word or even the part of a sentence to which we are accustomed, comes to the tongue without the attention of the mind to the particular movements required, and often with an inappreciably low degree of attention to the word or sentence, as when long strings of sentences are muttered unconsciously by one absent in mind.

Wherever and whatever the local seat of this education may be in the brain, it will be seen, that if the seat of these ideas of associated movements be destroyed, then the person so injured will be thrown back into the condition of a child, who has learnt to understand language but not to speak it.

An adult person thinking upon his own faculty of language will probably come to the conclusion, that language is coextensive with thought, as many hold—for a thought is embodied in its appropriate word in a healthy mind before it assumes an appreciably definite shape, and thoughts are thus put into words before they can be subjected to reflection.

But many facts go to show that the relation of the mind to incoming language is entirely different from its relation to outgoing language. A child learns to understand language before it can speak it. Most people understand 3000 words when they hear them, but few use more than 300 words in their conversation.

Almost every one will have experienced temporary difficulty in the use of familiar words. A word seems to slip from the tongue, whilst the hearing of that word will at once give its full meaning to the mind; the way in is easy, whilst the way out of the word is obstructed.

But the state of an individual who is affected with loss of speech is decisive of the existence of two distinct departments of the mind's relation to language; for incoming, or if I may so speak, afferent language, is perfect, whilst outgoing or efferent language, is obliterated. One may, as I have frequently proved, repeat in the ear of a person so afflicted, a name whose meaning is comprehended by him, and yet no effort of his will induce that word to pass to his tongue. Thirty or forty times over I have pronounced the word to endeavour to make him able to speak it after me, but his tongue uttered only an unintelligible noise, having no connection with the word required, yet he fully understood the name, and showed plainly his recognition of the right name when tried with others.

This resolution of the powers of language into incoming and outgoing language is a very striking fact, and its bearing upon the inquiry as to the site of the faculties of speaking and of understanding the speech of others, is, I think, very important, when we notice the significant circumstance, that the power of motor-language disappears with power of motion, whilst the power of sensory-language remains with the power of sensation. Not that the tongue itself is generally palsied, but the attack is always such as to deprive the right arm of motion, whilst the sensation of that arm either remains perfect throughout, or is affected only for a few minutes, or at most a few hours. The injury in short is not in the neighbourhood of the sensory but of the motor parts of the brain, and it does not paralyse sensory language but motor language.

Does not this indicate that the memory of movements combined for words, lies in anatomical connection with the centres which give motion to the tongue, &c., whilst the memory of sounds and sights combined for words, lies in anatomical connection with the centres of the nerves of the eye and ear; or in other words, that the situation of the ideas of *associated motions* which form the faculty of speech is supra-motory, whilst the situation of the ideas of *associated sensations*, which form the faculty of language-comprehension, is supra-sensory.

It is generally corroborative of the view I advanced, that education is unilateral, that the brain becomes unsymmetrical in higher and and more intelligent animals, and reaches its greatest want of symmetry in man, whose whole early life is spent in the acquirement of what I affirm to be one-sided educational developments.

Further, it is affirmed that the brain of educated individuals is manifestly more unsymmetrical than the brains of uneducated individuals of the same race; and that with great intellect goes unusual asymmetry.

If this be generally true, the duality of the brain itself is explicable; one side of it operating immediately, the other consensually, in all symmetrical movements.

ART. V.

Observations on (so-called) Nervous Apoplexy, on Congestion of the Brain and Serous Effusion. By W. BOYD MUSHET, M.B., M.R.C.P.

To render the argument more complete and intelligible, I prefix a very brief notice (which I shall extend in another place) of the presumed peculiarities of the endo-cephalic circulation.

Drs. Kellie, Alexander Monro, Abercrombie, Clutterbuck, Bennett and John Reid, and Drs. Burrows, Watson, Williams, Todd, Copland, Kirkes, Carpenter and Neil Arnott, whilst disagreeing, more or less, respecting the variability of the circulation within the cranium, its amenability to atmospheric pressure, and the importance of the cerebro-spinal fluid, are in harmony so far as the general admission of an intracranial *plenum;* and with most, a disproportion, or want of just balance, of blood in the different orders of vessels is regarded as the cause of congestive pressure (how demonstrable we are not informed), and the proximate origin of many comatose diseases. But, in opposition to these surmises, it cannot be doubted that the circulation in the brain, and the degree of vascular turgescence of its ganglia, vary even under normal physiological processes, which deviation may involve alterations in the relative complement, or in rapidity of flow, of blood in arteries, veins, or capillaries, or in its absolute amount in the entire series of vessels, as Mr. Durham has conclusively shown that the brain during sleep is in a comparatively bloodless condition. The circulation, as elsewhere, must be also much controlled by local (nutritive) forces, and the total volume of blood supplemented by the rise and fall of the serum in the cephalo-rachidian cavity, though the quantity of this is limited, and it can be but gradually effused or absorbed. It must, in addition, be borne in mind, that the manner of distribution of the blood in the brain, *during life,* is a consideration quite apart from the differences which are observed in the amount of fulness of the vessels *after death,* as states of post-mortem engorgement may depend on mere plethora, position, or mode of dying, whilst an exsanguined condition of the brain may be alike perfectly consistent with plenitude of the vessels, if there be dilution or poverty of blood from hæmorrhage or spanæmia.

Under ordinary circumstances, it is evident that adequate provision is ensured against the exigencies, whatever they may be, which arise from fluctuations in the ingress of blood to, and its egress from, the brain, as during excited action of the heart and lungs, in running, coughing, straining, and under various intellectual and emotional activities.

When we reflect that those pursuing the most laborious employments, and exposed to the most violent exercise, as artisans, athletes, tumblers, divers, players on wind instruments, and children, are not prone to apoplectic seizures, I contend—as will be more developed in connexion with pathological and traumatic lesions—that there is not a shadow of evidence that simple modifications in the cerebral circulation are capable of exerting any material pressure on the brain; nor, *à fortiori,* that such (hypothetical) pressure has any influence in the causation of apoplexy.

Apoplexia nervosa aut absque compressione (apoplexie nerveuse sans matière ou sans lésion appréciable of subsequent French authors) was a term invented by Kortum in 1785, to include those cases of coma and *sudden death,* in which no morbid appearance could be afterwards detected within the cranium, and where death was supposed to be due, according to the fantastic pathology of the period, to arrest, retardation or extinguishment of the animal spirits—*i. e.,* of the more subtle and volatile parts of the blood separated in the brain—to relaxation of the nerves, to spasm of the meninges, or of the nerves and vessels of the encephalon. Cases so viewed (quandoque nullum est vitium conspicuum in cerebro nec in cerebello) are recorded by Willis, Bonetus, and other older writers.

Morgagni[1] treated "De apoplexiâ quæ neque a sanguine neque a sero est," not in a *nervous* sense, but to illustrate certain obscure and insidious cerebral and other diseases. Thus—

[1] Lib. I, epist. v.

Art.	II.	Pus in skull.
,,	IV.	Much pus in left ventricle of brain.
,,	VI.	Disease of dura mater. Liquefaction and extensive disease of brain.
,,	XI.	Sanious matter on the surface of anterior lobes. Brain very flaccid (summâ flacciditate).
,,	XV.	Substance of right hemisphere very brown (valde fusca).
,,	XVII.	Sudden death (homo eodem quo cadebat puncto temporis est mortuus).
,,	XIX.	Sudden death (cor magnum et flaccidum).

The last two cases show the truth of the assertion of Heberden,[1] that *all sudden deaths are put down to the account of apoplexies*, when we find the illustrious Morgagni so committing himself.

Dr. Abercrombie believed that apoplexy might prove fatal without any morbid appearance, or with appearances so slight as to be altogether inadequate to account for the attack. For such cases he employed the term of *simple apoplexy*, and deemed them to depend upon a cause which acts simply upon the circulating system of the brain, producing a derangement, which takes place speedily, and is often as speedily removed. He relates cases (97, 98, 99, 100, 101) to support his views; but the histories and post-mortem details, which are most scanty, do not, I submit, irrefragably establish them as examples of primary simple apoplexy. In all, coma was the only leading symptom, and case 101 was undoubtedly uræmic, as the patient had anasarca and effusion into the thorax and abdomen. The others might likewise have been of renal origin (state of kidneys not recorded), or the result of narcotic poisoning, softening, plugging of the vessels, or of some other unrecognised cause of disease.

Dr. Clutterbuck, in like manner affirmed, that there are numerous instances on record of apoplexy proving fatal, where no change in the structure or condition of the brain could be detected, that was at all adequate to explain the symptoms, or to account for the death of the patient. He did not, however, confirm this statement by actual cases, but evidently followed Dr. Abercrombie, as he observed an arrangement and classification similar to that physician.

Dr. Cooke, nevertheless,[2] expressed an opinion that the spasmodic, convulsive, or nervous apoplexy ought to be classed amongst the symptomatic affections. Dr. Hope believed that a patient might die with symptoms of congestion, yet after death not a vestige of any morbid condition in the brain might remain. He thought the refrigeration of the blood, and its accumulation in the great vessels might remove all appearance of increased vascularity.

[1] 'Comments,' chap. lxix. [2] 'Nervous Diseases,' vol. i. p. 269.

Dr. Hughes Bennett is of opinion, that in individuals *who have died of apoplexy*, the brain may present in every respect a healthy appearance, the most rigorous and careful inspection failing to discover any morbid change. He candidly admits, however, that such instances, recorded by Morgagni and others, were, until lately, attributed to want of care during the examination. He considers the pathological condition of the brain, in these cases, to be temporary pressure on the encephalon by over-distension of its vessels with arterial or venous blood, either from *increased* or *diminished* action of the heart. After death, of course (he argues), this is not to be detected, the tonic contraction of the arteries is alone sufficient to empty them of their contents, and turgidity of the veins may remain or not, according to the symptoms immediately preceding death and the position in which the body is placed. He regards pressure to be the chief agent in the production of *all* cerebral diseases! Dr. Burrows also does not seem to be surprised that apoplexy, from vascular congestion, should prove fatal, without any lesion to be subsequently detected in the brain; but Dr. John Reid[1] sensibly remarks, that the disappearance of congestion is a supposition, not a fact, of Dr. Burrows, as no proof is offered in support.

In reference to apoplexy, Dr. Watson observes that we may detect no deviation whatever from the healthy structure and natural appearance of the brain. The congestive pressure (if it, indeed, existed) has left no prints of its action.

Dr. G. B. Wood, of America, respecting the occurrence of nervous apoplexy, states that it is generally admitted, death may occur with all the phenomena of apoplexy, without leaving any observable lesion in the brain. He deems such cases to be, at least, very rare, and mentions the suggestion which has been already advanced, that most of them might be referred to the existence of Bright's disease, and Dr. James Arthur Wilson, above thirty years since, wrote to prove that many apoplectic attacks, without lesion in the brain, were due to this cause.

To come more directly to the subject. The statements, concerning nervous apoplexy, as far as I can ascertain, are never supported by indisputable facts—that is, by recorded cases free from cavil, and I challenge pathologists, who endorse them, to supply the deficiency. On the other hand, I shall narrate three or four histories, militating against the hypotheses which have been propounded to explain the absence of morbid appearances in certain forms of fatal coma.

I previously venture to append a reflection of the observant Heberden, to indicate that a physician of the last century was not satisfied with the pathological doctrines of the period. "Theory may teach, but will find some difficulty in proving, that apoplexies

[1] 'Monthly Journal of Medical Science,' August, 1846.

must arise from a compression of the brain, owing either to a distension of the blood-vessels, or to extravasated blood from their rupture, and that the energies of the nerves can be deadened by no other cause beside fulness."[1]

CASE 1.—I was requested by Dr. —, of St. John's Wood, to accompany him to a post-mortem examination of a gentleman, aged thirty, who had died, with apoplexoïd symptoms. The doctor had been hastily called to this patient, and found him in a state of coma. He had been lying in bed, on account of pain in the knee (rheumatic?), and had taken medicine from a druggist in the neighbourhood. He was apparently well, two hours before taking a draught, and was playing with a child who had entered the bed-room. He was soon afterwards found insensible, snoring loudly, and he died, without change, in spite of the remedies adopted.

An examination was considered necessary, which was performed two days after death.

There were no morbid appearances within the cranium,—to which we naturally first appealed, with the exception of trifling congestion of the veins of the pia mater posteriorly, probably from gravitation. There was no softening or serous effusion. The viscera elsewhere were also healthy, and there was not any obvious cause of death. As there had been pain about the left knee, it was cut open, but there was no trace of inflammation, or of purulent effusion within the joint.

Dr. —, before the autopsy, unknown to me, went to the druggist, as he thought the symptoms referred most suspiciously to poisoning, and on examining his book, which was blotted, found a draught entered containing morphia. The symbol opposite to the morphia was gr. ss according to the druggist, gr. iss according to the doctor.

The friends, however, desired the affair to be hushed up, as the young man was on the eve of marriage, and the druggist was in some way connected with the family. According to my position in the matter, I was bound not to interfere, but I, nevertheless, rather freely expressed my sentiments.

At my suggestion the certificate was filled up—

$$\begin{cases}\text{Rheumatic pains.} & \\ \text{Coma.} & \text{(P.M.)}\end{cases}$$

and was so registered. I entertain no doubt that this was a case of narcotic poisoning, and would have been to our forefathers a case of *nervous apoplexy*.

CASE 2.—Henry H—, aged twenty-eight, had been a groom, but unable to work for two years past in consequence of disease of the

[1] 'Art. Paralysis et Apoplexia.'

left knee joint, for which he had been in- and out-patient of St. Mary's Hospital. He died suddenly on April 9th, 1860, whilst sitting up in bed drinking a cup of tea. This was unexpected by his wife and friends, and the medical men at the hospital refused a certificate. He was never known to have suffered from any cardiac affection. The autopsy was performed forty-eight hours after death, in company with Mr. Gascoyen. Rigor mortis well marked in extremities and jaw. Face pale. No foam at mouth; features placid. No indication of violence, but sugillation of posterior upper part of trunk.

Head.—Calvaria rather thick. Dura mater slightly adherent. Slight subarachnoïd effusion. Much prominence of veins of pia mater. Brain substance normal. No extra vascularity. Scarcely any fluid in ventricles. Sinuses at base rather gorged. No morbid appearance in cerebrum or cerebellum.

Thorax.—Right lung healthy, but lower lobe congested posteriorly. No adhesions, no effusion. Left lung same condition, but old pleural adhesions. About a drachm of serum in pericardium, no adhesion. Heart flabby, weighs eleven ounces. No valvular disease, but wall of left ventricle rather thin, flabby, and the organ generally larger than natural. A dark, but partially fibrinous, clot in right auricle, which distends its cavity and passes down into the right ventricle, interlacing firmly with the columnæ carneæ, and, at the latter situation, light in colour. Much black semi-coagulated blood also in right cavities. Stomach moderately large, almost empty. No erosion or ulceration of mucous surface, but pinkiness of the lining membrane in parts, as if digestion had been recently proceeding. Odour merely cadaveric.

Intestines mostly pale, contain some fluid, lining membrane healthy. Bladder empty. Spleen natural. Liver pale, healthy. Gall bladder distended. Right kidney healthy, pale. Left kidney, healthy, not so pale. No fluid in peritoneum.

Case 3.—M. A. L—, aged thirty-two, died suddenly in bed. There were no external marks of violence. The *brain* was healthy, as were also the membranes. There was some serosity at the base of the skull, and a little sanguinolent fluid in the ventricles. The *lungs* were much congested, and generally emphysematous; the bronchial tubes extra-vascular, and contained frothy mucus. There was about a pint of serous fluid in either pleural cavity. The *heart* was flabby, much enlarged, containing clots and dark fluid blood on both sides. The left ventricle was hypertrophied, and dilated. The right ventricle was also dilated. The aortic valves exhibited warty vegetations. The other valves were normal. There was no fluid in the pericardium. *Stomach* was very large, containing much semi-digested material. Intestines healthy. The spleen and liver were much enlarged, the kidneys healthy. There was some serum in the peritoneum.

CASE 4.—J. M—, a man about sixty years of age, suffering from a reducible hernia (right), and slight bronchitis, died suddenly on January 2nd, 1854.

Head. — Brain natural, not unusually congested. Substance healthy. About two drachms of fluid in each ventricle.

Thorax.—About an ounce of turbid serum in pericardium. Heart weighs rather over ten ounces, and is dilated, thinned, and flabby. The muscular tissue is pale, and there are yellowish spots on the endocardial surface. In the heart generally, there is substitution of fatty for muscular substance, especially in the parietes of right ventricle, midway between base and apex. There is no valvular disease. Much dark fluid blood in right cavities, and some on left side. Lungs are congested and emphysematous. The bronchial tubes afford evidence of chronic bronchitis. There is no softening of the mucous membrane. Old adhesions of pleuræ on both sides, and several ounces of fluid in each cavity. Slight exudation of recent lymph on anterior surface of left lung near its apex. Some congestion of liver. Stomach contains some yellow semi-digested matter, its mucous membrane is not softened, but here and there a little injected.

Intestines healthy, presenting slaty discoloration. The hernia presents trifling ulceration on its inner surface, but no evident injection. Kidneys, spleen, and bladder healthy.

In the majority of instances, I believe, the coma in (so-called) nervous apoplexy to be due to uræmia, which, as Dr. Richardson remarks (Asclepiad, vol. i), the unlearned as yet call apoplexy. This physician does not particularly refer to the complaint in the old, or in connection with the small contracted kidney (of which I shall speak further), but in one of his cases, in a woman, aged thirty-four, with enlarged kidney, there was *no evidence of congestion of the brain, nor of effusion; the membranes were natural, and the ventricles contained no fluid.*

He also notices that the effects of the scarlatinal poison may simulate apoplectic coma, and in two memorable instances of my own, I can affirm that one child, about eight or nine years old, died from sudden invasion of epileptiform coma (toxæmic) whilst malignant scariatina was prevalent in the house.[1] In the other case, a boy about fourteen, convalescent from the disease, was also *suddenly* carried off, from arrested renal function and accumulation of urinary elements in the blood.

Occasionally, fibrinous occlusion of the cerebral artery — the middle one, most usually —may cause death, especially in young

[1] See Dr. West, p. 24, for an analogous case.

adults, and be referred to nervous apoplexy, if the vessels remain unopened by an incautious observer. Dr. Markham in his work 'On Diseases of the Heart' (Appendix II), records, in detail, a striking instance in a woman of fifty, of infarction of the innominatæ, right common carotid, left internal carotid, and middle cerebral arteries, in which there were apoplectiform phenomena with hemiplegia, and death in sixty hours.

In Dr. Abercrombie's thirty-fourth case, there was plugging of the basilar. Further information on this subject may be obtained in Dr. Kirkes' original paper in the 'Medico-Chirurgical Transactions,' vol. xxxvi.

In the present state of medicine, it is unnecessary to enter at length into the manifold causes which may provoke more or less pronounced comatose symptoms. If the appearances within the cranium be of negative character, we may expect the cause of death to be manifested in other organs, or be owing to shock, syncope, lightning, or a physically undetected, or undetectable poison.

There are, of course, numerous sources of fallacy. Thus, as I have already noticed, under diagnosis, death may depend on an anomalous type of pneumonia, or severe concussion, with, or without intoxication. Delirium tremens also with our predecessors, and (unobserved) cerebral softening have also, it is almost certain, been regarded as nervous apoplexy, as well as cases of homicidal, suicidal, and accidental narcotism. Pulmonary apoplexy, too, has given rise to supposition of cerebral apoplexy, and under such a diagnosis, we must conceive that the pulmonary structure is not submitted to careful incision to correct the error. Cases of angina pectoris, of cardiac coma, and apnœa, of pressure on the recurrent nerve may have increased the list, as also the toxic coma of gout and pericarditis, cases of catalepsy, and even hysteria, and the collapse ensuing on rupture of internal aneurisms, as the most renowned pathologists of yore often confined their examination to the brain, and afford no history of symptoms.

Even impaction of food in the œsophagus may prove a perplexing cause of death. I once examined the body of a man, who, I was informed, staggered, and fell dead in the yard, outside a cottage. This appeared to point to heart or brain affection, and I had received no further account of the previous circumstances. A cautious but fruitless scrutiny was made of all the viscera, before the foreign body was detected—a piece of beef weighing two ounces, firmly implanted behind the larynx.

The brain, I admit, may not present any appearance sufficient to explain the cause of death, but it is not, therefore, to be concluded that the coma (apoplexy) is a primary neurosis, in the sense employed by Kortum, and his supporters—that is, dependent on some intrinsic undefinable disturbance, but *symptomatic* or consecutive to some well-recognised, if ascertainable, morbid operation. Perhaps, the old proverb contains some truth, that "Venus nimia in senibus multum ad apoplexiam disponit," that excess in this particular, as in any other, may so depress the functions of the nervous system already nearly exhausted, as to decide the term of life; but this is rather death commencing at the brain, previously atrophied, or otherwise undermined (necrencephalus), than mere idiopathic nervous apoplexy.

To return to the most common mode of production of coma (cerebral hæmorrhage excluded) Dr. Basham, 'On Dropsy connected with Disease of the Kidneys,' points out that renal disease will give rise to symptoms which often appear to have little reference to the renal functions, and which may be, and frequently are, accepted rather as evidence of disease of the cerebral organs. In these cases, *dropsy is altogether absent*, or the *anasarca is so trifling* in amount, as to excite but little attention. Symptoms referable to the brain are those which exclusively attract the notice of the patient, or to which the care of the practitioner is directed. Headache, frontal, or sincipital, dimness of vision, simulating the approach of cataract, slight convulsive attacks of epileptiform character, followed by more aggravated paroxysms of convulsive disorder constitute the class or series of symptoms; the other is expressed by headache and imperfect vision, followed by dulness of intellect, sluggishness of manner, frequent sopor, or drowsiness, or occasional stupor, terminating usually in fatal coma. The absence of dropsy, and the insidious approach of these symptoms, are well calculated to misdirect the attention of the medical practitioner, and the remedies employed to avert the simulated and suspected apoplectic seizure may be those which hasten forward the fatal effects of uræmic poisoning. The form of renal disease which usually develops these symptoms is the atrophied, shrunken, and nodulated kidney (p. 147).

Dr. Basham does not allude to its frequency in old persons, or in those who have prematurely degenerated, but thinks, in conjunction with the late Dr. Todd, that the wasted kidney is closely connected with the gouty diathesis. Yet he owns, that it may be met with in persons who have never exhibited gouty symptoms. He does not consider that the shrunken kidney represents an advanced stage of Bright's disease, as in this, he believes, the kidney is usually increased in weight. On the contrary, Dr. Handfield Jones maintains that the contracted granular form is the one most typical of true degeneration. Agreeing on the whole, with Dr. Prout, he does not regard it as necessarily connected with renal hyperæmia, although it may be thus complicated or intensified. Dr. Bright[1] describes three forms of diseased kidney connected with albuminous urine. In the first, the organ is natural in size; in the second, enlarged; in the third, contracted. Dr. Bright expressed uncertainty whether

[1] 'Medical Cases,' vol. i, pp. 67—69.

they were distinct or only modifications, and more or less advanced states of one and the same disease. Dr. George Johnson and Dr. Maclachlan insist that the small contracted kidney is the variety *most incident to age*, and agree that it may proceed to extreme degree in absence of the supervention of dropsy.

Without venturing to merge deeply into renal pathology, I feel confidence in asserting that all who are accustomed to examine aged subjects will allow that the small contracted kidney is, in them, by far the most common appearance, that it is absolutely frequent, and a more or less essential characteristic of senile degeneration, though, in many instances, it may not manifest itself at all during life, or until the occurrence of epileptiform or comatose symptoms. Its advent at earlier periods, I hold to be indicative of low vital power and premature physical impairment.

In gout, the kidney may present similar alterations, in consequence of the excessive demand on the urinary functions and the diathetic influence, but that such degeneration peculiarly or invariably depends on this complaint is unquestionably erroneous.

As with atheroma, in advanced life, it is difficult to declare how far the alterations in the kidney are physiological, how far pathological in their nature, but they clearly partake more of atrophy and senile changes, than of active disease. It is confirmed by all observers, that these renal conditions may not, and often do not, give rise to dropsical effusion, and Dr. Maclachlan remarks in his 'Diseases and Infirmities of Advanced Life,' that "the great majority of persons affected with the shrunken kidney die comatose, without any subsequent appearanee in the brain, the symptoms proceeding entirely from the poisoned state of the blood" (p. 583).

In briefly considering the subject of *cerebral congestion* as an efficient *cause* of fatal apoplexy, I would, at the outset, inquire if any practitioner can advance an undeniable example—verified by accurate detail of symptoms and exhaustive autopsy—of death from mere primary sanguineous congestion?

Dr. Abercrombie lays little stress on cerebral congestion as a cause of apoplexy; in fact, I am not aware that he once employs the term amongst his post-mortem appearances, though in one or two instances he refers to *increased vascularity*. He ascribes apoplexy (p. 206) to simple (nervous) apoplexy, in which there are no appearances to be subsequently detected; to serous effusion, which he regards as a consequence of simple apoplexy, and not a primary state; and to sanguineous extravasation. Dr. Clutterbuck referred the symptoms of apoplexy to impeded or interrupted circulation in the brain, leading to suspension of the sensorial functions, from pressure operating upon the blood-vessels, so as to hinder mechanically the passage of blood through them.

Dr. Sims, physician to the St. Marylebone Infirmary, in 1835, held that a loaded state of the blood-vessels of the brain is sufficient to produce all the symptoms of sanguineous apoplexy, and to occasion death without extravasation; and that convulsions in children arise in general from cerebral congestion, and are essentially cases of sanguineous apoplexy.[1] He admitted that the presence of serum, to which I shall presently refer, is not absolutely explanatory of the symptoms observed during life. By a parity of reasoning, the appearances of congestion are not of certainty indicative, being more probably a *consequence* than a *cause* of the morbid phenomena, and the cases of Dr. Sims in which cerebral congestion was observed are imperfectly recorded, and in several, the thoracic and abdominal viscera were not inspected. In some, in addition, other diseased appearances were noted in the brain. In the case of children, above noticed, the convulsions, it is generally agreed, are nearer allied to epilepsy than to apoplexy, originating in eccentric irritation, not congestion. In many instances, death is due to laryngismus stridulus. For my own part, I have more frequently seen coma, in children, connected with spanæmic or deteriorated quality of blood, than with evidences of vascular engorgement of the brain. Moreover, the convulsions, in them, for the most part precede the so-called congestion, not the congestion the convulsions.

Dr. Burrows does not mention *any particular case* of cerebral congestion causing apoplexy, but appears rather to regard it as an axiom, and assume it as a postulate. The cases referred to Dr. Abercrombie, Dr. Cooke, and the rest, are not described, but seem more fairly attributable to disease of the heart or uræmia than to perverted circulation in the head (congestion). I have already remarked, that Dr. Abercrombie records no case which he imputes to cerebral congestion, and after careful perusal of Dr. Cooke's learned treatise on 'Nervous Diseases,' I cannot find any of the cases which are referred to by Dr. Burrows, or which will, in any way, corroborate his views! Dr. Maclachlan again, though he treats of *congestive apoplexy* gives no illustrative examples.

Dr. Burrows goes so far, as to believe, that apoplectic coma is rarely dependent upon the extravasation of blood (p. 92), but in the vast majority of cases, upon the pressure induced by vascular congestion. He relates two histories (pp. 92 and 96), *in which both the patients died comatose,* to support his views; but they serve to demonstrate what he strives to disprove, and the intervals of *impaired* consciousness, which occurred in their progress, are explicable by the peculiarity in the site and mode of the effusion.

Of a truth, as Dr. John Reid points out in the article before quoted, "In analysing closely the matter contained in most treatises

[1] 'Med.-Chir. Trans.,' vol. xix.

on practical medicine, facts and opinions are with difficulty separated from each other, and no satisfactory evidence can be found of the evidence of truths upon which important inferences are founded."

Dr. Watson adopts the tenets of Dr. Burrows, that pressure is the ordinary cause of apoplectic coma; and that pressure can, and does, *sometimes* cause coma is incontestably established, but that cerebral congestion, pure and simple, can induce coma, *quoad* pressure, is a vastly different proposition.

Andral, in his 'Clinique Médicale,' gives five cases under the head of cerebral congestion, which I beg to remark, in all reverence to this distinguished physician, are not to be unequivocally relied on for determining the point disputed. In only one case, are the kidneys mentioned (gorged with blood), and of four he observes, "Is it not a circumstance worthy of notice, that the four cases of cerebral congestion now reported regarded individuals labouring under chronic affections at the time the brain became congested in them? In three of them, hæmatosis was for a long time vitiated; they were meagre, bloodless, and appeared to be in a condition entirely opposite to that which is usually laid down as favouring cerebral congestions." Two of these four were phthisical subjects, and the fifth case is a history of cerebral symptoms intercurring in ordinary pulmonary tuberculosis.

Dr. Bright,[1] under the head of "Pressure," mentions two cases which are said to have been due to cerebral congestion. In Case 86, there was giddiness, loss of sight, and of consciousness, in a stout-made man, who was, however, exsanguined. After depletion, tonics did most service. In this case, I humbly suggest, that the assumption of *simple* congestion is not satisfactorily proved, and in Case 87, in which, there was unconsciousness with convulsion, and slight, but persistent, paralysis, the diagnosis is favorable, I think, to limited hæmorrhage..

Cases 88, 89, 90, 91, related as samples of cerebral congestion, were dependent on narcotism. Cases 92, 93, 94, are cases of bronchitis with cerebral congestion. 95, 96, 97, are cerebral congestion from emphysema; 98, cerebral congestion from hooping cough; 99, effusion of blood on the brain in a child in connexion wiih the same disease, the account being scanty and the history wanting. Case 100, is congestion in the vessels of the brain in a man dying of hepatization and gangrene of the lungs after a severe scald; 101 is vascular congestion of the brain in fever, and 102 cerebral congestion in fever. These are the whole of the cases of congestion of the brain to be found in Dr. Bright's work. Of all these, only the first two, *which recovered,* are admissible, and these

[1] 'Reports of Med. Cases,' vol. ii, part 1, sec.

are fairly open to question. The remainder are secondary, and connected, in almost all instances, with respiratory disturbance, and many of them might be dependent on cadaveric states. In all, at least, the cause of death is evident, without the necessity of resorting to cerebral congestion for explanation.

Dr. Copland says, that in a large proportion of cases of simple primary apoplexy, *i. e.*, without extravasation, *excessive injection of the vessels* of the pia mater, and *engorgement of the whole vascular system* of the encephalon are the chief lesions. This (he owns) is of comparatively rare occurrence. It constitutes the *coup de sang* of the French, and is observed in those cases of *coup de soleil* which prove rapidly fatal. But Dr. Aitken, in his "Science and Practice of Medicine," justly maintains that it is erroneous to describe Insolatio or Erythismus tropicus as of the nature of apoplexy. The opinion possesses additional weight, as this physician would give a very wide signification to the term apoplexy, and use it to characterise a group of symptoms, irrespective of the anatomical conditions upon which they may depend. Thus he would include congestion of the brain, hemorrhage, sudden serous effusion, local cerebritis, tumours of the brain, meningitis simple and tubercular, the progress of various zymotic and constitutional diseases from blood-poisoning, anæmia, disease of the heart, and vascular obstructions under this head.

Death occurs in cases of insolatio, according to the concurrent testimony of military surgeons, from syncope or apnœa, and the chief morbid appearance usually observed is excessive engorgement of the lungs. In many cases the brain is healthy, without trace of congestion or accumulation of blood. Dr. Morehead states, that the blood in the vessels is always fluid (Aitken). The essence of the disease seems to be intense exhaustion, from the combined effects of over-fatigue and subjection to a very high temperature—intracranial changes being adventitious, not causative, or even constant.

Is it permissible to adduce cases like the preceding—cases of insolatio, of cerebritis (?), of tubercular disease of the brain or its membranes, of delirium tremens, of uræmia, of secondary or post-mortem engorgement of the vessels of the brain, &c., as examples of cerebral congestion constituting primary apoplexy? It is undoubtedly important to define accurately the limit and meaning of the word, as one of our most distinguished practical physicians, Dr. Watson, denominates by "apoplexy," "coma occurring suddenly, or coming on (at least) with rapidity," which would include every affection attended with sudden loss of consciousness, whether arising from hæmorrhage, tumour, intoxication, typhous complication, coma of epilepsy and hysteria, injuries to the head, softening, and even asphyxia, syncope, the terminations of cerebritis and hydrocephalus, narcotism, and the action of deleterious gases.

Practitioners generally make a distinction between *apoplexy* and *genuine apoplexy*, so as to render justifiable a remark of the late Dr. Jones Quain, that when we look into works on medicine, we are struck with the confusion which exists in the nomenclature of cerebral diseases. More precision has been attempted by our French neighbours, though Littré complained ('Dict. de Méd.,' art. Apoplexie) that "Le mot apoplexie, souvent aussi vague dans la bouche du médecin, que dans celle du malade, se prête à toutes les interpretations de l'ignorance."

"In the nervous centres, as elsewhere," says Andral, "before the production of hyperæmia or anæmia, we must conceive a primary modification of the force, whatever it is, which subjects the cerebral circulation to certain rules. In the midst of these numerous currents, of these oscillations of globules, which pass within the organic tissues, how many causes constantly presented, and whose influence is entirely unknown to us, may derange a current and modify the distribution of the globules? * * * When we thus examine minutely the grounds of the question, we soon see that hyperæmia and anæmia in the brain, as in other parts, are themselves but *secondary phenomena*—mere effects. But these effects, inconstant and variable, do not necessarily follow the action of the cause; they may be wanting, and yet the symptoms will still continue, for they depend less on the state of cerebral hyperæmia or anæmia, than on the organic modification which precedes them, and which causes them. Thus, our post-mortem examinations shew us, for the explanation of identical symptoms, sometimes a state of hyperæmia, sometimes a state of anæmia, sometimes nothing unusual in the quantity of blood contained in the brain; and in this brain, moreover, no lesion appreciable by our present means of investigation; the reason is, because these means do not shew us all; by them, we as yet discover nothing but effects; the material modification which incontestably precedes them, requires not their production in order that disturbance may take place in the functions of the organ. However, once produced, the different lesions which the present state of anatomy is calculated to reveal, may give rise to phenomena which depend on them alone, and which establish their diagnosis.[1]" This quotation contains the gist of my argument, viz., that the (so-called) nervous apoplexy and cerebral congestion are but secondary states, not the origin of the apoplectiform symptoms. I am aware that the doctrines of Andral may be urged, in some degree, on either side of the question, but they at least favour my views as to the uncertainty which besets the subject, which uncertainty most pathologists will admit. Dr. Watson, in his twenty-first lecture, adverts to the difficulties with which the study of the maladies and disorders of the brain and

[1] Translated by Dr. Spillan, page 92.

nervous system is surrounded; and Dr. Brown-Sequard acknowledges, that less is known about the anatomy and physiology of the brain, than about any other organ. The *onus probandi* rests with those who subscribe to the theory of cerebral congestion as a cause of fatal apoplexy, which they adopt, I suspect, on the score of conventional obedience to systematic writers. I therefore reinsist, that general objections and counter-assertions are pointless and futile, unless fortified by the crucial proofs of clinical and pathological observation. I speak with some confidence, the result of special investigation, as during a residence of five years at the St. Marylebone Infirmary, where I had medical care of nearly a thousand aged persons, and great opportunities of noting maladies from their commencement, of ascertaining the appearances after death, and of observing disease in all forms, at all ages, in a hospital of 200 beds, I never witnessed a fatal case which could be clearly ascribed to simple apoplexy or cerebral congestion.

Physicians, swayed probably by surgical considerations and physiological experiments, have been altogether too *mechanical* in their opinions concerning apoplexy and cerebral congestion, and have not dwelt sufficiently on the importance of the *quality* of the blood. *All* has been assigned to *pressure,* from augmentation or deviation of the normal amount of blood in the several orders of vessels, without emphasis on the *cause* of the pressure, *if it obtain.* For example, if cerebral congestion causes coma or apoplexy (congestive) from pressure, how can it be explained that inflammation of the brain-substance, or cerebritis, does not, in its earlier stage, produce coma, although there must be notably increased blood in the capillaries and other vessels of the part, and (theoretically) consequent pressure? Verily, the pathology of the brain is encompassed with difficulties, but these difficulties are oftener tendered and acknowledged as explanations or finalities, than appreciated as monitors to caution us against misinterpretation, and incite more diligent and profitable inquiry.

It may be said, that venesection has been *advantageously* had recourse to (the contrary might be also stated) in many cerebral attacks, supposed to depend upon congestion; but I reply, that although abstraction of blood may withdraw a given quantity of this fluid from the vessels, it is inexplicable how it can rectify the vascular balance in an (assumed) congested venous or arterial apparatus. Is the practice correctly founded, or does it hold good in spite of the erroneousness of the theory? And I ask each individual member of the profession, how frequently he has employed the lancet in simple primary congestion of the brain?

Much evidence might be collected in favour of bleeding in cases of sudden hemiplegia, many of which depend on discontinuity of the cerebral fibres or infarction of the vessels, and on theoretical grounds we should imagine such a line of practice positively injurious. In other than cerebral disorders, abstraction of blood has been indiscriminately had recourse to with the happiest advantage, if we credit the unqualified testimony of men of the largest experience. Thus in pneumonia, without distinction of cases, the school of Sangrado and Broussais has in its turn had a multitude of disciples; and in fevers, venesection has been carried to extreme by Clutterbuck and his followers, who imagined the febrile state to be associated with, or symptomatic of, inflammation of the brain. I regard the nervous manifestations in fever to depend rather on heteræmic influences, or, as Dr. Todd would say, disturbed polarity, than on congestion of the brain or encephalitis. The treatment of fever by bloodletting, is now generally abandoned and condemned: and this amelioration in practice cannot be ascribed to change of type of disease, but to clearer and more rational ideas on therapeutics. From these sentiments I must not be mistaken for a bigoted antiphlebotomist, as on one occasion—if anything relating to our art can be certainly declared—I saved life in a severe case of inflammatory anasarca from exposure and renal congestion, by the prompt removal of a very large quantity of blood from the arm. But to return to the state of the brain in fever: Dr. Watson confesses, that he cannot tell how often he has looked, and looked in vain in the brain, for some palpable disorganization, or some effusion, implying pressure. All who are familiar with the dead house of a hospital are aware, that this fruitless search for some physical explanation of the comatose state, after death by fever, is of very common occurrence (vol. ii, p. 807). In cases denominated congestion, in which benefit has been derived from, or has followed phlebotomy, I believe the relief to be at times due to the effect exercised on a commencing inflammatory state of the brain, where blood has been attracted more than determined to the head, its properties altered, and the nutritive operations interfered with and perverted, as in other tissues, during the inflammatory act. In many cases, the symptoms are probably uræmic, or arise from obstruction to the functions of the heart and lungs, producing disorders viewed as apoplectic. Alteration in the *quality* of the blood, under such circumstances, is capable of producing symptoms commonly referred to pressure from congestion, and we possess as much, if not more, evidence of the existence of the former, as of the latter. That enormous pressure may be exerted in absence of apoplectiform states (there being absence of altered quality of blood), is demonstrated by persons labouring under very large tumours within the cranium, compressing important structures, often remaining unattacked by coma until within a few hours of death. I can vividly call to mind a fibrous tumour as large as a billiard ball springing from the dura mater, and deeply indenting the hemisphere, an aneurism of the basilar artery nearly of the same size and a large malignant tumour of the cerebellum, amongst others, in confirmation. I know, it may be rejoined, that these growths were gradual, whereby the neighbouring parts were absorbed, or by degrees accommodated themselves to the pressure experienced, or that their position did not interfere with the more important parts of the brain; but these attempted solutions appear highly unsatisfactory, if we are to concede grave and even mortal consequences to a *slight and transient* (*and after death, unrecognisable*) *congestion*.

Not that I deny the brain can become the theatre of congestion, but that congestion, pure and simple, can be reasonably inferred, much less *proved*, to be a sufficient cause of apoplectic coma. Also, that congestion can produce pressure, equal in extent to many vascular variations consistent with health. It is beyond dispute, that coma exists as frequently unaccompanied, as accompanied by any indication of pressure or congestion. If congestion be combined, it may with stronger probability be accepted as the result, than as the origin of the phenomena, to which it is, I affirm, non-essential and sequent.

The most, indeed the only apparent, valid argument of the congestionists is, that in elderly patients of a plethoric constitution, symptoms referrible to the head, as cephalalgia, vertigo, drowsiness, sense of fulness and temporary confusion of thought sometimes occur. But I demand, can even these be shewn to depend necessarily on congestion of the brain? Can it be maintained that they are restricted to the plethoric, and that these persons are otherwise cerebrally sound? I admit, that patients with the symptoms I have specified, have come under my own observation. I remember in particular (as a type) a sanguine old man, in whom cupping from the neck was periodically practised with marked benefit. I may mention another case which comes under the same category, though occurring in a youth of fourteen, who experienced severe cerebral symptoms from assiduous application at the easel. Here purgation and rest from his pursuits afforded speedy cure.

In the first instance, the attack I consider to have been owing to alteration in the nervous substance of the brain or its arteries, or to an excessive composition of the blood, giving rise to nutritive impairment, but not to increment in the amount (congestion), but in quality of the fluids within the cranium. In the second case the symptoms are ascribable, not to congestion of the brain, but to protracted mental tension in a weakly boy, passionately addicted to painting. In either case, the ordinary nutritional changes in the nervous substance were deviated from, with consequent accumulation to greater or less extent of toxic materials in the minute vessels.

Many old persons in a state of coma are aroused and recover after bleeding, as attested by Morgagni, Forestus, and others of bygone times, who regarded them as specimens of *serous* apoplexy.

Cases of the kind I have observed and treated, but there is no proof that the attack in these is primarily dependent on inequality in the amount of blood in the cerebral arteries and sinuses (congestion or pressure), but there is considerable evidence in favour of a toxæmic state of the blood from ureal impregnation. The recovery after venesection is to be referred probably to the withdrawal of a portion of the poisonous agent from the circulation, by which time is gained to combat the renal degeneration. I am gratified to find that I am supported in this view by Dr. Richardson, who asserts that venesection is of the highest value in acute uræmic coma (Asclepiad); but when patients recover consciousness after bleeding, I suspect most practitioners impute the attack to vascular congestion or pressure on the brain. In many cases, the employment of hydragogue cathartics, diaphoretics, and mild diuretics, will restore the patient, and a course of iron produce more or less permanent improvement without abstraction of blood. Examination of the urine will reveal, in these cases, more or less albumen, an indication of degenerescence of the kidney, which I pointed out, when considering the subject of cerebral hæmorrhage, as a more or less constant senile change. Recurrence of the attacks in uræmia is especially apt to ensue, more particularly if the patient be not subsequently improved by the exhibition of steel medicines. I have attended a large number of old persons suffering from this affection—the majority without bleeding, and it is difficult to state definitely, under what circumstances the lancet is most demanded. As a rule, its employment is warrantable and serviceable when the coma is very profound, and the symptoms approach those of extravasation of blood; in fact, in certain instances, the diagnosis becomes impossible.

In milder forms of uræmia, compound jalap powder, elaterium or croton oil, with salines and nitric ether, are sufficiently efficacious with or without blisters or sinapisms, and stimulating enemata. On the whole, I would infinitely prefer to rely on alvine evacuants, than on bleeding in anomalous cerebral attacks. Every experienced man must be acquainted, how little benefit accrues on most occasions from the use of the lancet, until the active operation of cathartics. It is much more probable that the disorder is mitigated by the removal of deleterious matters from the circulation, than by a derivative action decreasing the volume of blood and vascular supply to the brain. Cases of uræmia are analogous to those of ordinary narcosis, and do not require congestion and pressure to explain the morbid state. Congestion is not the essence of the disease, but alteration in the blood, and congestion and pressure are consequences, if present. By prolonged bodily exertion the force of the heart is increased, and the circulation in the brain quickened. Yet it does not lead to cerebral mischief (congestion). On the other hand, protracted intellectual activity is not unfrequently followed by symptoms involving the brain. Under either circumstance, it is likely that the vascular conditions of the encephalon experience modification, but in the latter case only is there *polar* disturbance or altered innervation, to which congestion, if real, is subordinate and consecutive.

Further, signs ordinarily held to be dependent on, or conjoined with congestion of the intra-cranial structures, as flushing and turgidity of the face, throbbing of the temporals and carotids, injection of the conjunctivæ, &c., are but fallacious indications and doubtful exponents of encephalic states. Careful inspection of cases of strangulation are narrated by Drs. Kellie, Monro Secundus, John Reid, and Watson, in which these external characters were found to be coexistent with *non*-increased vascularity of the vessels and tissues of the brain. In a very careful examination made by myself of a child, aged six months, strangled by its mother, the brain was found to be not extra-congested, a few veins of the pia mater were rather prominent posteriorly, and there was fine ramiform injection over the surface of the hemispheres. No sub-arachnoïd effusion. Brain firmish for the age. About a drachm of serum in the ventricles, puncta vasculosa not abnormally numerous, a little dark fluid blood in the longitudinal and lateral sinuses, and about three drachms of serous fluid at base of skull.

In another child, aged three months, poisoned by laudanum, the brain presented a very pale, almost anæmic appearance. The ventricles were slightly moistened with serum, and there was about a drachm at the base of the cranium. In justice, I should add, that the infant was flabby and ill-nourished.

Dr. E. A. Sansom (On the action of Anæsthetics, 'Brit. Med. Journ.,' Sept., 1864) concludes, from the result of careful experiments, that chloroform, ether, and volatile narcotics, act directly upon the blood. He maintains that the essential concomitant of a state of anæsthesia is sluggishness of circulation; that it is not a condition of hyperæmia of any organ. As in sleep, Mr. Durham has pointed out that the brain is comparatively anæmic, and that the blood in its vessels is not only diminished in quantity, but also flows with a decreased rapidity, so in chloroform narcotism. Dr. Sansom alludes to an interesting case, recorded in the 'American Journal of Medical Science for Oct., 1860,' in which chloroform having been administered, in extensive fracture of the cranium, it is stated that during the full effect of the narcotic, the brain was remarkably pale; and whenever the anæsthetic influence began to subside, the surface became florid and injected.

(*To be concluded.*)

ART. VI.

An Attempt to adapt the Laws of Electro-dynamics to the Science of Medical Electricity. By HARRY LOBB, M.R.C.S.E., &c.

THE science of medical electricity, so far as it relates to the exact knowledge of the laws governing the passage of voltaic currents traversing a given portion of the human body, and a correct nomenclature, may be said, at the present time, to be in the same condition of empiricism as was the science of electro-telegraphy some few years back, before the introduction of Ohm's electro-dynamic formulæ, expressing the relation to each other of the active and opposing forces in the voltaic circuit.

The great impetus which the introduction of Ohm's formulæ has given to the science of electro-telegraphy has encouraged me to make a like effort for medical electricity by establishing a clear and simple exposition of electro-dynamic laws to the therapeutics of voltaic electricity.

A desideratum to the practical medical electrician is the knowledge of the number and size of the elements of a voltaic pile necessary to penetrate a given part of the human body, and a distinct knowledge of the electro-dynamic laws, affecting the theoretical and practical application of the voltaic pile to the treatment of disease, would be of great value.

The essential conditions on which depends the *intensity* of the voltaic current (*the quantity of electricity traversing a circuit in a given time*), upon which are dependent almost all the effects of the current, may be expressed by the following formula:

$$I = \frac{E}{R} \quad \text{I (the intensity)} = \frac{\text{E (the electro-motive force)}}{\text{R (the resistance).}}$$

The electro-motive force is proportioned to the number of elements of a given nature, in the electro-motor (battery) employed, measured by the galvanometer, and the resistance is that which is opposed throughout the circuit to the passage of the electricity.

According to this formula the current may be said to be directly as E, and inversely as R; the intensity may, therefore, be increased by one or both of the following means—increasing the power of E or diminishing that of R; similarly the intensity may be decreased by diminishing the power of E, or augmenting that of R. In the voltaic current, however, R is always compounded of two distinct resistances, namely, that of the fluids of the battery (necessarily traversed by the current) and that of the external portion of the circuit; for instance, the current passing from one hand to the other of a patient holding the conductors of the battery terminals. The former may be termed (r) limiting the symbol R to the latter; thus the intensity of a current will be directly proportionate to E, and inversely to r and R, or,

$$\text{I (the intensity)} = \frac{\text{E (the electro-motive force)}}{r \text{ (the battery resistance)} + \text{R (the external resistance)}}.$$

R being the resistance of a given external portion of the circuit necessarily remaining constant, it follows that, to increase the value of I we must adopt one or both of the following means—an increase in the value of E by augmenting the number of elements arranged in series, or a diminution in the value of (r), generally effected by increasing the surface of the plates.

When for the symbols in the above equation numbers are substituted, expressive of the value of E, r, and R, it becomes evident that the efficacy of either of the above-mentioned means of augmenting the power of the current is dependent upon the value of r and R. Thus in the case of the equation:—

$$I = \frac{60}{4 + 8} = 5.$$

If we decrease the value of r from 4 to 2 (by doubling the surface of the elements in the electro-motor) the value of I becomes—

$$I = \frac{60}{2 + 8} = 6,$$

or is only increased in the ratio 5 : 6; whereas by augmenting the value of E from 60 to 120 (by doubling the number of the elements in the electro-motor) the value of I becomes—

$$I = \frac{120}{8 + 8} = 7{\cdot}5,$$

or is increased in the ratio 5 : 7·5, taking into account the increase in the resistance of the electro-motor by doubling the number of elements in the battery, namely, doubling the power of r. In the case of the equation—

$$I = \frac{60}{8 + 4} = 5;$$

on the other hand, by halving the value of r, that of I becomes—

$$I = \frac{60}{4 + 4} = 7{\cdot}5;$$

whereas by doubling the value of E it would be only—

$$I = \frac{120}{16 + 4} = 6.$$

In accordance with the above, the following simple rule will be found useful in all cases. Where an increase of voltaic power is required without unduly augmenting the extent of metallic surface in, or the weight of the voltaic electro-motor.

RULE.—Measure the resistance of the external portion of the circuit, and also that of the battery (electro-motor); if the latter be the greater, the increase of power is best obtained by decreasing this resistance, *i. e.* by increasing the surface of the positive and negative elements in each couple, or by bringing them into closer proximity; if, on the other hand, the external resistance to be overcome be greater than that of the battery, the increase of power is best obtained by increasing the value of E, *i. e.* by augmenting the number of couples arranged in series.

This rule is the more valuable to the electro-therapeutist from the fact that it enables him to reduce to a minimum the amount of heat evolved in that portion of the circuit which is constituted by the human body. Whenever, as frequently happens in practice, this point is of importance, the resistance of the battery should never be less than that of the portion of the human body traversed by the current.

Neither, on the other hand, should it be much greater, for in this case the number of elements arranged in series would have been unduly augmented, and the amount of heat evolved proportionately increased.

These facts, to which no attention has been hitherto given by medical electricians, result from the elaborate investigations of Joule, De la Reve, and Favre, whose conclusions are embodied in the two following formulæ, of which the first expresses the amount of heat (H) evolved in a given time throughout the whole circuit, whilst the second shows the conditions affecting the amount of heat (h) evolved in any given portion of the circuit (r) of determinate resistance—

$$H = \frac{E^2}{R} \qquad h = \frac{r\,E^2}{R\,R}.$$

It is evident from the preceding that the (battery) electro-motor employed for medical purposes should be varied in intensity, according to the portions of the human body acting as part of the circuit. Inattention to this point frequently results in accidents, as burning the skin, causing eschars.

I have now to refer to the means to be adopted for the purpose of measuring: 1st, the electro-motive force (E) of a voltaic battery; 2nd, its resistance (r); and 3rdly, the resistance (R) of any portion of the human body, through which, by means of given electrodes, it may be required to pass the current. In the second of these de-terminations, more especially, I have studied to simplify the conditions by dispensing with the necessity for a galvanometer giving comparative indications, which has hitherto rendered the measurement of the resistance of electro-motors a matter of some difficulty; almost any ordinary galvanometer in lieu of a "sine" or "tangent" instrument will, consequently, suffice for these measurements.

Apparatus required for Electrical Measurements.

The apparatus necessary is as follows:

1. A galvanometer of moderate resistance and sensitiveness.
2. A small set of resistance coils, of which the highest resistance need not be more than a small fraction of the highest resistances which it is required to measure.
3. A Wheatstone's bridge, or parellelogram.

Measurements of Electro-motive force (E) of batteries.—The following method of measuring the comparative electro-motive force (E) of batteries is due to Professor Wheatstone, and is based upon the fact that the greater the electro-motive force, the greater will be the amount of resistance required to be inserted in the circuit in order to diminish, say by 5°, a given deflection obtained upon the galvanometer.

RULE.—Complete the circuit of the battery through the galvanometer, and add resistance to the circuit until a deflection, say, of 30° is obtained. Now introduce additional resistance until the deflection is reduced, say, by 5°. The amount of this resistance, necessary to reduce the deflection from 30° to 25°, will indicate the *comparative* electro-motive force of batteries tested under the same conditions.

Example.—Let x and x' be the electro-motive forces to be compared. In the case of x let us suppose that the resistance of the battery (r) and that of the external portion of the circuit (R), which resistance need not be ascertained, are respectively 5 and 15. Let us further suppose that the galvanometer indications represent the true intensities (I) of the currents. Then the expression for this circuit, according to Ohm's formula, would be—

$$I = \frac{x}{5 + 15} \qquad 30.$$

Knowing the intensity I to be $= 30$, and the resistance in circuit to be $5 + 15$, we are able at once to give a comparative value to x. Thus, $5 + 15 \times 30 = 600$[1]. But let us assume that neither the true intensity of the current nor the amount of resistance in the circuit

[1] This comparative value obtained by determining the resistances in the circuit and also by means of a galvanometer giving comparative indications, the intensity of the current will enable us to check the results obtained in accordance with the rule which has been given.

is known; and let us follow the steps indicated in the rule given above, through which another comparative indication of the value of x (E) will be obtained.

If we add to the circuit an additional resistance of 4 units, the deflection will be reduced from 30° to 25°, thus,

$$I = \frac{x = 600}{5 + 15 + 4} = 25\,;$$

and this number, 4, will, according to the rule, express the comparative value of $x = \mathrm{E}$.

II. In the case of x', we will suppose that $r = 5$ and that $\mathrm{R} = 5$; the formula for I will then be

$$I = \frac{x'}{5 + 5} = 30.$$

Knowing the value of $r + \mathrm{R}$ and that of I, it is evident that we may express the value of x as $x = 5 + 5 \times 30 = 300$. But if we follow out the rule assuming that the value of r, R and I are unknown, we shall have to add a further resistance of 2 units in order to reduce the deflection from 30° to 25°, thus,

$$I = \frac{x' = 300}{5 + 5 + 2} = 25\,;$$

and this number 2 will also express the comparative value of $x' = \mathrm{E}$.

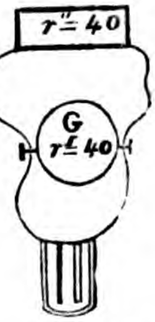
Since 600 : 300 : : 4 : 2 it is evident that the ratio $x : x'$ may be correctly determined according to the above rule; and that if, for instance, we express the value of x in terms of any given unit of electro motive force, as 600 or as 60, that of x' in terms of the same unit, will be $x' = 300$ or $x' = 30$.

Measurement of Resistance (r) *of Batteries*.—The method I employ in determining the resistance of batteries is based upon a suggestion made by Mr. Latimer Clark, relative to a system of obtaining accurate comparative measurements of the intensities of currents by means of a galvanometer, which need not give comparative indications.

RULE.—To the terminals of the galvanometer connect a coil of wire equal to the resistance of the galvanometer. Connect also to these terminals the poles of the battery of which the resistance is to be tested, and note the deflection obtained. Now disconnect the resistance coil, the deflection will be somewhat increased. Add resistance to the circuit until the same deflection is obtained as in the previous case. The resistance last added will equal (r) that of the battery.

Note.—The external resistance (R) should be small in proportion to (r).

Example.—Let the resistance of the galvanometer equal 40 of any given unit: by adding a similar resistance side by side with the former, so as to offer two paths instead of one to the passage of the electricity, this resistance is exactly halved thus $R=20$.



Let the resistance r, to be determined equal 5 units, and $E = 100$. Then I the intensity of the current will be

$$1.\ I = \frac{100}{5 + 20} = 4.$$

The intensity, however, of *the current traversing the galvanometer* G will be *one half* of the above, since one half of the electricity flows through the path of equal resistance $r''=40$ in the above figure. Let us suppose that the deflection produced upon the galvanometer by the passage of this current of the intensity $I=2$, is 10°.

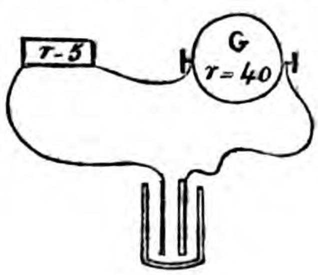
When the resistance coil is disconnected from the galvanometer, the external resistance (R) in the circuit is doubled; but the internal resistance (r) is evidently not affected. The intensity of the current is now

$$2.\ I=\frac{100}{5 + 40} = 2{\cdot}22;$$

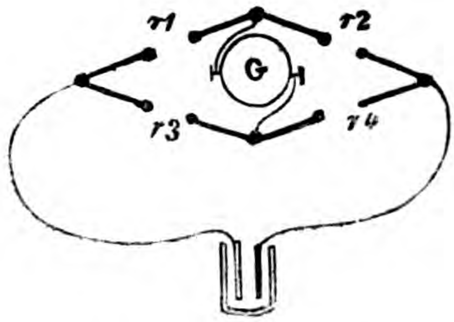
and since now the whole of the current must traverse the galvanometer, the deflection will be greater than that specified above, viz., 10°. The reason being, that the total resistance of the circuit has not been doubled, but only the external resistance $R=20$. When additional resistance is added until the deflection is reduced to 10°, the intensity of the current will be

$$3.\ I = \frac{100}{5 + 40 + 5} = 2,$$

which is the same as that of the former current traversing the galvanometer. Since, then, the whole of the present current traversing the galvanometer produces the same deflection as half of the previous current, the total resistance of the circuit has now been doubled; and the last resistance added, viz., 5 units, equals the resistance (r) of the battery which was to be determined. The following figure shows the alteration in the circuit (3).

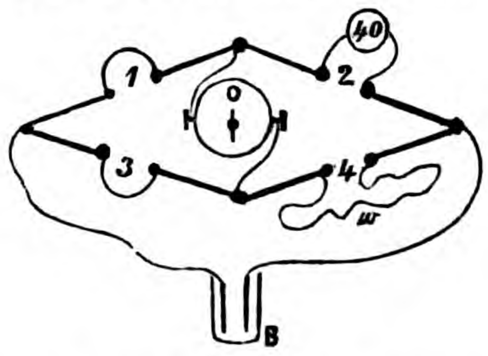


The external resistance (R), whether composed of a wire, a certain section and length of fluid, or a portion of the human body, may be measured by means of Wheatstone's bridge or parallelogram, represented below.



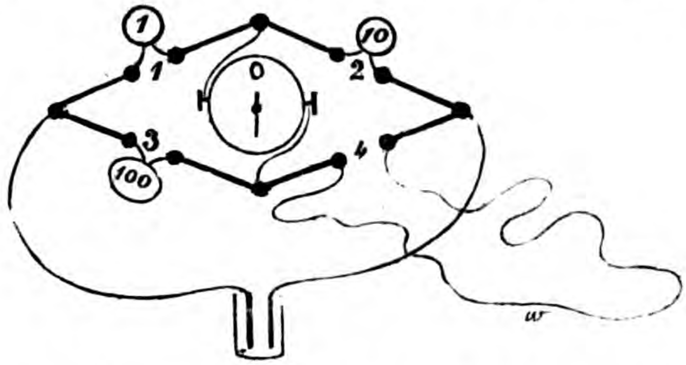
In whatever manner this apparatus may be used, the results obtained are due to this property of the instrument, viz., that when the circuit of the battery is completed by the insertion of wire or other resistances at 1, 2, 3, and 4; then if the needle of the galvanometer be not deflected, these resistances will constitute the four terms of a proportion. Thus, if the term of the proportion, viz., the resistance to be measured, be unknown, it can readily be found by means of a Rule of Three sum.

Measurements by means of two sides only of the Parallelogram.—



Examples.—In the simplest mode of measurement the sides of parallelogram 1 and 3 are completed by means of wires offering no appreciable resistance. Let *w* be the wire, the resistance of which is to be measured, it is found that the needle of the galvanometer is not deflected when 40 units of resistance are inserted at 2. The resistance of *w* will therefore be 40 units. When, however, the resistance at 4 is very high, it is preferable to employ the four sides of the parallelogram, principally because when this is done a coil of wire having, for instance, a resistance of 100 units may serve for the measurement of resistances of 1000 or more units.

Measurements by means of four sides of the Parallelogram.—



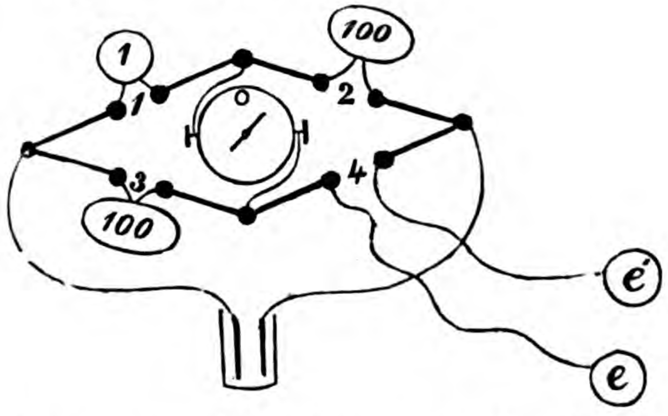
Thus, in the above figure let w be the wire of which the resistance x is to be measured: making x the fourth term of a proposition, and knowing that its value is probably greater than that of the resistance of the coils of wire at our disposal, we first make the second term of the proposition higher than the first term by inserting at 1 and 2 resistances equal respectively to 1 and to 10 units. We then insert at 4 the wire w, and insert resistance at 3 until the needle of the galvanometer remains stationary at o. We will suppose that this is the case when 100 units of resistance have been inserted at 3. We have then the proportion

$$1 : 10 :: 100 : x \therefore x = \frac{100 \times 10}{1} = 1{,}000.$$

and the resistance of the wire (w) is shown to be $=1000$ units.

Measurement of resistances of the Human body.

In the measurement of the resistance offered to the passage of the current, under given conditions, by various portions of the human body—moist electrodes e and e' are connected as follows, to the fourth side of the parallelogram, and the resistance at 2 is in most cases made very much higher that that at 1—



In the above represented adjustment, if when the electrodes e and

e' are applied to the skin so as to allow of the passage of a current through a given portion of the body, the needle of the galvanometer returns to o, we shall have the following proposition—

$$1 : 100 :: 100 : 10{,}000,$$

and the resistance of the body at 4 would therefore equal 10,000 units.

By the above described means we are enabled to test the resistance various portions of the body give to the passage of a voltaic current; and practically are made acquainted with the batteries most desirable to use under different circumstances, namely, the area of the surfaces of the electro-motive metals, and the numbers of the elements to be employed.

There are certain physiological phenomena to be taken into consideration, which must not be omitted in the practical application of electricity to the treatment of disease; but which I have not thought applicable in a paper of this nature.

PART FOURTH.

Chronicle of Medical Science.

(CHIEFLY FOREIGN AND CONTEMPORARY.)

HALF-YEARLY CHRONICLE OF PHYSIOLOGY.

By H. POWER, Esq., M.B., Lond., F.R.C.S.

PART I.—NUTRITION—BLOOD.

1. Dr. LYON PLAYFAIR: *On the Food of Man in relation to his useful Work.* (Pamphlet, Edin., 1865.)

2. Prof. A. ROLLETT: *On the Successive Alterations produced in the Blood by the Passage of an interrupted Current of Electricity.* (Moleschott's Untersuchungen, Bd. ix, 1865, p. 474.)

3. MM. ALFRED ESTOR and CAMILLE SAINT-PIERRE: *Note on the Cause of the Redness in Inflammation.* (Mèmoires de la Societé de Biologie, 1865, tom. i, ser. iv, p. 31.)

1. Dr. Playfair's essay is written with the view of ascertaining the function of the nitrogenous ingredients of our food as a magazine of force for the production of dynamical effects. He divides the work performed in the body into—1. Mental work; 2. Calorific work; 3. Internal dynamical work; 4. External dynamical work; and 5, Digestive or assimilative work. He considers that the food which is necessary for *mere subsistence*, without exercise, should contain 2·5 oz. of flesh-formers, 1 oz. of fat, 12 oz. of starch, and 0·5 of mineral matter—this diet containing 7·44 oz. of carbon. The food required to maintain adult men in good health, when not employed in hard work, he deduces from army dietaries. It should contain 4·2 oz. of flesh-formers, 1·4 oz. of fat, 18·69 oz. of starch, &c., and 0·73 of mineral matters. This contains 11·642 oz. of carbon.

For men engaged in heavy work (which he defines to be a walk of twenty miles, or the raising 792,000 lbs. one foot high), the food should contain 5·41 oz. of flesh-formers, 2·41 oz. of fat, 17·92 oz. of starch, sugar, &c., 0·68 oz. of mineral matters, and a total quantity of carbon = 12·71 oz.

Dr. Playfair then proceeds to discuss the question, whether there be sufficient potential energy in the nitrogenous tissues, or of the food representing them, and in the oxygen required for their transformation, to account for the dynamical actions within and without the body. This he answers in the affirmative, though admitting that the economy of material observed in the body is of a very extraordinary character, which he illustrates in an interesting manner, by pointing out that it would take from 1000 to 1200 grammes of coal burned in a steam-engine to raise a man from the level of the sea to the top of Mont Blanc, though the same man could do the work in two days by the transformation of 198·4 grammes of dry muscle. He fully coincides with Liebig, in regarding the non-nitrogenous portions of our food as mere heat-givers, never taking the place of albuminous bodies as tissue-formers, although tissues may and do evolve heat by transformation when required to do so. He scarcely considers the "luxus consumption," *i. e.*, the conversion of albumen into urea in the blood, before forming tissue, as possible, or at least as what occurs in normal nutrition, unless when very great excess of food is consumed; and he considers that the excretion of urea is unquestionably augmented by exercise.

From these statements it may be gathered that he regards the urea as directly originating in the metamorphosis of the proper muscular tissue, the result of internal and external dynamical work.

The carbonic acid, on the other hand, evolved in the respiratory acts, is partly the result of the dynamical work and partly the consequence of the opus calorificum.

Finally, he considers that about one twelfth of all the nitrogen injected is expelled per anum.

2. Dr. Rollet shows that the well-known change of the blood-corpuscles into stellate bodies, which is usually regarded as of a spontaneous character, can be produced at will by the passage of an interrupted current of electricity; soon after this has occurred the corpuscles yield up their colouring matter to the serum, and this is followed by their crystallization. These changes do not always occur coincidentally in the corpuscles of the same specimen of blood, some being more easily and readily acted on than others. Before the blood-corpuscles lose their colour through the action of the electric current, they behave like drops of some substance which is not miscible with water. But if they have undergone the succession of forms (cup-shaped, changing to rosette-like, mulberry-like, stellar, induced by the successive electrical discharges) till they have reassumed the globular shape they will coalesce and form larger globules. The nuclei (in frogs' blood) can be seen to detach themselves from such drops without any loss of colour or sharpness of outline in the latter. No similar changes of form can be shown to occur in the red blood-corpuscles whilst they are still contained within the vessels. In the capillaries of the frog the only changes observable are of a passive nature, as extension and bending. Such changes are, however, much more frequently to be seen in the blood-corpuscles traversing transparent parts (mesentery) of mammalia.

The electrical discharge makes not only arterial bnt venous blood transparent; and effects even the same change in blood saturated with carbonic oxide. Blood which is thus made transparent undergoes the same changes in colour as normal blood when exposed alternately to oxygen and to carbonic acid.

To give an exact explanation of the effects produced by electricity on the blood is not yet practicable, but it seems certain that no direct mechanical influence on the corpuscles is exerted by the discharge, or the succession of changes in form would not be of so regular and systematic a character.

3. MM. Estor and St. Pierre have made investigations on the pneumatology of the blood coursing through inflamed parts, as the foot of a dog seared with the actual cautery. They estimated the amount of oxygen present by treating the blood with carbonic oxide, as recommended by Bernard, and obtained the following remarkable results:

Experiment.	Inflamed side, amount of O in 100 parts of blood (venous).	Sound side, Ditto.
1.	6·01	2·41
2.	6·04	2·40
3.	4·74	2·36
4.	3·60	2·40
5.	4·80	2·40

They conclude from these and other experiments—

1. That the venous blood returning from an inflamed part contains constantly more O than the blood of the sound side, the proportion being as 1 : 1·50 or 2·50.

2. That the venous blood of the inflamed side contains more CO^2, and

3. That it is to the excess of O in the venous blood, rendering it of brighter tint, that the increased redness of an inflamed part is due.

Part II.—Respiration.

1. M. Paul Bert: *On the Length of Time required to produce Asphyxia by Submersion in various Species of Warm-blooded Animals.* (Gaz. Médicale, 1865, p. 79.)

2. MM. Alfred Estor and Camille Saint-Pierre: *An Experimental Inquiry into the Seat of the Respiratory Processes of Combustion.* (Robins' Journal de l'Anatomie, 1865, p. 302.)

3. H. Lossen: *On the Excretion of Ammonia by the Lungs.* (Zeitschrift fur Biologie, Band i, p. 207.)

4. M. Marey: *Essay on the Graphic representation of the Respiratory Movements.* (Memoires de la Societé de Biologie, 1865, p. 175.)

1. M. Bert remarks, that when a warm-blooded animal is submerged, violent agitation of the body occurs, followed by a period of quiescence, with deep inspiratory movements; these diminish, then cease, and the animal, sometimes after a forced expiration, remains motionless. He dates the period of death from the commencement of this last period of quiescence. The sensibility of the animal is soon abolished, but the heart continues to beat for a variable period. The duration of life is scarcely in any way connected with the size of the animal. A rail, about the size of a thrush, retained life for four minutes thirty seconds, whilst immersion for one minute thirteen seconds, on the average, killed pigeons. Wrens, however, died in twenty seconds. Perhaps it may be stated very generally, that small birds are sooner asphyxiated than large ones. Violent movements accelerate death. A fowl, which remained very quiet, lived for four minutes forty seconds, though these birds usually die in three minutes thirty-eight seconds. The withdrawal of blood from the carotid artery or jugular vein of rabbits exercised no notable influence on their power of resisting asphyxia. No difference occurred between fasting and recently fed animals. Wounds and fatigue accelerated death. In an addendum to the preceding paper, M. Bert discusses the question, whether mammals, when plunged in water, draw the fluid into their lungs by aspiration? He gives the results of several experiments, and maintains that little or no water enters during the first period of submersion, when the animal is violently agitated, the glottis being then spasmodically closed; but that when fluid is found in the lungs, the quantity of which is very different in different instances, it enters during the second period, when loss of consciousness has taken place, and the animal makes some involuntary inspirations, the contraction and closure of the glottis sooner or later giving way. Much of the water that gains entrance may be absorbed by the pulmonary veins.

2. The conclusions at which MM. Estor and St. Pierre arrive are:

1. That the respiratory oxidations take place exclusively in the blood, and are not limited to any particular part of its course, continuing during the whole period of the passage of the blood from the lung till it arrives at the lung again.
2. That they are very active in the arterial system.
3. That the capillaries only augment the venous character of the blood by retarding its course.
4. That the respiratory processes of oxidation are progressive: that in the arterial system they are direct or indirect causes or consequences of reduction, whilst in the capillary and venous systems they are complete, extending to the destruction of the compounds.

3. Lossen refers to the results of previous experimenters, especially to those of L. Thompson, Regnault and Reiset, and Thierry, and describes an apparatus he has devised for determining the amount of ammonia eliminated by the lungs. He finds that the

quantity is scarcely appreciable, amounting to only ten milligrammes per diem, and is of opinion that this is not developed or generated in the blood or tissues, but that it originates during the passage of the air through the air-passages—the various secretions and epithelial formations which are thrown into these tubes, the presence of carious teeth, and the decomposing remains of food in the crevices of the teeth, even of the healthiest person, being sufficient to account for the presence of the exceedingly small quantity observed.

4. M. Marey observes that the respiratory movements are well known to vary in different pathological states, becoming less frequent in meningitis and in certain cerebral affections, whilst they are accelerated in pleuritic effusions and in acute diseases of the lungs. But the alteration in the form of these movements, the mode in which the chest expands and collapses, and the exact relation which the duration of the act of inspiration bears to that of expiration, are all circumstances which usually escape our notice. Vierordt and Ludwig, who first endeavoured to obtain some visible and persistent representation of these changes, employed the sphygmoscope, invented by the former experimenter, and deduced from their observations the two following general conclusions:

1. That the height of the curves is directly proportioned to the quantity of air expired.
2. That the extent of the curves becomes less in proportion to the increase of frequency of the acts of respiration.

In M. Marey's experiments the cardiograph was employed, which he had previously used in his experiments with M. Chauveau. This was attached to a kind of elastic sac surrounding the chest. Each dilatation of the chest so acted upon the sac, as to cause a lever connected with it to fall. Each contraction of the chest, on the contrary, effected the elevation of the lever. In his experiments on the action of the heart, M. Marey ascertained that—1. All obstacles to the flow of blood occasioned by the systole of the heart retarded the heart's action; and 2. That all circumstances which facilitated the systole augmented its frequency. He thinks he has proved that the same law holds in respect to the respiratory as well as to the circulatory function. Thus, if we compare the effects upon the number of the respiratory acts of breathing with all the passages patent, and of breathing with more or less occlusion, as through a narrow tube, we shall find the frequency of the acts to be greater in the former than in the latter case. And under these conditions (*i. e.* of narrowed passages) the depth of the acts of respiration increases as their frequency diminishes, so that there is an evident attempt to effect, within certain limits, the introduction of the same quantity of air into the lungs in a given time. But constriction of the respiratory passages is not the only mode in which a mechanical obstacle may interfere with the acts of respiration; embarrassment may arise from compression of the chest, or even of the lung itself. This does not appear to follow quite the same law, since in this case there is not so much a definite resistance to be overcome, but rather a limit to the possible extent of the respiratory movements. When considerable circular pressure was exerted on the trunk, the depth of the respiratory acts became small, whilst their frequency augmented.

Under the term rhythm he designates the relative duration of the two periods of inspirations and expiration.

Under normal conditions inspiration is shorter than expiration; it represents about one third of a whole respiratory act. In the cases above mentioned the obstacles to the passage of the air, in retarding respiration especially act in prolonging the inspiratory period. M. Marey has constructed an instrument (a tube with a valve) by which the hindrance to respiration may be made to act either during inspiration or expiration, and has found that if the act of inspiration is rendered difficult there is an increase in the length of that act; if the act of expiration is constrained, then it is this act which is prolonged. He applies these results to practice, by taking an example of nervous asthma, which may essentially consist either of a kind of spasm or of a paralysis of the bronchial tubes. In the former case there ought to be an obstacle to the act of inspiration, and a consequent prolongation of the duration of that act. On the second hypothesis the inspiration is easy, whilst the expiratory force is defective, and the expiratory period should be prolonged. Clinical investigation will furnish the solution of the question, and will show, perhaps, that the two causes may concur.

Digestion.

1. Dr. Krishaben: *Auto-laryngoscopic Experiments made for the purpose of ascertaining the Mechanism of Deglutition.* (Comptes Rendus, July, 1865, tom. lxi, p. 52.)

2. M. H. Guinier: *Experiments on Deglutition, made by Means of Auto-laryngoscopy.* (Comptes Rendus, May 1st, 1865, and July 3rd, 1865, vol. lxi, p. 53.)

3. J. C. Dalton: *Experimental Investigations to determine whether the Garden Slug can live in the Human Stomach.* (American Journ. of Med. Science, April, 1865, p. 334.)

1. M. Krishaben states, that in the act of deglutition the alimentary bolus passes along one of the pharyngeal channels on either side of the epiglottis, which last is raised by the elevation of the larynx; the bolus, consequently, enters the œsophagus at the moment when, by the contraction of the constrictor muscles, the pharynx is diminished in size, and is brought up against the bolus. The deglutition of liquids is effected in the same way, except that these pass pretty frequently over the epiglottis itself, which rarely occurs with solid aliments. A small quantity of fluid, when liquids are drunk, enters the larynx around the margin of the epiglottis and may even creep down to and moisten the vocal cords. In the

act of gargling, the larynx is widely open, and a large quantity of fluid passes into the interior of the vocal organ.

It is easy to bear with the presence of an alimentary bolus in the respiratory tract, *i. e.* in the larynx, even on the vocal cords, and in the interior of the trachea. The sensibility of the trachea to the contact of foreign bodies is infinitely less than that of the larynx. The contact of hard and cold bodies with the mucous membrane of the larynx cannot be tolerated; but the contact of soft and moist bodies, of the same temperature as the body, can be sustained for several minutes without occasioning any cough, or other inconvenience. For the performance of these experiments but little practice is required.

2. In the number of the 'Comptes Rendus' for May 1st, 1865, is a communication from M. Guinier, of Geneva, tending to show that during normal deglutition the alimentary bolus enters the larynx, and penetrates into the larynx as far as the vocal cords before passing into the œsophagus. It is evident that M. Guinier has been led into error by the insensibility of his mucous membrane. In fact, in his experiments he voluntarily permits the bolus to fall into the larynx, instead of making the movement of deglutition, and of transmitting it normally into the larynx.

3. Dr. Dalton refers to the occasional accounts of cases in the medical journals, in which various cold-blooded animals, especially slugs and lizards, are said to have remained alive for a considerable time in the human stomach or alimentary canal, and details one or two remarkable cases that occurred in his own practice. He points out, however, the difficulties that lie in the way of admitting any explanation of this nature—as, the improbability of an animal so large as a slug or snail having been recently and inadvertently swallowed, and the equal improbability that it should have been hatched from the egg or grown from a very small size; the injurious and probably rapidly fatal effects of a temperature of 100° to a cold-blooded animal; the softness, and, as is shown in the case of the oyster, the easy digestibility of the living animal; and the necessity, in the case of the slug or snail, of air for respiratory purposes. Notwithstanding these difficulties, he still thought that some more exact experiments than those hitherto made might be set on foot, and he accordingly introduced living and active slugs into the stomach of healthy dogs, taking care that no injury was occasioned by mastication, the dogs being killed after the lapse of various periods. No traces of the slug were found in one instance after twenty-four hours; in another case none after only one hour; in a third case the animals were found all dead after the lapse of fifteen minutes. Fresh gastric juice from a dog, at the temperature of 100°, killed four slugs in nine and a half minutes, and at the expiration of five hours they were thoroughly disintegrated. Mere immersion in pure water at a temperature of 100° was sufficient to prove fatal. In another experiment two living water-lizards, *Triton mille-punctatas*, were adminis-

tered to a young dog. In fifteen minntes they were both found in the stomach, dead, soft, and flaccid. From the similarity of human gastric juice to that of the dog, there can be little doubt but that the effects would be analogous in the case of similar animals being introduced into the stomach, and that consequently the cases of their supposed long-continued residence in the stomach are unworthy of credence.

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1. M. BROWN-SEQUARD : *On the Transmission of Impressions through the Spinal Cord.* (Journal de la Physiologie for October, 1863, published in December, 1865, No. xxiv, p. 645.)
2. M. A. VULPIAN : *Remarks on the Statements of MM. Jacubowitsch and Roudanowsky respecting the Action of Certain Poisons on the Anatomical Elements of the Central Nervous System.* (Gaz. Méd., 1865, t. xx, p. 117.)
3. DR. KLEBS : *On the Mode of Termination of Nerves in Organic (unstriped) Muscular Fibre.* (Virchow's Archiv, 1865, p. 168.)
4. DR. BASILIUS ROSOW (of St. Petersburgh) : *Experiments on the Effects of Division of the Optic Nerves.* (Sitzungsbericht d. k. Akad., d. Wiss. zu Wien, B. xlix, 1864, p. 431.)
5. A. F. SPRING (of Luttich) : *On the Relation existing between the sense of Temperature, the sense of Touch, and the sense of Pain.* (Presse Médical., 1864, No. 34.)
6. M. JULES CAYRADE : *Critical and Experimental Researches on the nature of Reflex Movements.* (Comptes Rendus de la Soc. de Biologie, t. i, ser. iv, 1865, p. 25.)
7. DR. A. HERZEN : *On the Centres which exert an inhibitory influence on Reflex Actions.* (Moleschott's Untersuchunger, 1865, Band ix, p. 423.)
8. DR. H. NEUFELD : *Upon the action of Upas antiar on the Heart.* (Studien des Physiolog. Instituts, zu Breslau, Heft iii, Leipzig, 1865.)

1. In the present number of the 'Journal d'Anatomie,' M. Brown-Séquard concludes a long paper on the "Transmission of Impressions in the Spinal Cord," for the chief conclusions of which we have only here space. He maintains that in man, after any injury damaging the whole transverse thickness of a small portion of one lateral half of the cord there may be observed—A. On the same side. 1. Paralysis of voluntary motion. 2. Hyperæsthesia for sense of contact, for tickling, for pain, and for temperature in the paralysed part. 3. An anæsthetic zone of small extent, corresponding to the parts supplied by nerves which take their origin from that part of the spinal cord

situated immediately below the lesion. 4. Hyperæsthesia, in a greater or less extent of surface, above the zone of diminished sensibility. 5. Absolute or relative elevation of temperature in the paralysed parts, and often also in those parts of which the sensibility is exalted, but not in the parts which are not paralysed. 6. Phenomena indicating paralysis of the origins of the great sympathetic nerve in the neck, when the lesion has occurred in the cervico-brachial enlargement. B. On the opposite side. 1. Complete anæsthesia as regards contact, tickling, pain, and temperature, in those parts which correspond to the ones that are paralysed on the opposite side. 2. Perfect preservation of the voluntary movements, and of the muscular sense. 3. A zone of exalted sensibility of small extent, and feeble in degree, situated in the parts above those the sensibility of which is diminished.

In animals M. Brown-Séquard has also observed occasionally amaurosis, ulceration of cornea, hypertrophy of supra-renal capsule, epileptic convulsions, and general augmentation of the vital properties of both the muscles and nerves in the hyperæstheticised parts.

From these observations he considers himself justified in drawing the conclusion, that there is an absolutely complete decussation of the conductors of the various kinds of susceptibility, with the exception only of those conveying the muscular sense. Further clinical facts appear to demonstrate—1. That each of the four species of sensibility referred to above, possesses its own proper conductors which are distinct from those of the other kinds. 2. That each of these species of conductors occupies a distinct part of the spinal cord; and, lastly, that at the upper part of the cervical portion of the spinal cord the four species of conductors coming from the abdominal members and a great part of the trunk, form a group placed behind a similar group constituted by those proceeding from the thoracic members. For numerous cases, and much ingenious reasoning in support of these propositions, the reader is referred to the paper itself, which will well repay perusal.

2. M. Vulpian has repeated Roudanowsky's experiments. (See "Report on Physiol. Microl." in this Journal, April 1865, p. 527), and is unable to substantiate them. He thinks the pigmentary deposits observed after the administration of nicotine were preexistent, and points out the improbability of the sudden formation of pigment. Nor has he been more fortunate in observing destruction of nerve-cells, or of their prolongations. He thinks such destruction incompatible with the fact, that a moderate dose of nicotine may induce a state of apparent death, from which nevertheless the animal may perfectly recover. This state of lethargy is remarkably permanent in frogs, lasting far from twenty-four to forty-eight hours. The same holds with strychnia.

3. Dr. Klebs' observations were made on the bladder of the frog, preserved in a five per-cent. solution of cane sugar, feebly acidified

with sulphuric acid. After describing the characters of the muscular fibres which present no points worthy of notice, he remarks that the larger trunks of the nerves are arranged in the form of a plexus, the several branches presenting well-marked dark edges; on tracing them onwards, they rapidly dwindle into pale nerves, composed of numerous fibrillæ, which soon separate from one another and appear as flat, homogeneous, "pale fibres." These constitute a plexus termed by Klebs the "intermediate" plexus, since from it the branches directly proceed, which are distributed to the muscular fibres. On examination of the dark-edged nerve-fibres he finds that a "periaxial fluid" exists between the primitive band of Remak and the nerve-sheath, and from the results of examinations made with polarised light, he believes that this "periaxial fluid," or white substance of Schwann, contains numbers of solid bodies or crystalline elements, arranged in a definite order. The fibrillar nerve-fibres of the sympathetic are difficult to discriminate from connective tissue, occasionally contain ganglion-cells, and, when the fibrillæ are isolated, are usually found to be varicose; the dark-edged fibres can frequently be seen to terminate in them. The intermediate plexus which they form is composed of very delicate fibres, and has been particularly described by Dr. Beale, who considers it to be terminal, but Klebs believes that from it a still more delicate set of fibres is derived, not exceeding 3-10,000ths of a millimetre in diameter, and presenting well-marked varicosities; these frequently bifurcate, the branches being of the same size as the parent stem, and pursuing a looped course. At the points where they anastomose a small triangular swelling occurs, in which, however, no nucleus can be perceived. Near the point where they enter the muscular bundle to which they are distributed, they form a pear-shaped swelling, or "nervous knot," which sometimes contains a nucleus. On entering the muscular bundle the delicate nervous fibre immediately splits into two branches, which pursue diametrically opposite directions, and are lost in its interior. Later experiments (Klebs, ii, p. 191) show that with the larger bundles of muscular fibres a somewhat different arrangement exists. In the centre of the bundle a good-sized nerve-trunk, composed of pale fibres, may often be found, which gives off fibrillæ, that at once form, as before, a plexus, and from this the terminal varicose fibrillæ originate. The nervous knot is now spindle-shaped, and can be distinguished from the nuclei found in the nerve-fibre by its form and appearance. The ultimate branches apparently dip into the thickest part of the muscular fibre-cells, and divide into two branches, running in opposite directions to the two pointed extremities. Klebs believed that he had indeed observed the direct fusion of the two structures, but more extended and exact observations have led him to believe that the nerve-fibre only runs along the edge of the muscular fibre, exactly filling up the space which exists between two neighbouring ones, and terminating in a row of, to all appearance, separated points.

4. In Dr. Basilius Rosow's "Experiments on the Effects of Division of the Optic Nerve," the animals operated on were rabbits. The

superior rectus was divided in the ordinary mode of operating for strabismus—the distal end of the tendon seized, and the eye rotated downwards. The retractor bulbi was then divided, and, finally, the optic nerve. The posterior ciliary vessels and nerves were uninjured. Scarcely any blood was lost. A few minutes after the operation the pupil was usually contracted and irregular; in a few instances it was unchanged in form and dimensions. Ophthalmoscopic observation showed the optic disc whiter than natural in some cases, in others with a round or oval dark-brown spot in the centre, especially when the section of the nerve was made close to the sclerotic. The retinal vessels were small in most cases, but sometimes only the arteries were constricted, and occasionally no change was apparent. The choroid and iris were always anæmic.

Twenty-four hours after the operation the cases could be divided into two groups, according to the appearances presented. A. In a few cases panophthalmitis set in, with œdema of lids, congestion of conjunctiva, and a characteristic diffuse, bluish-gray haziness of the cornea, completely obscuring the pupil and iris. Forty-eight hours after the operation the cloudiness of the cornea remained in *statu quo*, but there was remarkable diminution of the tension of the globe. The epithelial layer of the cornea was undisturbed, but a discharge of pus began to take place from the conjunctiva. One rabbit was killed after three days, a second after five days, and a third after fifty-two days. In the last case there was extreme atrophy of the eye.

On *post-mortem* examination the cornea in the first two cases was found to be greatly thickened, and this was effected in such a manner as to render the posterior surface strongly convex, whilst the curvature of the anterior surface was scarcely altered. The projection was so great as to fill the anterior chamber, and to press the iris and even the lens backwards. In the third case the membrane of Descemet was attached to the front of the iris. In the second and third cases there was complete absorption of the vitreous humour, so that the posterior surface of the lens was in contact with the retina.

B. In the second class of cases there was no inflammatory reaction set up. Twenty-four hours after, there was slight œdema of the upper lid; the pupil was much dilated; the papilla red, and not well defined. The retinal vessels normal, or the veins alone dilated. The brown discoloration when this was previously present, remained unchanged, and the circulation in the choroid and iris was undisturbed. In three to five days all signs of inflammation passed off, and the only difference between the sound eye and the one which had been operated on was the wider pupil of the latter. In six of these cases (one of which was examined as late as fifty-one days after), the ophthalmoscope showed indistinctness of outline in the reddened papilla, and in many cases dilatation of the veins.

Microscopic examination of the retina in two instances (twenty-fifth and thirty-ninth days after the operation), showed that the retina preserved its morphological elements unaltered to a surprising degree; many groups of nerve fibres appeared to have undergone fatty degeneration, whilst others only appear to be paler than natural.

5. The following interesting case of Dr. Spring is almost unique, and affords a strong confirmation of the opinions of those who think the sensations of temperature, pain, and pressure, are conveyed through separate channels, or are perceived by separate centres. The patient was a female, aged sixty, who had long suffered from hypertrophy of the heart, dyspnœa, and persistent bronchitis. From exposure to cold she became paralysed, though without loss of consciousness or deviation of the tongue when that organ was protruded. The entire right half of the body, including the head, became insensible to temperature and to pain, but there was no loss of motor power; the muscular power, in fact, as measured by the dynamometer, being somewhat increased on the affected side. She could feel the slightest touch on the anæsthesiated (?) side, and, when the eyes were closed, she could discover and pick up a pin from the floor. On washing the hands she could distinctly perceive the shock and movement of the water flowing over them, but was quite unable to distinguish whether it was hot or cold. In winter she could only perceive the temperature with the left half of the body, and the same when standing near a fire. The normal temperature of the skin on the affected side was maintained in every part, or differed only to the extent of 1° or 2°. Neither the pricks of needles nor strong pinching was perceived in the slightest degree. She suffered from neuralgia in the temporal region at night. In consultation with M. Schwann, the author ascertained that there was no diminution in the acuteness of the patient's perception in regard to impressions of weight and of contact. The hand lying prone on a table, and weighted with 500 grammes, readily distinguished the addition or removal of two or three grammes, and when weights were concealed in a cloth, and the amount estimated alternately by the two arms, no difference was remarked. From experiments made in the method suggested by Weber for determining the delicacy of touch by applying the points of compasses, it appeared that there was a considerable diminution of acuteness on the left, or healthy side, but a still more marked diminution on the right side. On the eighth day after this consultation the sensibility to pain returned, under the form of a painful formication, and from this time every object appeared *hot* to the patient, so that she was unable to distinguish ice from water at a temperature of 122°. This state lasted two months, when death occurred from an attack of apoplexy.

In this case the sense of variation of temperature, instead of being associated with tactile sensations, followed the same course as the sensations of pain, disappearing and reappearing, though modified with the latter. The muscular sense was intact, and the sense of touch was only deteriorated in regard to its perception of distance. The cause of these abnormal conditions was evidently seated in the nervous centres.

6. The following are the chief points noticed by MM. Vulpian,

Robin, Gubler, Charcot, and Martin Magron, in their report of M. Cayrade's thesis, to which the Godard Prize has been awarded by the Society of Biology in Paris.

After reviewing the chief ancient and modern doctrines respecting the nature of ordinary reflex actions, M. Cayrade lays down the following definition: "Reflex movements are involuntary movements consequent upon an impression." The only parts requisite for their due performance are the spinal cord, medulla oblongata, and its extension into the corpora quadrigemina. He believes that the impressions producing reflex movements are transmitted through the same channels as those of sensibility and of voluntary movement, but that they are not perceived by the mind; and further, that the power of effecting this reaction does not belong to the whole mass of the spinal cord, but only to its gray substance. Stimuli applied either to the nerves of organic life or to those of ordinary sensation, act through the intervention of the spinal cord. The will exerts a powerful controlling action over the phenomena of reflex action. M. Cayrade then considers successively the influence of temperature, of the blood, of sections of the spinal cord at different heights, of the nature and amount of the stimulus applied, &c. In examining the reflex movements which are limited to the muscles and nerves of organic life, M. Cayrade, with other modern physiologists, recognises in the ganglia of the great sympathetic not an absolute autonomy, but the power of acting as centres of reflex actions, and then proceeds to consider the power of certain toxic substances on the excito-motor energy. He divides the poisons into those which augment and those which diminish the reflex powers, and at the head of the former he places strychnia. He considers that this alkaloid acts on the spinal cord, not by directly exciting it, but by rendering it excitable. If moderate and repeated irritation be applied, the cord loses, without violent shocks, this excess of power, and recovers it again with a rapidity five or six times as great as when it is in the normal state; from whence he points out the utility of exciting those thrown into a tetanic state with this poison, at frequent intervals and with moderate stimuli. In one experiment, M. Cayrade, after having poisoned two frogs with strychnia, left one in a state of repose, whilst he irritated the other at intervals of ten minutes; the latter survived, whilst the former died. By administering very small and carefully graduated doses of strychnia, M. Cayrade has been able to observe other phenomena besides the convulsions and tetanic attacks, which have been so universally recognised. He has established the occurrence of an excess of contractility in the extensors, the observation of which phenomenon is a *point de départ* for him in a series of investigations that are full of interest.

In his opinion, this predominance of the extensors must be due either to the supremacy in force and energy of the extensors over the flexors, or to the exclusive action of strychnia on the former set of muscles, and he has consequently instituted experiments with a view of determining which of these views is correct. In the first place, he has satisfied himself, by direct examination of the posterior extremities of a frog from which the skin has been stripped off, and which has been poisoned by strychnia, that during the tetanic spasms the extensors are very hard, whilst the flexors preserve a certain degree of softness. Secondly, he has divided in one frog the extensors and in another the flexors, and he has observed that no tetanic attack can be induced in the former. And thirdly, he has divided the extensors of one side whilst those of the other were left intact; and in this case, again, tetanic convulsions only occurred on that side on which the parts were uninjured. Lastly, having denuded the inferior extremity of a frog, isolated the nervous trunk, and divided the extensors and flexors, both in the thigh and leg, he poisoned the frog with strychnia, and then distinctly observed, with each convulsive quiver, the superior segment of the extensors contract from one to two millimetres, whilst the flexors underwent no diminution in length. These statements are somewhat hesitatingly received by the commission, and M. Gubler, the reporter, suggests that there may exist in the spinal centre, a region exercising control over the extensor muscles, and that this region possesses an inferior condensing power for the excitor-motor energy than that governing the flexors; whence, with equal tension, greater intensity in the discharges, and more violent contraction of the muscles, supplied by nerves emanating from the former, M. Gubler ingeniously compares the mechanism of the centres governing the flexors and extensors to an electrical machine acting in dry air, the conductor of which terminates at one end by a round knob, and at the other by an elongated ellipsoid; a much severer shock will be experienced from the latter than from the former, with equal tension. And again, when the air becomes moist, and thus an indifferent conductor, the electric fluid is retained, to some extent, in the ball, but is rapidly lost from the pointed extremity, and the tension is reduced to nothing on that side. Hence some explanation may be afforded of the fact that the extensors suffer more than the flexors from the deprivation of nervous influence in cases of medullary asthenia, since an unknown anatomical disposition renders their proper centre of innervation less fitted to condense and retain force.

M. Cayrade then investigates the action of morphine, to the history of which he contributes nothing of importance, and also of the alkaloids narcotin and picrotoxin. The action of the two latter resemble, whilst at the same time they differ from that of strychnia. They appear to act generally on the gray substance of the spinal cord, occasioning convulsions of the whole muscular system. The effects of picrotoxin are very singular, often throwing the animal, in the act of performing some movement, into the most bizarre position, and in fact inducing a condition which may be termed *cataleptic*.

7. Dr. Herzen's observations are a commentary on the theory propounded by Setschenow, and briefly given in the physiological report of this Journal for January, 1864. His experiments have led him to conclusions of an opposite character to those of Setschenow, for he maintains—1. That strong irritation of any considerable portion

of the central, or even of the peripheric portions of the nervous system is itself capable of producing great and persistent depression of the reflex activity. 2. That removal or simple division of any considerable part of the central or peripheric portions of the nerve-system from the rest, has, as a consequence, a considerable increase in the reflex activity of the remaining portions. 3. That there exists no inhibitory mechanism, and, still less, any inhibitory centres in the brain; and he adds, with some humour, that with the collapse of this theory, he trusts that no further inhibitory influence will be exerted from this quarter on the further progress of physiology.

8. Dr. Neufeld made a series of about fifty experiments on frogs, with ʒss of *Upas antiar* poison (*Antiaris toxicaria*), which have led him to dispute the view of Kölliker, that it is a paralysing agent. A minute portion of the poison was injected into the dorsal lymphatic heart of the animals. Five to ten minutes after the injection the apex of the ventricle during the period of diastole became white in colour, and remained contracted; the ventricular pulsations then became irregular, and peristaltic in their character; the ventricle was imperfectly filled with blood, and its pulsations became less frequent, owing chiefly to prolongation of the act of contraction. The auricles in the meanwhile continued to pulsate with their usual regularity, so that it often happened that the auricles contracted twice or even three or four times for each ventricular contraction. At length, in about eight to ten minutes, the systole of the ventricle having previously become greatly protracted it ceased altogether, the ventricle remaining firmly contracted. After a while slight relaxation occurred, blood being again propelled into the ventricles by the auricles, there then appeared slight flickerings and convulsions of the muscular fibres, especially if the animal struggled or the heart had been touched. On pricking the ventricle with a needle it relaxed, passing into the condition of diastole, which was again followed by contraction, but this ventricular diastole was only passive, and dependent upon the strong reaction of the auricles to the stimulus effecting the distension of the ventricle. Half an hour after the injection of the poison the action of the auricles ceased, though the animal often survived the cessation of the heart's action for half an hour. Dr. Neufeld remarks, that the experiments he made with this poison agree in many particulars with those described by Dr. Braidwood, made with the poison termed "Dajakseb," from Borneo.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. Lond.

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I. The Non-pregnant State.

Uterine Tumours removed by Abdominal Section. By Dr. H. B. Sands.—The following case presents a warning illustration of the danger attending the removal of uterine tumours by gastrotomy:—Dr. Sands was consulted by a woman aged forty-five, who had been conscious of the presence of an abdominal tumour for seven years; it began in the left iliac fossa, and when seen, the abdomen was of very large size. There was also an umbilical hernia. The tumour consisted of irregular masses, and no fluctuation was made out. The uterine sound penetrated four inches. Gastrotomy was performed; a very large incision was necessary; the mass of tumours was bound in the pelvic cavity by adhesions, but the operator persevered in dissecting it out; there was considerable hæmorrhage, and the mass was divided above the line of implantation of the vagina into the cervix uteri. Hæmorrhage was found springing from a rent in the common iliac vein. "The summit of the bladder was unavoidably ruptured." The patient died a few minutes after the completion of the operation. The tumour weighed sixteen pounds, and in one part were several cysts, containing serum. (In this case the absence of fluctuation, the penetration of the uterine sound four inches, and the solid masses felt through the abdomen pointed with sufficient clearness to the fibroid nature of the tumour. I believe it ought to be accepted as a rule in practice, that whenever a tumour is felt to be very solid and firm, giving no evidence of fluctuation, gastrotomy should not be performed. The probability, in such a case, is that the tumour is not ovarian. R. B.)—*New York Med. Jour.*, Dec. 1865.

Successful removal of Uterus and Ovaries (with fibroid Tumours). By Dr. H. Storer.—Dr. Storer relates the sixth case in which the uterus has been removed by gastrotomy successfully. He precedes his narrative by an historical retrospect of the subject. His case is briefly as follows:—A single lady, aged forty-seven, had very large abdominal tumours; she was larger than a woman with twins, and was greatly prostrated. Adhesions of the tumour to omentum were found. One large mass was first separated by écraseur, in order to get more easily at another which filled the pelvis. The pelvic mass was largely attached; a clamp was passed beneath it. Excision was then accomplished by écraseur. Free hæmorrhage took place, which was checked by three hours' exposure to the air. She recovered well. The tumour weighed thirty-seven pounds, containing thirteen pints of fluid. The two ovaries were found attached. The tumour was fibro-cystic.—*Amer. Jour. of Med. Sc.*, Jan., 1866.

Bifid Uterus and Double Vagina. By Dr. A. B. Hoyt.—The physiological history of cases of double uterus is interesting. Dr. Hoyt relates a case of a woman who died at fifty-seven. She had given birth to three children; labours all difficult. When pregnant menstruation was always suspended. After death two vaginæ were found; the septum extended from just within the vulva to the uterus; it consisted of compact cellular tissue. Close to the uterus the vaginæ communicated with each other through an opening of about a fourth of an inch in diameter. From each vagina a probe passed into a separate uterine cavity. The organ was hardly larger than the normal uterus; but about one inch and a-half from the os it bifurcated into two symmetrical cornua, as large round as the forefinger, and about one and a-half inches long; these terminated in the Fallopian tubes which, with the ovaries and broad ligaments, were natural. There was nothing to indicate that one side of the uterus had been impregnated and not the other, unless it was the greater capacity of the left vagina.—*Amer. Jour. of Med. Sc.*, Jan., 1866.

II. Labour.

Narrowing of the Pelvis, with consecutive Putrescence of the Fundus Uteri. By Dr. Scharlau.—The following case illustrates the practice in Germany in difficult labours, and gives an instance of acute gangrene, or, as it has been called by Rokitansky and Klob, putrescence of the uterus. A primipara, aged thirty-four, fell in labour on the 3rd Sept., 1865. On the 5th the os uteri was fully dilated, and the forceps was applied. The "strongest tractions" were used in vain. Then a colleague assisted during two hours in a second application of the forceps. This failing, the patient was taken to the lying-in institution at Berlin. She was then very much exhausted; pulse, 128. A laceration of the vagina extending to the fundus was felt. The os uteri was rent in several places. The head presented. The child being alive, it was not thought justifiable to perforate. She was ordered ten grains of Dover's powder, and left to rest. After an hour and a half, the fœtal heart being no longer heard, perforation was resorted to. The cephalotryptor (and other means of extracting) was rejected on account of the existing lacerations. The exhausted efforts of nature were trusted to for expulsion. Exhaustion increased, and in another six hours the head had made little progress. The cephalotryptor was now applied; after a time it slipped, and delivery was completed with forceps. After labour her condition did not improve. The temperature rose from 38°·3′ cent., on the first day to 41°·2′ on the ninth day. It maintained this elevation for three days, then fell to 38°·9′. She died on the sixteenth day. The pulse rose with the temperature. On the third day there was vomiting and great abdominal pain. On the sixth day the uterus was felt as high as the navel; a foul, sanious discharge flowed from the vagina. There had already been gangrenescence of the vulva. The post-mortem examination showed adhesions of uterus to intestines and abdominal wall. Near fundus of the uterus was an opening, with thin, discolored edges, and corresponding to it was a small opening in the intestines where this had been bound in contact; so that there was a direct communication between the uterine cavity and the intestinal canal. In the vagina, corresponding to a necrosed point in the ramus of the pubis, was a long deep rent; the fundus of the vagina was perforated. The inner surface of the uterus was covered with a shreddy, blackish, putrescent mass. Near the fundus a portion of the muscular tissue was destroyed. No thrombi were formed. The right synchondrosis was torn open, and in the articulating surfaces was much purulent fluid. On the right ascending pubic ramus was a spot of necrosis, quite denuded of periosteum; opposite this spot was a rent in the vagina. The pelvis is described as belonging to the generally small class, compact; pubic arch, narrow. The conjugate diameter was a little under four inches, the transverse was four and a half inches. The cavity of the pelvis was also somewhat contracted. (The points that attract attention are, 1. The persistent use of the forceps after strong efforts had shown the inefficacy of this instrument to extract; 2. The postponement of perforation *until the child was dead*, notwithstanding progressive exhaustion of the mother; 3. The final resort to the cephalotryptor; 4. The extensive lacerations of the vagina, the denudation of the pubic bone, and the starting of the sacro-iliac synchrondrosis. Timely perforation, the removal of the vault of the cranium, and the use of a properly constructed craniotomy-forceps would have delivered in such a case, with perfect safety to the mother in an hour. It is probable that the putrescence, or gangrene, was simply the result of primary injury, and exhausted reparative power. R. B.)—*Monatsschr. f. Geburtsk.*, Jan. 1866.

Complete Amaurosis for several days' following Eclampsia. By Professor GRENSER.—A primipara was delivered without assistance. Suddenly, a few hours afterwards, an eclamptic fit came on. She was bled to fourteen ounces, calomel, ice applied to the head; other fits followed. The pulse rose during the fit to 100, then fell to 80. After the twentieth fit chloroform was tried, but the restlessness seemed increased. Much albumen in the urine. On the third day, being conscious, she declared she was quite blind. The eyes were void of expression, turned upwards, iris reacting under light. The albumen diminished in quantity. On inspection by ophthalmoscope, under atropin, the veins in the right side seemed fuller, and the arteries more empty. The papillæ nervi optici of greenish tint, and around it on retina were several gray turbid spots. Similar appearances were found in the left eye. The patient was totally insensible even to the flame of a lamp. In about a fortnight sight was restored. —*Monatsschr. f. Geburtsk.*, Jan. 1866.

On Certain Modifications in the management of Turning by the Feet. By Professor MARTIN.—Professor Martin says that he has abandoned the dorsal or supine position of the patient during the operation of turning, and now prefers to place the patient on that side to which the pelvic extremity of the fœtus is directed, whilst he places himself behind the back of the woman, and fixes the fundus uteri with the homonymous hand of the side upon which the woman lies; thus, with the right hand when she lies on the left, and *vice versâ.* Thus we avoid collision with the promontory, and whilst the uterus with its burden sinks down from the pelvis, there is more room for the hand. He advises to seize only one foot.—*Monatsschr. f. Geburtsk.*, Dec. 1865.

Case of Central Laceration of the Perinæum. By Dr. SCHMIDT MÜLLER.—A primipara was in labour, head presenting, when a violent pain drove the head through the centre of the perinæum, notwithstanding powerful support to the part. The trunk of the child and placenta quickly followed. The opening was closed two days later by sutures, and after free suppuration, it healed gradually.—*Bayer., aerztlich. Intell. Blatt.*, 1865.

Case of Forcible Tearing Away of the Uterus of a Woman just delivered.—The following case, related by Dr. Hoffmann, is a melancholy example of the evil resulting from the lack of habitual practice in ordinary midwifery, and the concomitant one of medical practitioners being called in almost exclusively to difficult labours:—A woman, aged thirty-nine, was in her ninth labour, under a midwife who gave a powder, soon after which the child was expelled. She immediately removed the placenta by drawing on the cord. Strong after-pains followed, and the midwife felt a fleshy mass in the vagina. The district surgeon found the patient pale, cold, and almost pulseless, and a dark red fleshy mass projecting from the vulva. As he could not return it, he took it for a growth or fleshy mole, and passed his hand near the mass and through an opening which he took for the os uteri. After twenty minutes' manipulation he directed the midwife to take away the mass. Immediately this was done a loop of intestines appeared. The mass torn away was the uterus. The examination of the body showed that the lower part of the vulva, together with the perinæum as far as the anus, was torn off, and no trace of uterus, ovaries, or Fallopian tubes was found.—*Vjhrschr. f. gericktlich. Med.*, 1865.

A Case of Extra-uterine Gestation. By Dr. G. PHILIPPART.—A woman, aged thirty, who had never borne children, when in her fourth month of pregnancy, experienced pains in the hypogastrium. Towards the end of the seventh month she had pains, as if of labour, and some hæmorrhage. One physician who saw her diagnosed normal gestation and impending labour. But labour did not take place. Still later severe pains set in, and when estimated to be nineteen months it was thought necessary to perform gastrotomy. The child's head was found closely adherent to the containing cyst; it was impossible to extract the fœtus entirely, but several bones were brought away. Soon hectic followed, and the patient seeming to be in great danger, the neck of the fœtus was seized with a hook, and the whole extracted. The patient has recovered.—*Gaz. des Höpitaux*, 1865.

Two Cases of Obstructed Labour through Distension of the Child's Belly. By Dr. A. Voss. (*Mon. f. Geburtsk.*, Jan. 1866.)

Another Case of Complete Inversion of the Uterus after Labour; Death from Hæmorrhage (and Shock). By Professor Lazzati. (*Annali. Univ. di Medicina Milan*, Oct. 1865.)

Physical and Differential Diagnosis of Cystic Disease of the Ovary from Ascites. By Dr. De Cristoforis. (*Amali. Univ. di Med. Milan*, Oct. 1865.)

III. The Puerperal State.

Studies on the Change of Tissue in Labour and Childbed, based on Analysis of Urine of Pregnant, Parturient, and Lying-in Women. By Dr. F. Winckel.—The author gives a history of the literature of his subject, and relates the results of his own examinations of urine. He points out how desirable it would be to institute analysis of the milk and sweat. From forty analyses on four persons, during *pregnancy*, it resulted that the secretion of urine is more copious than in the non-pregnant state, whilst the daily excretion of urea, chloride of sodium, sulphuric acid, and probably also of phosphoric acid, is scarcely so great. During *normal labour* twenty-one analyses on five persons showed that the urine is increased in quantity, the specific gravity diminished, and so also is the secretion of urea, phosphoric and sulphuric acids; on the other hand the chloride of sodium is notably increased. In the second stage of labour the quantity of urine is greater than in the first, and there is more urea, phosphoric and sulphuric acid, and above all, the chloride of sodium is remarkably raised. Lastly, the separation of urea, common salt, phosphoric and sulphuric acids differs according to the hour of the day, showing the same curves as the temperature. As to *preternatural labours*, twenty-five analyses on five women showed that generally, on the access of fever, the urine fell off whilst the urea increased, the common salt was in the inverse ratio to the urea. Whilst the chloride of sodium appeared to be increased in protracted labour, and only fell in high fever, the urea fell off during labour, but rose with the increase of temperature. The action of phosphoric and sulphuric acids corresponded to the excretion of urea. In *healthy puerperæ* forty-eight analyses on five women showed that during the first days the urine was increased in quantity principally within the first twenty-four hours; that the urine was clear, of low specific gravity, generally bright yellow. The absolute excretion of urea, phosphoric and sulphuric acids was somewhat diminished, that of chloride of sodium hardly at all; a gradual decline in the quantity of urine with the progressive involution of the genital organs, a rise in the specific gravity and a return to the ordinary characters. In puerperal diseases 114 analyses on eleven women showed that in light, quite local febrile illnesses, the urine sinks greatly in quantity, but always remains greater than in the non-pregnant. A very marked falling off in quantity generally foretells a severe and long illness. In severe puerperal diseases, especially with exsudations, the usually copious excretion of common salt falls remarkably and rises again on the subsidence of the fever. In severe febrile puerperal diseases the relation of the urea to the sulphuric acid is always remarkably increased.—*Rostock*, 1865.

IV. The Fœtus.

A Case of Lithopædion.—Dr. R. Wagner describes the dissection of a woman aged sixty-eight, who had died suddenly. She had borne five children at twenty-four, and believed herself again pregnant, when she fell sick of typhus. During this illness the movements of the child ceased. Notwithstanding that the child had been retained twenty-nine years in the abdomen, it was entire, although much contracted. It weighed 3½ lbs., and was of the size of a child's head. The soft parts were much dried; some bones showed strong calcification; the scalp and one ear had grown to the membranes. Whether the extra-uterine gestation were primary or secondary, Dr. Wagner does not decide. The woman had rejected an offer of Cæsarean section twenty-nine years before.—*Arch. f. Heilk.*, 1865.

On the Origin of Malformation through External Conditions.—Dr. Engel does not admit that malformation arises from irregularities of the nervous or vascular systems. He looks for the explanation in external conditions. Changes of temperature do not produce monstrosities. But the case is different with moisture of the surrounding air. When the humidity is extremely reduced malformations are often observed in the first days after hatching. He describes his apparatus for producing these malformations in birds.—*Wien. med. Wochenschr.*, 1865.

Anatomical Description of Three very early Double Embryos of Birds, in Illustration of the Origin of Double Monstrosities. By C B. Reichert. (*Archiv. f. Anat. Phys. u. Wissenschaftl. Med.*, 1864..

History of the Birth and Examination of a Fœtus in Fœtu.—By Breslau and Rindfleish. (*Virchow's Archiv.*, 1864.)

On Tetanus Nascentium. By Dr. Lewis Smith.—Dr. Lewis Smith gives an interesting historical summary of tetanus neonatorum, and analyses thirty-two fatal and eight favourable cases collected from various sources.—*Amer. Jour. of Med. Sci.*, Oct. 1865.

On the Influence of Chlorate of Potash on the Fœtus. By Dr. Bruce.—Dr. Bruce brought the subject of the influence of chlorate of potash upon the fœtus before the Edinburgh Obstetrical Society. He related several cases in which abortions had taken place or dead children had been born in previous pregnancies, and in which, under the use of chloride of potash, live children were produced. Drs. Andrew Inglis, Cairns, Moir, and Keiller adduced corroborative testimony in favour of the use of the salt.—*Edinb. Med. Jour.*, Jan. 1866.

V. Miscellaneous.

On Lying-in Hospitals with reference in the Cellular System.—Professor Breslau attempts to remove the evils attendant upon the present system of placing the women in lying-in hospitals in wards, by giving each patient a separate room. He points out the following advantage of the cellular system:—1. Noxious effluvia can only affect the single patient in whose room they arise. 2. Infection by convection (contagion) is avoided, since each patient has to herself all that is necessary. 3. Changes of rooms, occasional disuse, and cleaning are easier. 4. Natural ventilation (Stromeyer's) is more easy in small rooms. 5. Artificial ventilation is more successful. 6. Isolated patients have more quiet. The objections are chiefly the greater cost and the demand for more nurses. The plan is adopted at Kiel. Here one patient and one midwife student sleep in each room. This plan has worked for a year and a half, several hundred labours have taken place, and no puerperal fever has appeared. (This is an approach to the cottage hospital system of Mr. Napper, of Cranley. R.B.)

HALF-YEARLY REPORT ON TOXICOLOGY, FORENSIC MEDICINE, AND HYGIÈNE.

By Benjamin W. Richardson, M.A., M.D., F.R.C.P.

Senior Physician to the Royal Infirmary for Diseases of the Chest.

I. Toxicology.

On Poisoning by Benzine, Nitro-benzine and Aniline.—Chevallier contributes a short and excellent article on poisoning by these agents, and on the injuries sustained by the workmen who make them. *Benzine* has a smell which varies, and which is more or less agreeable. Pure benzine, made from benzoic acid, is not commonly used in the arts; what is commonly used for the manufacture of benzine is the tar derived in the production of coal gas. Benzine, inhaled for a time, induces headache, and a state of uneasiness which gradually disappears on removal from the poison. In some cases the workmen who inhale the vapour experience sweatings and feebleness, which cease only at the end of several days. *Nitro-benzine*, known also by the name of artificial essence of bitter almonds, when made with care, is not attended with many dangers. The chief danger is that of fire from the inflammability of the materials used. The dangers to the workmen arise mainly from the inhalation of acid vapours; and these dangers can be avoided by securing free ventilation, and by the workmen wearing over the mouth and nostrils during the exposure a sponge or linen cloth, charged with a feeble alkaline solution. While the process of manufacture is progressing the escape of hypo-nitric acid, resulting from the action of nitric acid on the benzine, is injurious. The inhalation of this acid first produces coughing; the cough increases, and is followed by vomiting and violent colic. The workman leaves the shop with a sensation of oppression at the chest and panting for breath. Pure air, milk diet, and a day's rest are usually sufficient to remove these symptoms; but, occasionally, other treatment has to be adopted. The operation of washing the nitro-benzine is the most critical, on account of the vapours evolved. Some workmen cannot perform this operation on account of the irritation produced on their respiratory organs. *Aniline*, in its manufacture, leads to many serious accidents which can only be traced to the deleterious action of the aniline itself. In one case a workman employed in superintending the apparatus in which the aniline is made was attacked violently with neuralgia, so that he was forced to leave off his work. During one of these attacks his comrades were obliged to carry him into the open air and to lay him upon the grass, where he remained almost insensible for two or three hours. His face was discolored, his skin was bathed in continual perspiration, and his limbs were attacked with a marked rigidity, followed by extreme relaxation; later, when the man recovered, he experienced chiefly sharp pains and great weakness in his limbs. He was carried to bed and next day was well. Accidents from aniline may and do also occur from the process of rectification of the substance. The foreman of a factory, a strong courageous man, was on several occasions obliged to leave his work in consequence of illness, which kept him in his bed for several days. In other factories violent attacks of neuralgia have been noticed; these attacks begin with a sensation of heaviness in the head, and giddiness almost amounting to syncope. In one shop the workman who had charge at night was found in the morning lying on the ground several yards from the building, whither, he said, he had dragged himself in order to be in the air and get rid of a giddiness he felt an hour before, and which he attributed to his work. These symptoms correspond with those specified in two cases recorded by Messrs. Knaggs and Mackenzie. In conclusion M. Chevallier describes the conditions required from those persons who establish manufactories for benzine, nitro-benzine, and aniline. He places these recommendations as follows:

1. The manufactories should be at a distance from dwelling-houses.

2. The washing rooms should be within the building where the operations of nitrification, decantation, and rectification have to be performed.

3. The building where these different operations are performed should be open on one side and completely closed on the three other sides by walls six yards and a half high; there should be no opening on the roof. The building should be nearly ten feet in height under the roof; the walls should be constructed of very hard rough stone, or of bricks joined by cement, but not coated; the roof should be of iron, supported only by the walls or by iron columns, wood being excluded; the covering should be of galvanised or tarred sheet-iron. The apparatus should stand on masonry, constructed like the walls,

and water taps should be placed in the workshops for the washings and to be ready in case of fire.

4. If, in spite of the arrangements made, vapours escape which are hurtful either to the workmen or to the neighbours, and the fact be duly established, the administration should have the power of enforcing that these vapours be carried away through cast-iron tubes to the chimney of the steam engine, or that a brick chimney thirty-three yards high should be constructed.

5. The manufacturer should be careful in selecting his workmen; and he should recommend those whom he employs to take certain indispensable precautions while mixing nitric and sulphuric acids.

6. Those workmen who have not to breathe the acid vapours which are disengaged should be so placed in a well ventilated situation so that the wind may convey the vapours in a direction from them. In mixing the acids, in decanting, and in washing, the workmen should be required to cover the mouth and nose with a sponge dipped in slightly alkaline water.

7. If any accidents happen to the workmen employed on nitrobenzine and aniline they should be made immediately to cease all work, and the sick workman should be compelled to rest and should only be allowed to resume his occupation gradually, the effects produced being carefully watched. If the symptoms return the work of the man should be changed, or he should be obliged to leave the factory and follow another occupation.

8. The liquids resulting from these manufactories should not be introduced into the soil; they should be directed into drains, or into a water-course where the impurity is of no moment.

9. There ought always to be sand in the workshops; it may be used with greater advantage than water for extinguishing hydrocarbonaceous liquids when they take fire.—*Annales d'Hygiène Publique*, October 1865.

Case of Poisoning by Narcotic Vapours in a Disused Tar-boiler.—Mr. Nowell supplies the notes of a case of singular interest, under the care of Dr. Gull, at Guy's Hospital:

T. L—, aged forty-two, a labourer, was admitted Feb. 20th, 1866. He was employed at a gas-tar distillery, where pitch is made. One of the large boilers or stills, which are eight feet deep, and connected to one another by short pipes, capable of being opened or shut off by taps, was undergoing repair, and had been empty for more than a week. The engineer had been down on the 16th ult., without suffering any inconvenience from the gases generated in the adjoining still, and had gone down again on this day at eight a.m. to repair the damage. Whilst thus engaged he called out, and the present patient going to see what was wanted observed him reclining on his arm, like one dead or asleep. Whilst going down a ladder to his assistance, he in his turn suddenly called out for some one to steady the ladder, and would have fallen but for help. He was dragged out insensible, and continued so for more than an hour. Then he became quite ungovernable, and endeavoured to bite and strike anyone who came within his reach, requiring three or four men to hold him. He made a great outcry, and from the description given would appear to have resembled a person affected with epileptic mania. Three hours afterwards he was admitted into the hospital, and the following note was then taken of his condition: "He will be quite still if not disturbed; but when moved or touched cries out, and requires to be restrained. He is not conscious of surrounding objects; his pupils are dilated; common sensation is not impaired. The breath is rather cool, skin moist, and he is perspiring freely; legs and feet cool; shivers now and again. Respirations easy, not stertorous, 20 to the minute; pulse 50, regular, very weak. Half an hour afterwards he could swallow fluids freely; his eyelids were firmly closed, and sensibility to cold is noted as being increased. He endeavours to wrap the clothes more closely around him. He will not answer when spoken to; pulse 64; respirations 20, quiet. In half an hour's time he was conscious of surrounding objects, looking wildly and astonished; could talk distinctly and rationally; pulse 72, rather full; respirations 24; skin moist. His urine contained a trace of sugar. The man told the reporter on the following day that he had no recollection of anything which occurred after his descent into the boiler until he found himself in bed next morning."—*Lancet*, March 10th, 1866.

[From the suddenness of the insensibility in this case, the character of the sleep, the dilatation of the pupils, the epileptiform paroxysms, the comparative quiet of the pulse and respiration, and from the fact that the urine contained sugar, we infer that this was a case of poisoning from the inhalation of carbonic oxide gas. The symptoms are identical with those we have observed in inferior animals after their exposure to carbonic oxide. B.W.R.]

Case of Poisoning by Essential Oil of Bitter Almonds.—Dr. Reginald Thompson supplies the notes of the following rare and very interesting case:—On the 3rd January a man was brought into St. George's Hospital, under the care of Dr. Barclay, with the symptoms of poisoning by oil of bitter almonds. He was a barber, aged fifty-seven, and had taken (it was believed purposely) two drachms of the oil, which was used in the trade for scenting pomatum. The oil was taken at twenty minutes past ten a.m., one hour after breakfast. Warm water and tartar emetic were at once administered by those in the house, without, however, vomiting being induced. He was brought into the surgery at twenty minutes to eleven. He was then speechless. The smell of the oil was very perceptible in his breath; no pulse could be felt at the wrist; the breathing was hurried; the pupils were dilated; the extremities cold and clammy, and there was a general purple hue on the face and other parts of the body; the tongue was furred; the fauces and uvula œdematous and congested. At a quarter to eleven, half an ounce of carbonate of magnesia was administered, followed by a draught containing the perchloride of iron and sulphuric ether, of each one drachm. At five minutes to eleven the stomach-pump was introduced, and the contents of the stomach well washed out. The smell of almond oil was very strong, pervading the room and the passage. There was not much food in the stomach. A very marked rally followed this operation. The pulse returned to the wrist, and beat with fair strength, 88 in the minute; the pupils became smaller, and the extremities warmer, and he recovered sufficiently to move and put himself to rights. He was then carried up stairs, and was put to bed. He was apparently doing well, when at half-past eleven he was seized with sudden vomiting; the face became purple and full, the breathing laboured, and a fit was evidently coming on. The pulse was beating 118 in the minute; the pupils became small, the jaws closed, and he became quite insensible; the breathing became spasmodic, and the motions were passed involuntarily; the respirations fell to 20, and the pulse to 76. There was blowing action of the right buccinator, and diminished sensibility of the right eye. Heart's sounds were healthy. At forty minutes past eleven cold douche was freely applied to the nape and spine, and the face slapped with a wet towel, which temporarily roused him. Two ounces of brandy and some beef-tea were injected into the rectum, but were returned immediately. At half-past twelve the heart was still beating; he was quite insensible, pale and blue. Temperature in the axilla 91°·6′; pulse 68; respirations 12. The pulse became gradually weaker, and though galvanism was applied vigorously and steadily, no effect was produced, and he died at twenty minutes past one, the lungs acting once or twice after the heart had ceased to beat.

The examination of the body was made by Dr. Dickinson, and the following is a copy of his notes:—The body was examined twenty-four hours and a half after death. It was in good condition; the rigor mortis being well marked. On cutting into the scalp a quantity of dark fluid blood run out, the bones of the skull were discoloured, and a good deal of fluid blood ran out of the cerebral vessels when the brain was sliced. The ventricles were nearly empty and natural. There was a decided smell about the brain, which was suggestive of prussic acid, but was less distinctly recognised than what was emitted by the stomach. A very little old tubercle, scarcely visible, was found at the apex of each lung. Both lungs were congested behind and full of frothy fluid. There was a little atheroma of the aorta, and some thickening of the mitral valve. The left ventricle was quite closely contracted; the right was uncontracted, and contained decolorised coagulum. The stomach contained a good deal of blood-tinged mucus. The mucous membrane of the great curvature was congested, and of a bright pink colour. There was a decided smell of bitter almonds in it. The intestines were natural externally. The kidneys, liver, and all the other abdominal viscera were examined and found natural. The fauces were œdematous. There was simple effusion of serum under the mucous membrane in the fauces and upper part of the larynx. Excepting the clot mentioned in the heart the blood was fluid.—*Ibid.*

Poisoning by Petroleum.—Dr. Mayer, of Antwerp, relates a case of poisoning by a glass of petroleum, which was drunk in the dark by mistake for a glass of beer. About half an hour afterwards Dr. Mayer found the patient pale, restless, with sunken eyes, strongly contracted pupils, the irides insensible to light, hot dry skin, weak voice, and short quickened respiration. The pulse was excited, hard, incompressible, 60 in the minute, and afterwards fell to 48. (The ordinary number of pulsations in health was 80.) The patient complained of an indescribable feeling of *malaise*, great anxiety, and a sensation of constriction at the diaphragm and in the pharynx. Consciousness was undisturbed. The breath had no odour of petroleum. In spite of the administration of tartar emetic and of tickling the fauces, no vomiting occurred; neither fæces nor urine were discharged. After the epigastrium had been rubbed with a flannel dipped in eau de Cologne the patient had eructations, smelling strongly of petroleum, and afterwards vomited copiously undigested food, having the same odour, immediately after which an improvement took place; the pulse became quicker, the skin warmer, the pupils dilated, and the sensibility of the iris to light returned gradually. The vomiting recurred frequently, and always with an odour of petroleum; the patient had a copious stool, slept well through the night, and was quite well the next day. The urine, throughout the whole of the next day, still had a marked odour of violets.

Dr. Mayer remarks that the petroleum appears to have produced total paralysis of the stomach and alimentary canal, so that emetics had no effect, and the food, which was vomited four hours after the poison was taken, showed no traces of digestion. It was only through the stimulation produced by rubbing the epigastrium that vomiting was excited. The retardation of the pulse Dr. Mayer ascribes to the anæsthetic action of the petroleum, which action also shows itself locally in the anæsthesia and paralysis of the digestive canal. The recovery of the patient from so large a dose of the poison may in great measure be attributed to the circumstance that he had previously had a full meal, which thus prevented the petroleum from exerting all its action on the stomach.—*Journal de Bruxelles*, May, 1865.

A case of poisoning by petroleum is recorded also in the *Journal de Chimie Méd.* The patient drank a small glassful of petroleum in a cup of coffee. The symptoms and result are not given. Cases of poisoning by petroleum have also been recorded by Weinberger ('Schmidt's Jahrb.,' cxxi, 34) and by Clemens (ibid., cxxv, 294).—*Schmidt's Jahrbücher*, Band cxxvii, 1865, No. 9.

Poisoning by Tobacco-juice.—M. A. Marchant relates the following case:—A smoker, in drawing air strongly through an obstructed pipe, in order to make it more permeable, took into his mouth and involuntarily swallowed a dislodged plug of inspissated tobacco-juice. In a short time his head became heavy, his thoughts confused, his speech indistinct; he had noises in the ears, a disagreeable feeling at the epigastrium, and dryness of the throat. Believing that the open air would remove these feelings, the patient went out; but the headache and giddiness increased, and the patient at last fell down insensible, in which condition he was after some time found by a passenger and carried into his house. Copious and repeated vomiting then set in; consciousness returned; but the patient fell into a restless, somnolent state. He had severe headache, *malaise*, and faintness, during the whole of the next day. The spontaneous recovery may be attributed either to the small amount of nicotine contained in the plug, or to the imperfect absorption of the poison contained in the hardened plug.—*Journal de Bruxelles*, and *Schmidt's Jahrbücher*, Band cxxvii, 1865, No. 9.

Beer containing Lead and Copper.—Dr. Verver found that in many of the beerhouses in Maestricht the beer contained lead and copper. In those houses which were most frequented this was only the case in the morning; it was no longer so when a large quantity had been drawn. The origin of the poison is, beyond doubt, due to the use of copper taps and leaden tubes, connecting the vessels with the pump on the buffet. A number of cases of lead colic had occurred in Maestricht; especially among the factory workmen, who were in the habit of drinking a glass of beer early in the morning.—*Schat der gezondheid*, 1864; and *Nederlandsch Archief voor Genees. en Natuurkunde*, Part I.

Cases of Poisoning from eating the Roots of the Œnanthe crocata.—Dr. Popham describes the following cases:—On April 15th, 1865, five boys were brought to the Cork North Infirmary at three o'clock p.m. with symptoms of poisoning by *Œnanthe crocata*. They saw the plant growing on the banks of the stream, and, mistaking it for field-carrots, they all began to eat it with avidity. The effect of the poison was soon apparent. They felt a burning in the stomach and constriction of the throat, with nausea and headache, and one of the party fell down on the bank in strong convulsions. Terrified by this, the others left him, in order to procure assistance; but when help arrived he was found lying on his face in the stream, quite dead. On being brought to the infirmary, between one and two hours after the occurrence, four out of the five were relieved from the severe symptoms of the poison by emetics and other remedies, but its sequelæ, such as colic pains in the abdomen, loss of animal heat, giddiness, and depression of spirits remained until the following day. The fifth boy, named Mulcahy, was long in a very precarious state, passing in the interval before his admission through alternations of tetanic convulsions and insensibility, with loss of speech. An emetic of sulphate of zinc was given him, and succeeded in bringing up a piece of the root; the effect was kept up by draughts of mustard and water, which produced a salutary irritation of the tongue and pharynx, rousing him from the lethargic state, as he struggled violently against their administration. His symptoms were very critical: face flushed up during his struggles, livid when quiet; pupils dilated and insensible; breathing slow and laboured, interrupted by constant sighing and convulsive cough; pulse 84, feeble and irregular; both the heart's sounds distinctly audible. In order to test the loss of speech, as all our efforts were unsuccessful, I got his mother to speak to him, but for some time without effect, till at last a dim, hazy perception of her familiar voice began to dawn upon his mind, and with a spasmodic exertion he jerked out the word "mamma." After a course of similar entreaties and shakings up she got him to put out his tongue, but in a very hesitating and tremulous fashion. Considerable hyperæsthesia existed in the soles of the feet, the slightest tickling sufficed to rouse him from stupor, and accordingly, when his somnolence waxed very profound, we availed ourselves of it as a therapeutic agent, and he would invariably withdraw his feet with a growl of impatience. When placed in the sitting posture his head used to fall forwards or backwards, or to the shoulder, as if the co-ordinating power of the muscles was suspended, or the polar force exhausted by the previous discharge on them of the spinal dynamic matter; but when replaced on the pillow he tossed his head from side to side, accompanied by jactitation of the hands. As the emetics ceased to act the stomach-pump was used by Dr. O'Sullivan, house-surgeon, and warm water was thrown in, with the effect of bringing off some imperfectly masticated flakes of the root. Strong coffee was given him, which he took willingly, stopping after each mouthful for a second or two. Other remedies, such as sinapisms to the spine and abdomen, cold affusion to the head, friction and warmth to feet, and stimulants, were used. Roseola was noticed on the abdomen in patches, such as Devergie describes as being occasionally observed in like cases.

Seven o'clock p.m.—He is in a deep sleep, snoring loudly and moaning, the eyelids spasmodically closed. When slapped on the cheek by the house-surgeon he bounds up indignantly, stammering out in pitiful remonstrance, "Wisha, don't, then," and covers his head impatiently; he is immediately buried in unconsciousness again. No urine passed.

April 16th.—More conscious, but still much confused in intellect and vacant in expression; speech returning, but he takes time to answer a question; pupils less dilated; tongue sore and swollen; pulse 84, rising to 108 on sitting up.

17th.—Consciousness has quite returned, but all is a blank since he ate the root up to this morning; temper irritable; tongue raw at tip and edges. He says that the quantity of the root which he swallowed was about the top joint of the little finger; the piece thrown up by the zinc emetic was about half that size. He left hospital next day, quite recovered.

Remarks.—Dr. Popham was able to identify the plant by comparing the leaves and root with the excellent figure in Sowerby's 'Botany' (No. 3713). The symptoms just described show the double property which this poison possesses as an acro-narcotic, first of causing local irritation of the mucous membrane in the primæ viæ and the follicles of the stomach; and, secondly, of violently disturbing the equilibrium of the muscular tissues, producing at intervals a powerful discharge on them of polar force, to be succeeded by great exhaustion; along with this irritation of the spinal cord there occurs cerebral congestion, torpor of the organs of sense, and stupefaction of the intellectual faculties; it seems to terminate life by one of two ways—one by asphyxia, from its tetanic action upon the muscles of respiration, and, perhaps, the heart; the other and more general way by coma, resembling the mode of death caused by opium in some respects, but differing in the dilated state of the pupil. Its effect upon the fibrin of the blood, in destroying its coagulating power, is common to it with other poisons.

Aphasia.—A symptom occurred in this case, namely, loss of speech, which has lately excited much attention. For some time after the occurrence his utterance was totally abolished, and but slowly returned. The tongue was certainly injured by the teeth during the spasms, but this would produce indistinctness, and not extinction, of speech; besides, he had the power of uttering sounds or outcries when stirred up actively. There is little doubt that this poison has the property of paralysing the muscles of the tongue, producing that form of impeded movements of this organ as connected with vocal sounds which Romberg calls glossoplegia articulata. The other form of paralysis of the tongue, Romberg's glossoplegia masticatoria, also existed in this case; he was able to swallow, but not by a continuous effort, but more *per saltum*, and by small portions at a time. This paralysis passed off sooner than the other.—*The Dublin Quarterly Journal of Medical Science*, November, 1865.

Poisoning by Water-hemlock.—Dr. Lender relates a case in which three boys ate of the roots of water-hemlock. In one vomiting set in, by which further symptoms were prevented. The second, who had eaten but little, vomited after some time, and became faint and unconscious, but had no very severe symptoms. The third, who had eaten most, vomited in about an hour; he became insensible and convulsed generally; his respiration became stertorous, and water, tinged with blood, flowed involuntarily from his mouth. Death followed, about three hours after the first appearance of the symptoms. At the *post-mortem* examination the vessels of the brain were found to be remarkably full of blood; there was about an ounce and a half of bloody serum in the pleuræ, and a singular effusion in the pericardium. The costal pleura was injected of a bright red colour; the lungs were hyperæmic, and the trachea and bronchial tubes were injected, of a bright red, and contained reddish mucus. The mucous membrane of the stomach and duodenum was of a dirty dark-red colour, but with spots of injection; the liver, spleen, kidneys and pancreas, were hyperæmic.—*Viertel-jahrschr. für Gericht. Med.*, 1865, and *Schmidt's Jahrb.*, vol. cxxvii, 1865, No. 9.

PART II.—HYGIENE.

Prison Dietary in India.—Mr. W. R. Cornish, Assistant-Secretary to the Principal Inspector-General of the Medical Department of the Madras Presidency, has a most able article on the prison dietaries of India. He shows that a sweeping reform is immediately needed in respect to these dietaries, and that, in fact, nearly every acknowledged rule of science is at this moment scientifically broken. The dietaries are defective in arrangement, and they have been for the most part arbitrarily fixed, and without regard to the scientific data we already possess in reference to the relative values of food. For instance, with Mr. Mayer's analyses of the Indian cereals we are forced to admit that sixty-five rupees' weight of rice does not contain anything like the same amount of nitrogenous principle which is found in sixty rupees' weight of raggy or cholum; and yet in the majority of the Madras gaols these quantities are considered as equivalents or substitutes one for the other. The injudicious dietaries of gaols have a great deal to do with the excessive mortality of prisoners, though in Madras, at least, there are other causes which influence their health, especially overcrowding, and deficient ventilation of most of the gaol buildings.

The results of improving the diet of the prisoners in the Bombay House of Correction were to reduce the proportion of sickness and mortality according to the following:

	Admissions per 1000.	Deaths per 1000 strength.
Under the old and deficient diet for 10 years	1761	64·5
Under the new and improved diet for 4 years	818	11·14

Mr. Cornish submits that in the reforms of the prison dietaries of India, in the Madras Presidency, the subjoined points should be remembered:

1. That the proportions of nitrogenous to carboniferous material in the food should be as nearly as possible as one of the former to three of the latter.
2. That variety of food is necessary.
3. That all dietaries should contain a proportion of animal and of vegetable food, and that money allowances for condiments at present given in the Madras gaols should be prohibited.
4. That the mechanical preparation of the food should be more carefully attended to when the chief food of the gaol is made up of grain. The grains should be reduced to a state of ultimate division.
5. That with a due admixture of the cereal grains, dholl, fresh vegetables, and animal food, it is possible to maintain the standard of health amongst prisoners, provided that they are not overcrowded, and that the sanitary condition of the gaol be unexceptionable.

Mr. Cornish holds that rice is not essential in gaol diets, and that the quantity of vegetables should be fixed, and that three quarters to one ounce of salt a day is necessary with a grain diet. He adds a dietary scale for gaols where dry grain is the staple adapted, specially to the wants of prisoners subjected to hard labour.—*The Madras Quarterly Journal of Medical Science*, No. 15.

Sanitary Measures and their Results in Croydon.—Dr. Westall, in a very able work on the advantages to be derived from the adoption of the Local Government Act, as exemplified in Croydon, gives a report of the good results following upon sanitary measures, which is most encouraging. The result of the modern improvements in Croydon is a large decrease of sickness, especially amongst the poorer classes; a very large increase of population, the birth-rate increasing from 2·91 to 3·14 per cent., and the deaths decreasing from 2·366 to 1·845 in the thousand, showing a saving of above 200 lives per annum. Excepting in extreme infancy, the average deaths at all ages have materially decreased; and, even when the infant deaths are taken at the old rate, all those under twenty years of age have decreased by nearly 10 per cent. Taking the deaths from fever and all zymotic diseases alone, the decrease is respectively from 6·1 per cent. to 3·1 per cent., and from 22·5 per cent. to 17·2 per cent., taking from the year 1845, and including the two unhealthy seasons of the years 1863-64.

These, then, are some of the results of the application of the Local Government Act (patent to all) to Croydon, a town in many respects not most advantageously situated, by reason of its proximity to London, of which, in fact, it is a suburb; and by its being the chief town of a large agricultural district, the majority of the lower classes, male and female, obtaining their livelihood by out-of-door work, and thus, perhaps, greatly accounting for the large infant mortality; also by the daily passing through of a large number of tramps; and again, as containing the union-house for nine parishes and two hamlets, for which no deduction has been made in the calculations. That there are results far higher than these, of a religious and moral character, we may be assured; and the Croydon ratepayers may well be satisfied that they have not exercised a large amount of self-denial and perseverance in vain, but that they have added somewhat to the improvement of their fellow-men, and thus, humbly though it be, joined in the tribute of glory and praise to Him who gave us so great an example of beneficence, and have endeavoured to fulfil their highest duty on earth.

III. Summary.

The following summary refers to papers which, from want of space, cannot be quoted at length in this report:

1. *On Sunstroke, as it occurred in the Army of the Potomac.* By Charles Smart, M.D. (*American Journal of the Medical Sciences*, April, 1865.)—This is a very thoughtful and suggestive paper. The author considers that sunstroke is caused by loss of water from the body by the skin and lungs, and he recommends the free administration of water as the best treatment. In one of his cases, where the patient seemed nearly lifeless, recovery followed the free use of water externally applied to the body, with copious drinks of water afterwards.

2. *Poisoning with Sulphates of Zinc and Iron.* By W. Herapath, sen., F.C.S. (*Pharmaceutical Journal*, July, 1865.)—In this note Mr. Herapath puts the question: can the sulphates of zinc and iron be resorted to to effect slow poisoning? The inference drawn from a case related by the author tends to an affirmative answer of the question raised.

3. *Poisoning by Veratrum Viride.* By J. C. Harris, M.D. (*Boston Medical and Surgical Journal*, April, 1865.)—In 1861, as many of our readers will remember, Dr. Cutler, an American physician, brought before the notice of the profession in this country his experience as to the medicinal value of veratrum viride. The case related by Dr. Harris is important, as showing the toxicological effect of this substance. The patient, a child, had been successfully treated for pneumonia with four minim doses of the tincture of veratrum. After its recovery its mother, by mistake, gave it a large dose, equivalent,

probably, to thirty-five drops. The poisonous symptoms were retching, pallor, stertorous breathing, pulse 40, cold extremities, and cold profuse perspiration over the whole body. The child died in thirteen hours.

4. *Aniline und Anilinfarbers.* By Professor Sonnenkalb. Leipsic, 1864. (Volume.)—The author discusses the poisonous properties of benzine, nitro-benzine, and aniline.

5. *Modern Improvements in the Homes of the People.* By Thomas Beggs, F.S.S. (*Social Science Review*, May, 1865.)—The plan proposed is to form villages in the neighbourhood of the metropolis sufficiently distant to ensure a pure atmosphere and healthy soil. The dwellings may be erected at a moderate price, yet so connected with the metropolis by distinct lines of railway and stations to each, as to leave no portion of London more than one mile distant from some one station. It is proposed to erect ten cottages to an acre.

6. *The Microscope in Toxicology.* By A. Helwig, M.D. Volumes II, with Photographic Plates. 8vo. Mayence, 1864.—This is the first attempt to systematise the study of the microscope for toxicological inquiries. The author does not attempt to supplant chemistry, nor to interfere in any way with chemical investigation, but to supplement chemical labour. Facts are supplied on nearly every known poison, and it is shown that in many cases the microscopic test is the best.

7. *Artificial Insanity.* By Daniel H. Tuke, M.D. (*The Journal of Mental Science*, April, 1865.)—Deals with the question of hypnotism and kindred topics. A very useful paper for the legal psychologist.

8. *Question Médico-Légale de la Pendaison.* By Ambrose Tardieu, M.D. (*Annales d'Hygiène Publique*, April, 1865.)—The distinguished medical jurist, the author of this essay, discusses the distinction between hanging as the act of the suicide or of the homicide. The author destroys many of the views which have been considered up to the present time as beyond dispute, and while very precise in his details, leaves the question of distinction as one rather more difficult to settle than has generally been surmised.

9. *De l'Empoisonnement par la Strychnine.* By T. Gallard, M.D. (Ibid., April and July.)—This is the most comprehensive work that has been. written on the subject of which it treats. There is no point connected either with strychnine poison, whether in relation to symptoms, mode of action of the substance, chemical detection, or pathology, that is omitted. There is also an excellent chapter on the remedies for strychnine when it has been used as a poison. There is not much of original matter in the communication, and the final passages, "conclusions," are, perhaps, too dogmatic, especially in regard to the absolute diagnosis of the poisoning, and to the negative influence of all remedies. At the same time, whoever would be quite conversant with the latest information on this topic should consult M. Gallard.

10. *Alleged Poisoning by Atropine.* (*Pharmaceutical Journal*, Sept.

1st, 1865.)—In this article, based on a case of suspected poisoning by a medical man named Sprague, a question, raised by Dr. Ogle, of Clarges Street, is considered. The poisoning occurred to several persons after they had fed on a rabbit-pie, and Mr. Herapath asserted the presence of atropine in the said pie. Dr. Ogle's suggestion on the case is that the symptoms observed were due to atropine, but the poison was not feloniously induced. He maintained that rabbits can eat belladonna without being injured by it, but that thus fed their flesh may prove poisonous to persons who partake of it.

11. *A new Method of Artificial Respiration.* By Dr. De Chilly. (*Journal of Practical Medicine and Surgery*, August, 1865.)—Dr. De Chilly's plan of artificial respiration consists in seizing the short ribs of the person asphyxiated, and in working the thorax as though it were a bellows, of which the ribs represent the alæ. In a case of drowning he obtained, he says, unhoped-for results by this plan.

12. *On Albuminuria in Poisoning by Lead.* By M. Ollivier. (*Ibid.*, March, 1865.)—M. Ollivier placed animals under circumstances in which they were obliged to absorb lead. He found an albuminous condition of the urine, a constant symptom so long as lead was passing by the kidney, but the symptom was transitory in all cases except where the saturnine poisoning was so prolonged that chronic nephritis was established.

13. *Reports on the Sanitary Conditions of Railway Stations in Madras.* (*Madras Quarterly Journal of Medical Science*, No. 15.)—These reports were drawn up after careful inspection, and were presented to the Governor of the Madras Presidency. They show, that in the erection of railway stations in India, the sanitary requirements are the last subjects to be considered.

QUARTERLY REPORT ON SURGERY.

By John Chatto, Esq., M.R.C.S.E.

On the Putrid Infection complicating certain Simple Fractures of the Jaw.—In a communication to the Paris Surgical Society M. Richet drew attention to this subject, which he believed has been overlooked by all writers on surgery. Its purport is summed up in his conclusions:—1. Fracture of the lower jaw, when the alveolar-gingival periosteum has been lacerated, and there is also a displacement of the fragments, should not be regarded as a simple but as a compound fracture, since the seat of fracture communicates with the cavity of the mouth, that is to say, both with the external air and the salivary fluids. 2. Besides purulent secretion at the seat of fracture, and the various complications of neighbouring abscesses, osteitis, necrosis, or delay in reunion, observed and described by authors (but which I believe to be of much more frequent occurrence than is generally admitted), there are other accidents of a general character which may become very serious and even fatal. 3. These general accidents, characterised by scarcely perceptible and irregular shivering, putridity of the breath, diarrhœa, vomiting, &c., when they terminate fatally do not leave any traces discoverable after death. 4. These general symptoms cannot be referred either to purulent infection, properly so called, or to typhoid fever. They are due to a kind of septicæmia or putrid intoxication, which I think should be termed acute to distinguish it from what was formerly called hectic fever.

The best means of preventing this very serious complication is the maintenance of the fragments in a state of complete immovability after the reduction of the fracture; and this may be best done by means of ligatures applied to the teeth. Even when the symptoms are very far advanced they may be arrested by this means, as proved by a case related in the paper. M. Dolbeau also informed its author that he had seen very formidable symptoms of putrid infection disappear in two cases after incisions had been made in a dependent position. At a subsequent meeting of the society, M. Chassaignac pointed out that he had in his work on suppuration fully described this serious complication of fracture of the jaw, and its treatment by means of drainage tubes. Nevertheless, it is certain that the occurrence is far from being generally recognised; fractures of the lower jaw being usually regarded as of little consequence, and certain to do well. Yet, of 27 cases, collected by Malgaigne from the registers of the Hotel Dieu, 4 proved fatal; and of 10 cases observed by M. Richet, 2 were followed by death by reason of this putrid intoxication.—*Gazette des Hôpitaux*, 1865, Nos. 117 and 124.

Statistics of Hernia.—This is an account by Dr. Scholz of the cases of hernia which have come under the care of Professor Lorinser during the last twenty years at the Vienna General Hospital. They are not very numerous; but having been minutely analysed, they yield some deductions worthy of record.

The entire number of cases admitted during 1844—1863 was 422, viz., 220 males and 202 females; 268 (215 males, 53 females) being examples of inguinal; 142 (4 males, 138 females) of femoral; and 11 (1 male and 10 females) of umbilical hernia. There was also 1 case of hernia of the foramen ovale in a female. The right side was affected in 246 cases (163 inguinal, 83 femoral), and the left in 156 (97 inguinal, 59 femoral). With respect to age, it was found that the number of cases was twice as large during the decennium 20 to 30 as during that of 10 to 20. During the three decennia 20 to 50 it continued nearly the same; reached its maximum during that of 50 to 60, and then gradually diminished. Before the age of 40, the males were in largest proportion (113 to 46); but after that age the proportions were reversed, there being 156 females to 107 males. Of the 422 cases of hernia, 247 (109 males, 138 females) were treated for strangulation; 192 (77·4 per cent.) recovering, and 55 (22·6 per cent.) dying. In 12 of the cases the treatment was by position (*lagerung*), in 119 by the taxis, and in 116 (60 recoveries, 56 deaths) by operation—39 cases having been also brought in in too advanced a state to admit of the operation being performed. In 104 cases of inguinal hernia in males there were 37 operations, in 32 cases in females 10 operations; and in 4 cases of femoral hernia in males there was 1 operation, in 95 cases in females, 65 operations. The cases were fatal after operation at the rate of 40 per cent. for femoral hernia in females, 51 per cent. for inguinal hernia in males, and 70 per cent. for inguinal hernia in females. The duration of the strangulation averaged for the 247 cases 2·1 days per case, *i. e.* 1·88 days for males, 2·27 days for females. In inguinal, the duration was 1·79 days (1·87 males, 1·5 females), and in femoral, 2·50 days (2·0 males, 2·5 females). Comparing the successful with the fatal cases, in general it was found that the duration in the former was 1·7 day, in the latter, 3·1 days. In those who were not operated upon, the duration was 1·3 day compared to that of 2·8 days in those who were operated upon. In recoveries after operation, the strangulation had lasted 2·5 days, in fatal cases 3·1 days. Of the 116 cases operated upon, the contents of the sac was intestine alone in 90 (49 recoveries, 41 deaths), omentum alone in 2 cases, both recovering, and omentum with intestine in 24 (9 recoveries, 15 deaths). In males, intestine alone was found in 26 cases, or 29 per cent.; in females, in 64 cases, or 61 per cent. In inguinal hernia intestine alone occurred twice as frequently in males as omentum and intestine, and exclusively in females; while in femoral hernia in females it occurred 4 times as frequently as omentum and intestine. In cases of intestine alone in inguinal hernia, 42 per cent. of the males, and 70 per cent. of the females died; and in femoral hernia, 45 per cent. of the females; while, when omentum and intestine were present, the deaths of the males were 66 per cent. in inguinal hernia, and of the females, 50 per cent. in femoral hernia. As to the issue of the 116 operations, there were 56 recoveries, and 60 deaths. Of the 37 males affected with inguinal hernia, 18 recovered, and 19 died; and of the 10 females, 3 recovered and 7 died. Of the 66 females, the subjects of femoral hernia, 39 recovered, and 26 died, the only male dying. Thus, 19 per cent. more females than males operated upon for inguinal hernia died, and about 11 per cent. more male subjects of inguinal hernia died than did female subjects of femoral hernia. As to the ages of the persons operated upon, 29 were under forty (14 recovering and 15 dying), 53 between forty and sixty (32 recovering, 21 dying), and 34 above sixty (14 recovering and 20 dying). The immediate cause of death was peritonitis in 26 cases, and intestinal gangrene in 17.—*Wien Medicinische Wochenschrift*, Nos. 48 and 78.

On Inflammatory Cutaneous Papilloma. By Professor ROSER.—In this paper Professor Roser calls attention to a growth of the papillæ of the skin, accompanied by the formation of intrapillary abscesses, and the conjunction of the swollen ends of the papillæ. It may be mistaken for cancer, condyloma, lupus, elephantiasic growths, &c., but it consists really in a papillomary formation, essentially inflammatory in character, and independent of any specific cause. Circumscribed inflammations of the skin, especially of the face, but also of the hand and foot, are sometimes observed to give rise to a remarkable growth and enlargement of the papillæ; and persons unacquainted with the affection, and who only see it for the first time on the face, when it has reached its height, may readily mistake it for cancer. Examining the case more closely, the enlargement of the papillæ becomes obvious, while small abscesses are found between the bases of these, and a probe can be passed behind their points, which are grown or fused together. The skin may seem perforated in several places, as in carbuncle, but this really arises from this junction of the ends of the papillæ, giving rise to little canalicules and "papillary bridges." The abscesses of the sudoriparous glands, described by Verneuil, can scarcely be confounded with this appearance, although they bear some resemblance to it, for they are confined to the mammæ, axilla, and anus.

The papilloma may assume an acute or subacute form, and may pass into the eczematous or carbuncular form of skin disease, especially in the regions of the chin or cheek. Accordingly as the papillæ themselves, or the intrapillary spaces, are the chief seat of the phlogosis we have enlargement of the papillæ or intrapillary abscesses; and when the inflammation penetrates somewhat deeper, and gives rise to fibrinous exudation, an appearance much resembling superficial carbuncle is produced. In carbuncle, however, a destruction of the skin itself takes place, while in papilloma at most the papillæ are destroyed. The affection bears considerable resemblance to *lupus anatomicus*, inasmuch as in both there are intrapillary abscess; but lupus is serpiginous in its progress, and most obstinate in its resistance to remedies—neither of which characters attach to papilloma. In fact, for the latter any active treatment is only exceptionally necessary, as, after a few weeks waiting, and under the use of such simple applications, as lead lotion or white precipitate ointment, the papillæ will atrophy, and the skin resume its former condition—just as is the case with flat condylomata. In recent cases, neither the knife or caustics are required; but in some very old cases, just as in large condylomatous growths, the use of the scissors cannot be dispensed with. An interesting case of the latter description, the instep being the part implicated, is related by Professor Roser.—*Archiv der Heilkunde*, 1866, No. 1.

On Gun-shot Fractures of the Thigh. By Dr. Post.—This is an interesting paper read at the New York Academy and founded upon the consideration of forty-eight cases, a report concerning the treatment of which was prepared for the use of the Sanitary Commission. With respect to the question of amputation, Dr. Post believes that the following rules may be laid down:

"1. When an army is engaged in active military operations in an enemy's country, and at a great distance from its base, or when it has cut loose altogether from its base, almost every case should be submitted to amputation, as the transport of the patient with his fractured limb would be an occasion of extreme and protracted suffering, and would deprive him almost entirely of any chance of recovery which he might otherwise enjoy. There is no great danger in transporting a patient for many successive days after amputation of his thigh, if the stump be properly supported, and the transport be not too rudely effected. Indeed, patients often do better under these circumstances, than when they are crowded together in large hospitals, where they enjoy entire rest, but where, at the same time, they breathe a more or less infected atmosphere. 2. Amputation should be performed in nearly all cases in which the femoral or popliteal vessels are wounded. When the injury is so high up as to admit of amputation only at the hip-joint, there may be a question as to the expediency of the operation, as primary amputation at the hip-joint is almost inevitably fatal. 3. It should be performed in all cases in which the ball has passed fairly through or into the knee-joint, or on which the bone has been comminuted at its inferior articular extremity. When the joint has been but slightly exposed on one of its lateral surfaces, and the fracture only involves the superficial part of the external or internal condyle, an attempt may be made to save the limb, although, even under these circumstances, the issue of such an attempt is extremely doubtful. 4. Amputation should be performed in all cases of gun-shot fracture of the thigh below the junction of its middle and upper third, when the base is extremely comminuted, over a space of nine or more inches of its length. When a comminuted fracture is very near the upper extremity of the bone, there is reason for more hesitation, as the operation at the hip-joint, or in its immediate vicinity, is followed by a speedily fatal result in so large a proportion of cases. 5. It should be performed in all cases in which the injury is inflicted by a cannon ball, or a large fragment of shell, and in which the soft parts are so disorganized, that they must necessarily lose their vitality. If, however, the shock of the injury occasion extreme depression of the vital powers, from which the patient does not rally, it is better to let him die in peace than to hasten his death by the performance of an operation which he has not strength to endure."

Dr. Post speaks highly of a mode of treatment known as Dr. Buck's method.

"It consists essentially of extension made by a weight attached to a cord, passing over a pulley and secured to a block of wood, the ends being applied to the sides of the leg and thigh, and kept in place by spiral strips of adhesive plaster and a roller. By this means the pressure of the extending force is equalized upon the sides of the limb, and no injurious pressure is made upon the instep. A brick being placed under each foot-post of the bed, the weight of the body generally secures a sufficient amount of counter-extension. I had long been familiar with the excellent results of this method of treatment in simple fractures of the thigh as they occur in civil practice, whether in adults or children. It has been found advantageous in simple fracture, under my observation, in promoting the comfort of the patient during treatment, and in securing union of the fractured bone with the least possible amount of shortening or other deformity. From the experience which I had had in the treatment of simple fractures by this method, I was, in some measure, prepared to appreciate the results of the same method as applied to the treatment of gun-shot fractures. And I was very much gratified with the opportunity which I enjoyed of observing the satisfactory results of this mode of treatment."—*New York Journal of Medicine*, January.

On Consecutive and Indeterminate Hæmorrhage from Large Arteries after Gun-shot Wounds. By DR. HOLLOWAY.—One of the results of four years' active military experience has been to produce the conviction in Dr. Holloway's mind that the present mode of treating bleeding vessels is far from satisfactory. He lays down the propositions, that: 1. The application of the ligature, no matter by what plan, is, in many instances, not only futile but very injurious. 2. The proper employment of astringents, both local and constitutional, together with compression, is oftentimes sufficient to arrest hæmorrhage from arteries of large calibre, until the natural process of repair has set up in and around the bleeding vessel.

With respect to the first proposition, Dr. Holloway adduces three cases as justifying his general impression, that the ligature of arteries for the arrest of hæmorrhage, whether performed by Hunter's or by Guthrie's methods, is often an uncertain, inefficacious, and dangerous procedure. In three other cases, in which, after secondary hæmorrhage from large vessels, ligature seemed unadvisable, the hæmorrhage was controlled under the conjoined employment of compression and styptics; and these are only adduced as specimens, numerous other cases having been so treated by the author's colleagues. Indeed, we suppose it is within the experience of every surgeon that secondary hæmorrhage may sometimes be so arrested; but we question whether the evidence the author has adduced will justify the adoption of the practice he recommends as a general rule. The styptic he gives preference to is the persulphate of iron, used either in powder or saturated solution, "Generally filling a bleeding cavity with alternate layers of lint and the powder, I have yet to see any bad effects, local or constitutional, from its use.—*American Journal of Medical Science*, October.

On the Reduction of Dislocations of the Shoulder, by Schinzinger's Method. By PROFESSOR DUMREICHER.—Professor Dumreicher brought Schinzinger's new mode of reducing this dislocation under the notice of the Vienna Medical Society, having of late had several occasions of proving its efficacy. Its simplicity and the small amount of force required for its execution are its chief recommendations as compared with other methods. An assistant having fixed the shoulder by crossing his hands over it, the operator takes hold of the upper arm and rotates it outwards to such an extent that its inner surface is brought round in front, also pressing the elbow against the trunk as much as possible. A second assistant having placed his forefinger on the inner side of the head of the bone, pressing it somewhat outwards, the operator now presses the humerus against the acetabulum, rotating it slowly inwards, and the head of the bone slips into its cavity with a loud noise. In three cases which had recently occurred in his practice, Professor Dumreicher, the reduction, performed without anæsthetics, was effected by the exertion of very little force and without inducing any pain. Professors Roser and Bardeleben have objected to this method, that the strong rotation outwards might easily, in the case of adhesions existing, give rise to fracture of the humerus. There might certainly be some danger of such an occurrence if this rotation were performed in a very old dislocation, unless the adhesions had first been loosened by traction.

In the discussion which followed, Professor v. Pitha directed attention to Richet's method, which is of easy accomplishment by the exertion of little force, providing the muscles can be kept in a relaxed condition, and the patient's attention so occupied that he does not offer any resistance. The hand is passed into the axilla and an endeavour is made to surround the dislocated head by the fingers, which can be easily done unless the patient offers resistance. Indeed, the whole of the head need not be surrounded, for if the fingers can be planted into its larger circumference and slight traction be made on the head, the reduction may be accomplished. The force employed is so very slight that if the head is seized even by the left hand it may be reduced and neither preparations or assistants are required. It is only necessary that the arm should be kept abducted in an easy position. This method succeeds even in very muscular subjects. As to Schinzinger's method, v. Pitha recommends that it should be confined to recent dislocations; for, employing external rotation in a case of old dislocation, but to a less extent than here recommended, a cracking was produced, not from fracture of the bone, but from rupture of the tendon of the triceps. Dr. Dumreicher quite agreed that this plan must be resorted to only with great prudence for old dislocations. With respect to fractures occurring during reduction of old dislocations, he is of opinion that these are often the consequence of periostitis, to which repeated attempts at reduction have given rise. Such cases have repeatedly occurred at his Klinik. Professor v. Pitha added that Richet's method was especially indicated in cases in which fracture complicated the dislocation.—*Allgem. Wien. Med. Zeit.* Dec. 26.

On the Treatment of Fistulous Discharges due to Caries, &c., by the "Liqueur de Villate." By M. Notta.—In a former communication, in 1863, M. Notta drew attention to the value of the *Liqueur de Villate* in the treatment of certain forms of obstinate fistulous discharges. Transferred from veterinary practice, in which it has been most advantageously used for more than twenty years, this substance has proved of great service, according to the testimony of MM. Notta, Nélaton, and other distinguished French surgeons. In the present paper M. Notta relates several additional cases, in order the better to distinguish the circumstances under which it can be resorted to with advantage. Those in which it has proved pre-eminently useful, have been examples of caries of bone, accompanied by fistulous tracks, which have resisted various other modes of treatment. Still, it must not be indiscriminately employed here as it will succeed only in cases in which the caries is soft and vascular, and of limited extent, being of no avail when this is very extensive or sequestrated. Another class of cases which can be successfully treated are fistulæ consecutive to chronic abscess, often when iodine has been tried in vain. In several cases of tubercular fistula of the testis, M. Nélaton has effected a rapid cure by five or six injections. Even when the abscess has been of an acute character, but has eventually given rise to great detachments and obstinate fistulæ; the *Liqueur* is found of equal value, several examples of which are given by M. Notta. But any attempts that have hitherto been made to extend its employment to the seemingly analogous case of fistula in ano have so decidedly failed that there is no encouragement to repeat them.

The composition of the *Liqueur de Villate* is as follows:—Liquid subacetate of lead 30; sulphate of copper and sulphate of zinc āā 15, and white vinegar 200 parts by weight. It is essential to remember that it is suitable only when the affections reach their chronic stage; and that if employed prior to this period, in place of a salutary modificatory inflammatory action, it may give rise to a phlegmon of a dangerous character. In suitable cases, injection of the fistulous tracks may be employed every second, third, fourth, or fifth day, according to the amount of inflammation produced, the patient then being allowed to repose awhile. In very obstinate cases, the injections may be made, as in veterinary practice, daily, and continued for months, if required. In all cases, however, they must be suspended awhile if too great inflammation be excited; and, in some cases, when the wound is not deep, or filled with fungosities, or when the caries is not easily accessible, some lint, soaked in the *Liqueur*, may be applied instead of using the injections. Generally the injections give rise to pretty severe pain, which may last for several hours, or even all day; but usually, though not always, after some days it becomes easily supportable. The first injections give rise to sharp inflammation in the fistulous tracks; but the suppuration, which is temporarily increased, soon diminishes. Small osseous fragments are often detached after injection; and M. Nélaton has several times observed membranous shreds—analogous to the false membranes in the horse—coming away.—*L'Union Médicale,* Nos. 7, 11, 15, and 17.

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107)—Sayre. Dislocation of the Femur into the Ischiatic Notch. (New York Journ. of Med., Jan. A case of spontaneous dislocation, consecutive upon gunshot injury, reduced by manipulation, after eight months. A discussion at the New York Academy.)—Forget. Question of Reducing Old Dislocations of the Shoulder. (Union Méd., 1855, No. 150. A detailed account given of a fatal case occurring in Lisfranc's practice.)—Hitzig. Tarso-metatarsal Dislocations. (Berlin Klinische Wochen., Nos. 39—42.—Willemin. Case of Dislocation of the Foot forwards. (Union Méd., 1866, Nos. 4 and 5.)—Robert et Collin. New Apparatus for Reducing Dislocation of the Elbow. (Bulletin de l'Académie, Nov. 14. Woodcuts.)

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